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THE
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U. of M.

OF
PRACTICAL MEDICINE AND SURGERY,

BEING

A Half-Yearly Journal,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL
IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY
W. BRAITHWAITE, M.D.,
LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN
AT THE LEEDS SCHOOL OF MEDICINE, ETC.,

AND

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PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ART. 1.—ON DIGITALIS IN ACUTE FEBRILE DISEASES.

By Dr. ANSTIE.

[In this paper Dr. Anstie invites our attention to the effect of this drug in cases of fever.]

The special subject which will now be discussed is suggested to us by the appearance of a pamphlet by Dr. T. W. Grimshaw on the action of digitalis on the weak heart of typhus; a paper which contains some remarkable facts, although I am not able to agree with all its conclusions. The question of the power of digitalis as a heart-tonic in the so-called adynamic fevers generally—not merely in typhus—is becoming very important: and the following remarks will be concerned with this alone, to the exclusion of other questions, such as the power of digitalis to lower temperature in acute disease.

I can hardly preface my own observations more effectively than by bringing forward some of Dr. Grimshaw's facts. When it is remembered how strikingly the pulse of typhus fever exhibits the characters of low arterial tension and low heart-force, it must indeed be astonishing, to those who cherish any remnant of the classical belief in digitalis as a heart-depressor, to find Dr. Grimshaw giving 1 ounce, and even 1½ ounce of the *infusion* of digitalis every three hours for five or six days together, not only with impunity, but with seeming benefit. Whether the benefit was real or not, at least this is certain, that in nearly all the cases there was a total absence of those symptoms of heart-depression which used to be held up as a bugbear of digitalis treatment; indeed, on the contrary, it is asserted by Dr. Grimshaw, with apparent accuracy, that both the heart-force and the arterial tone were markedly invigorated. And in addition to this, it is to be remarked that but small amounts of alcoholic stimulants were given in any case, and in several no alcohol was administered. There were but two deaths out of twelve cases, and in the whole of the ten recoveries the patients were adults.

I do not propose to dwell any further on the cases brought forward by Dr. Grimshaw, which have been cited simply as proof of the fact that in typhus, even of severe type, the phenomena of cardiac and vascular depression are relieved rather than aggravated by the use of digitalis in very large doses. Observe, that these cases, in which the *infusion* of digitalis was employed, stand on a very different footing from experiments in which large doses of the *tincture* had been employed: so long as the evidence consisted only of instances in which the latter preparation had been used it was impossible to say how far the result had been influenced by the large quantities of alcohol necessarily given. In Dr. Grimshaw's cases, on the contrary, we have to deal with an uncomplicated digitalis action.

It would be a mistake, however, to limit our consideration of digitalis as a heart-tonic in febrile diseases to the case of typhus. It must not be forgotten that the latter is an affection that runs a brief course, and in which asthenia is not the mode in which death usually occurs. Some of the best authorities on fevers have remarked that the issue in uncomplicated typhus depends upon the intensity with which the nervous system is affected, far more than on cardiac asthenia of local origin; and certainly the characteristic aspect of such cases is pre-eminently that of overwhelming nervous poisoning. No doubt the muscular tissue of the heart is frequently, if not always, considerably changed, and both Murchison and Buchanan state, from personal observation, that in fatal cases this affection is not a mere fatty softening, but sometimes, at least, involves more or less elaborate changes of the same kind of those which have been so carefully described by Zenker as occurring in the heart and voluntary muscles of typhoid patients. But on the whole it seems evident that local cardiac changes count for much less in a brief disease like typhus than in a prolonged affection like that form of typhoid in which death occurs from simple heart-failure at an advanced period, and in which "Zenkerism" of the cardiac muscular substance is especially pronounced. It is in connection with this latter form of fever that I have more especially studied the question of weak heart in febrile disease; and before speaking of digitalis as a remedy, I wish to make some remarks upon the clinical facts of the case.

I doubt whether any published account of typhoid fever gives a just idea of the frequency with which sudden failure of the heart—and this alone—is the cause of death in cases of typhoid which run a longer course than four weeks. I do not mean that all these deaths are instantaneous; but the following is a fair average description of what happens:—Either from the original intensity of the febrile attack, or from the existence of severe complications, the heart has been kept pulsating at a rate varying from 120 to 150 or more, under an internal temperature which was always very high, even at the daily minimum, for some four or five weeks. The inflammatory complications, or the intestinal ulceration, however, have fairly subsided, and the pulse now begins to change. But instead of the simultaneous diminution of frequency and increase of arterial tone which would show the passage to convalescence, we remark (if we observe with attention) a very different kind of pulse-change. Careless feeling of the radial pulse might lead one to believe that the pulse, though somewhat irregular, had diminished very greatly in frequency; but a thoroughly skilled finger, or still more surely a sphygmograph, would immediatly bring out the fact that many small intermediate pulsations had been overlooked; in fact, that the pulse is scarcely, if at all, less rapid than before, but now presents the dangerous quality of varying in force from moment to moment, *and that in an unrhythmical manner*. Nothing can be of worse augury than this—more especially when the sphygmograph shows the combination of rounded apices of the individual pulse-waves with unrhythmical undulation of the general line of the tracing. It is under such circumstances that patients often "sink out" after all active mischief would seem to have ceased. It is very noticeable that such a catastrophe is far more common in adults than in children: the hearts of the latter possess a prodigious power of recuperation, which seems to be reduced to a minimum by the time that the middle age—so dangerous to typhoid patients—is reached. At the latter epoch, also, not merely is heart-failure a too likely occurrence in the later weeks, but this form of death may overtake the patient at an earlier date; and there seems reason to believe that in such persons the degenerative process goes on with far less interruption from recuperative efforts than in the young.

It is now well known that a very marked rise in the mortality from typhoid is found in the quinquennium immediately succeeding the development of puberty: and from personal observations I believe that, even so early in life as this there is a change in the nutrition of the cardiac muscular structure which renders it more incapable of resisting the influence of high temperature and very rapid pulsation for prolonged periods, and which consequently allows, more frequently, the occurrence of death from heart-failure. But the tendency is, doubtless, greatly increased later in life.

I think we are entitled to view the occurrence of muscular heart-failure in acute febrile disease as essentially dependent upon an enforced rapid action *under high temperature*, prolonged for a period which is excessive, *in proportion to the vital recuperative power of the cardiac muscular tissue*. In this point of view we should logically place on a line with the effects of the mere prolongation of high temperature and rapid pulsation, in protracted typhoid, the effects of a shorter exposure, to these conditions, of a heart whose tissues had been previously so modified by pathological degeneration as to render their restoration to a healthy state unusually difficult. This principle is very important. We should expect heart-failure to be a very formidable danger in delirium tremens, with high temperature and rapid pulse, if the patient were an old drinker; and, similarly, we should expect that acute diseases, like pneumonia or erysipelas, occurring in persons who had long been addicted to alcoholic excess, would be very liable to terminate fatally in the same way. Nothing can be more certain than that this liability exists; it is the terror of every experienced physician who encounters these acute maladies in old topers. Such persons cannot endure for more than a very limited period the presence of pyrexia and rapid pulsation; their hearts give way under the combined pressure.

In applying considerations of this kind to the question of digitalis treatment for heart-weakness in febrile disease, it is at least evident that there are very good *primâ facie* grounds for expecting valuable results in a considerable proportion of cases. By a strange perversity, which is the more surprising in view of the greatly improved pathological knowledge of the present day, too many teachers of medicine ignore the influence (which cannot be too highly estimated) of the previous condition of the bodily organs of any given patient upon the cause of acute febrile diseases by which he may chance to be attacked. Such an error is to be strenuously avoided by any practitioner who desires to attain a mastery of treatment: and certainly, in no instance should it be more carefully shunned than in the use of digitalis in acute febrile disease.

What is it, in fact, that we expect to gain from digitalis in acute febrile disease? Putting aside the question of reduction of temperature, which we are not now considering, we may endeavour to operate on the heart itself. It matters not whether we adopt the theory of vagus stimulation or that of direct tonic action on the muscular fibre *plus* a stimulation of the vasomotor system producing tonic contraction of the arterioles; the practical effect at which we aim (and are justified in so aiming by a number of experimental facts) is twofold—the simultaneous slowing and strengthening of the ventricular contractions. It may be asked whether this in itself amounts to any real advantage, unless the course of the disease were also cut short. I believe that, in regard to the danger of heart-failure it may be of great advantage. The normal tendency of muscle which is persistently overworked in proportion to its nutrition is undoubtedly towards degeneration; and, certainly, there can be no more decided instance of such overwork than that of the cardiac muscles when they are forced to contract some 130 or 140 times per minute, for a long time together, under a temperature of 103° to 105° or 106° Fahr. Merely to reduce the frequency of these contractions must surely be an important object, for the sake of preserving the soundness of the muscular tissue.

A question of much interest arises whether the impunity with which digitalis has been prescribed in such large doses by Dr. Grimshaw is due to the fact that the infusion of digitalis is a weaker preparation than British physicians have been accustomed to suppose. Upon this point there is a curious conflict of evidence, which has never yet been cleared up. The researches of modern chemists (especially those of Wiggers and of Nativelle) have made it clear that digitalis contains two active ingredients—digitaline and digitaleine,—of which the former is said to be decidedly the most powerful of the two; but of these only the latter is soluble in water to any considerable extent, while the more powerful principle requires alcohol for its extraction. On the other hand, there is abundant recorded evidence of very powerful poisonous effects being produced by aqueous infusions and decoctions of digitalis. In making comparisons between the potency of the infusion and that of those preparations of digitalis which contain the more powerful alkaloid, it is necessary, as already said, to leave the tincture out of the question, since the alcohol in that preparation confuses any experiments which are made, and renders their results altogether equivocal. It remains, then, to inquire whether the powder of the leaves, the alcoholic extract, and the crystalline digitaline itself, can be given in respectively large doses in the condition of a weak heart in febrile diseases. Now, it is remarkable that the so-called "digitaline" of Homolle and Quevenne (a mixture, in fact, of digitaline with a certain amount of digitaleine and digitalose), the dose of which, as recommended by the inventors, is from $\frac{1}{17}$ to $\frac{1}{11}$ grain, has been given by Trousseau, in uterine hemorrhage, in as large a quantity as 1 gramme (15·6 grains) in the twenty-four hours; and by many other physicians, in pneumonia, typhoid, &c., to the amount of nine or ten grains daily. And the powdered leaves of digitalis have equally been given, especially by the German physicians, in doses which sound perfectly frightful as compared with the doses laid down by authorities in this country. On the whole, then, it appears at least very probable that there are some circumstances which confer on the organism a large degree of tolerance for digitalis; and, looking to the remarkable experience of Trousseau above mentioned, we get more than a suspicion of the grounds of this tolerance. Trousseau gave as much as 15·6 grains of digitaline in twenty-four hours to a woman suffering from uterine hemorrhage; and we can hardly doubt that in a healthy person a much smaller dose than this would have caused fatal tetanus of the heart. Good digitaline of Homolle and Quevenne is probably quite fifty times (not 100 times, as the inventors say) as strong as fresh digitalis leaves; and, not to mention the instances—due, probably to idiosyncrasy—in which as small a quantity as one or two milligrammes (·0156 grain, or ·0312 grain) has produced uncomfortable symptoms, it has very frequently happened that doses twice or three times as large as this have caused nausea and great depression. The true explanation of the tolerance of such enormously greater doses in cases of uterine hemorrhage must be sought in the modern physiological experiments which have shown that the real action of digitalis on the heart is that of a stimulator, instead of a paralyser, of the cardiac muscular substance; in accordance with which fact one may well suppose that the extremely weakened cardiac muscular fibre of a patient, exhausted by profuse hemorrhage would sustain and even require a dose of digitalis, merely to support a sufficient amount of contraction to continue life, which in health would fatally tetanise the heart.

The general effect of the various considerations which have now been advanced seems to be this:—that the action of digitalis (*a*) as a sustainer of exhausted heart-power, and (*b*) as a restrainer of the rapidity of pulsation, seems to especially point it out as a promising remedy in that very large class of acute febrile diseases in which the greatest danger is failure of the heart before the malady has had time to run through its natural periods of development; and that there are strong grounds for holding this opinion, whether we admit or deny the reduction of temperature which is so strongly maintained by many eminent authorities. On the latter point I cannot but think that judgment should be as yet suspended; not that I deny the fact that febrile temperature can be reduced by large doses of digitalis, but that a careful review of the literature of the subject leaves me in doubt whether the lowering of temperature is not, in an inconveniently large number of cases, attended with an amount of nausea and depression which more than counterbalances the advantage of the mere cooling. In my own experience, which may have been unfortunate, it has been difficult to frame any rules of dosage and circumstances under which one might with confidence expect a beneficial reduction of febrile temperature. But I can answer for the fact that, even without this action, digitalis has, to all appearances, proved of the utmost value in many cases of acute disease running a protracted course or occurring in subjects whose tissues were damaged by disease or intemperance. And the more I reflect upon the matter, the more I am inclined to trace these benefits to the sustaining action of digitalis upon the enfeebled heart.—*Practitioner*, *Sept.* 1873, *p.* 179.

2.—ON EPIDEMIC TYPHOID RESULTING FROM AN INTERMITTENT WATER-SUPPLY.

By the EDITOR OF THE LANCET.

It will be remembered that in May last Dr. Blaxall, one of Mr. Simon's staff, was sent to prosecute a sanitary inquiry at Sherborne, in consequence of the prevalence of both endemic and epidemic typhoid. His report, now published, on enteric fever in the town of Sherborne, is unusually interesting, as it shows how clearly the chief epidemics of enteric fever may be traced to contamination of the drinking water by sewer air, and fully bears out the views recently ventilated by Dr. Carpenter as to the dangers of an intermitting water-supply. Dr. Blaxall bases his opinion, that the epidemic this spring was caused by persons drinking of water contaminated with sewer air, on the following considerations:—First, the sudden appearance of seventy-three fresh cases in the first week of March (the average weekly number for the three previous months having been two and a quarter)—viz., after the Board of Health water had been shut off every night during February; second, the gradual diminution of cases after the first week in March, during which month the water was not turned off at nights; and third, the manner in which the cases were scattered about the town; but, mainly, the far greater proportion of cases occurring amongst persons supplied with the Board of Health water than amongst those who derived their water from other sources. The danger of an intermitting water-supply here exemplified is so important, and yet so little appreciated, that we consider the following details, taken from the above-mentioned report, will well repay a careful perusal.

Sherborne, according to the last census, had a population of 6041. In 1867 it was visited by an epidemic of enteric fever, which caused six deaths; since then enteric fever has continued endemic, doubtless owing to the defective means for the removal of excreta, and to the water having been contaminated from time to time by sewage or sewer air. The accompanying table shows the progress of the recent epidemic, which caused 13 deaths in the first four months of this year, and aids much in elucidating its cause.

Number of New Cases of Enteric Fever.

In the month of	December, 1872..........	2
"	January, 1873...........	12
"	February, 1873....... ...	15
In the week ending	March 8th...........	73
"	March 15th..........	45
"	March 22nd	28
"	March 29th..........	17
"	April 5th	17
"	April 12th...........	23
"	April 19th	2

It will be observed that rather numerous cases occurred in January and February; that in the first week of March there was a sudden and great increase, and during the remainder of the month a gradual diminution.

Dr. Blaxall made a careful inquiry into all the conditions likely to affect the health of the community, particularly with respect to the supply of articles of general consumption. The influence of milk in the distribution could be excluded, but respecting the circumstances of the water-supply the following facts were ascertained. During December and January last the Board of Health water was frequently shut off at a point slightly above the houses on the highest level of the town, and the same thing was done during every night in February. It is known that when the water was thus shut off there were certain delivery-pipes up which a rush of air occurred immediately the tap was unscrewed. Now in many instances the pipes open with taps into watercloset pans, there being a frequent absence of service-cisterns. At night, after the closet had been used, the tap would be turned on for the admission of water; none flowing, the tap would frequently not be turned off again, thus a direct passage into the water mains would be left patent. Even this accidental leaving of the tap open was not necessary, as many taps, being broken, permitted a continuous flow of water during the day, but at night, no water being present, were uninterruptedly free to admit sewer air. Through these openings, when water did not flow, air commonly passed from the closet-pan to the water-pipe, the system of pipes of the water-*supply* becoming, in fact, the ventilating channels *for the closet pans; and, if a pan happened* to be full, its contents might even *be sucked into the water-pipe.* In January and February, when at least twenty-seven cases of enteric fever were in the town, closet-pans thus circumstanced were doubtless in some instances tainted with the specific contagium; some of them may even have been filled with the excreta of fever patients. The sewer air which entered the pipes at night, when the water was turned off, would be driven forward in the morning, when the water, turned on again after washing the pipe surfaces upon which foul air had been resting for hours, could hardly (even supposing that no actual excrement to have got into the pipes) have failed to contain fever contagium; and this water was destined to be drunk by the people.

The part played by the Board of Health water in the recent epidemic is tolerably evident from the adjoining tabular statement:—

Houses in Sherborne, 1010.....	780 supplied with Board of Health water; 148, or 19 per cent., visited by fever. 230 supplied with other water; 13, or 5·6 per cent., visited by fever.
Persons attacked between Dec. 1 and May 10.....	In the 780 houses, 226; or 29 cases per 100 houses. In the 230 houses, 17; or 7·4 ditto.
Peasons attacked in the first fortnight of March..	In the 780 houses, 111; or 14·2 cases per 100 houses. In the 230 houses, 7; or 3 ditto.

Though the origin of the Sherborne epidemic, which is so evident from the above history, must be far from an uncommon element in the production of urban typhoid, yet we believe Dr. Blaxall has been the first to demonstrate it in practice; and, though almost despairing, we are encouraged to hope that this danger from an intermitting water-supply may startle the metropolitan authorities into providing that flood which Sir Wm. Fergusson calculates would threaten to run the Thames dry.

Whether or not our ideal sanitary reform—a constant water-supply for London—will result from this fresh knowledge of the danger its neglect entails, we must ever be grateful to those who have added to our sanitary science by their careful deductions from practice and theory.—*Lancet*, *Aug.* 30, 1873, *p.* 304.

3.—ON SEWAGE AND DISEASE.

By the EDITOR OF THE MEDICAL TIMES AND GAZETTE.

The discovery of the cause of the outbreak of enteric fever in Marylebone has naturally led to much public discussion as to the mode in which the poisonous material has been conveyed into the milk. In our article on the propagation of zymotic disease by milk, last week, we discussed two methods—one a tolerably certain one, the other a probably possible one—in which the contagium of disease may be conveyed into milk. The certain mode is by polluted water, the more doubtful by polluted air. But the outbreak has led to the publication of speculations as to the mode of infection of milk, to which the actual facts of the case give not the slightest confirmation, but which, nevertheless, in the very imperfect state of our knowledge of the precise methods in which diseases are propagated, may be allowed to demand examination.

On Saturday last a well-known member of our profession—Mr. Alfred Smee, F.R.S.—published a letter in the *Times* newspaper, in the first sentence of which he announced that he is "in a position to explain the occurrence of typhoid fever from the use of milk." The subsequent part of his communication, however, fails to corroborate the opening sentence; for the only fact which Mr. Smee adduces as capable of throwing light upon the occurrence is that *some cows of his own*, fed on two occasions with sewage grass, produced a *milk which, after keeping for* twenty-four hours, had a slightly rancid odour, *and a butter of an offensive* character, which turned rancid a day or two after it was made. Mr. Smee, however, does not add that any one who partook of this milk or this butter was made ill in the slightest degree by it. Now, in the first place, we are informed on the best authority that the pastures of the farm from which the infected milk was exported were not irrigated by sewage, nor in any other mode was sewage manure specially employed. We cannot, therefore, but think it a great pity, so far as this particular outbreak of fever is concerned, that a theory should have been started which might have had the effect of diverting the attention of the scientific public from the real source of the mischief. The account which Professor Corfield, the expert employed by the milk company, has published in a medical contemporary, proves that there were ample means and opportunity for contamination of the water used in the dairy operations at the farm from the privy in which the excreta from one, if not two, patients suffering from enteric fever were received; but Dr. Corfield does not say one word as to sewage irrigation or the employment of putrid manure in the pastures. On one occasion a quantity of blood and putrid matter, discharged from the bowels of a farmer, who died in June, was buried by order of the doctor in a field immediately outside the farm premises. But burying in one isolated spot is quite a different thing from manuring by sewage, and there is no evidence that the cows were fed in the field in question. On the other hand, there is a direct communication by soakage from the privy to the well, and the water of this well, which supplied the pump used in the dairy, is so bad that it has long been condemned by medical men as unfit for drinking purposes, and was not so used by the residents on the farm. Besides the soakage from the privy, the well is so situated as to drain the whole premises, including soakage from a ditch which received fluids from the pigstyes, the dairy, and a manure heap on which the house slops were thrown. Anything worse than the arrangements, from a sanitary point of view, could scarcely be conceived. The epidemic itself is an instance of widespread typhoid poisoning, in which the poison was collected in a well in Oxfordshire, and thence carefully conveyed and distributed in London. The middle ages believed that epidemics were produced by poisoning wells; it has been reserved for the nineteenth century to prove the fact.

We are justified, therefore, in saying that there is nothing in the facts alleged by Mr. Smee which would support the hypothesis that milk produced on land manured by sewage is capable of giving specific enteric fever, so neither does the Marylebone outbreak lend a shadow of confirmation to that hypothesis. But, separating the hypothesis from this special outbreak, it is of vital importance that the inquiry be made whether there is any evidence that sewage used as manure is capable of so affecting the produce of the land—either animal or vegetable—as to render it unfit for human food, and the means either of propagating the germs of specific disease derived from the sewage or of producing other toxic effects. *In limine*, it must be acknowledged that disease has from time to time appeared to arise in the neighbourhood of land irrigated with sewage as a consequence of the pollution of the air or water. In the Medical Times and Gazette of April 17th, 1869, is a notice of an outbreak of dysentery in the Cumberland and Westmoreland Lunatic Asylum, which seems to have been due to emanations from some sewage-irrigated land in close proximity to the Asylum. In the number for May 24th of the present year, we noticed an outbreak of enteric fever at Ecton, near Northampton, which was investigated by Dr. Buchanan, and which appears to have originated amongst some labourers who were at work on a farm irrigated by the sewage of Northampton, and who drank some water from a brook which received the water that had drained through the soil of the farm, as well as any of the sewage that had been discharged from the farm without passing through the soil. These occurences, and others of a similar bearing, may be held to prove that sewage itself can produce toxic effects, and that sewage irrigation, unless proper care be taken, not only in its application, filtering, and deodorisation, but also in apportioning precisely the quantity used to the area over which it is spread, is not a proceeding entirely devoid of danger. But it does not help us to an answer whether the toxic qualities of sewage are capable of reappearing and producing their effects in *animal and vegetable produce.*

With regard to vegetable produce, we have, on the one hand, Mr. Smee's assertion that putrid matters can affect vegetables, but whether by communicating to them toxic qualities he does not say. On the other hand, there is an enormous mass of evidence that the vegetable produce of much sewage-irrigated land is eaten both by man and animal without any ill effects accruing. In the *Times* of Tuesday, for instance, is a letter from Dr. Williams, of the Sussex Lunatic Asylum, at Hayward's Heath, who states that thirty cows and twelve horses are fed with the ryegrass grown on land irrigated with the sewage of the Asylum, and that the inmates of the Asylum get their dairy produce from the cows, and that no bad effects have ever resulted. But, besides ryegrass, beetroot, oats, and other vegetables, have been raised on sewage farms, and made the food of man and animals with impunity. We have no evidence that the poison of enteric fever can produce that fever either in carnivorous or herbivorous animals, and all proof is wanting that any poisonous effect other than enteric fever has been produced in animals by sewage vegetable produce. With regard to man, evidence is also lacking. Of course, if sewage has been used in a proportion too great for the soil to absorb and decompose, it may remain on the surface and adhere to the vegetables grown. It is conceivable that a danger might arise were such vegetable matter made human food. But we believe there is no evidence of mischief having arisen from such contamination.

The other question of possible contamination through animal produce, and especially through milk, is that of greatest interest at the present juncture. Positive evidence of the toxic contamination of milk and other animal produce by sewage we believe to be entirely wanting. It was supposed that sewage pastures would spread far and wide entozoa amongst our population. But, although theoretically this would appear probable, we know of no evidence that diseases of this kind have increased since cattle have been fed on sewage grass. With regard to dairy produce, one outbreak of typhoid in the neighbourhood of Croydon was attributed to milk from a sewage farm, but tadpoles were found in the milk, which settled the question as to the real source of contamination. On the other hand, the observations of Dr. Alfred Carpenter, of Croyden, Professor Corfield, and a number of other scientific witnesses, are unanimons as to the excellent character both of milk and butter produced from sewage grass, and we may confidently assert that the greatly preponderating weight of evidence is against the occurrence of any contamination. But it may be dangerous to dogmatise as to its possibility. No secretion of the body is more delicately sensitive to diet than the mammary. To say nothing of the epidemics of obscure disease which in the United States are referred to milk, it is well known that milk may be rendered poisonous by cows or goats being allowed to feed upon poisonous umbelliferous and other toxic plants. It is also certain that some organic poisons may be passed through the systems of several animals without losing toxic power. Thus the blood of an animal killed with the poison of the cobra is itself poisonous, and will rapidly kill if it be injected into the veins of another. But beyond such scattered facts, which may be supposed to be analogous, there is no evidence that the germs of a specific disease, such as typhoid, can be taken into the cow's system through the channel of sewage grass, be thence excreted by the mammary glands, and, producing no toxic effect on the cow, can spread enteric fever amongst the children who drink her milk. Such a sequence of events is most likely impossible; but, if possible, there has been no outbreak of fever or other disease in this country which would warrant us in believing that it has taken place.—*Med. Times and Gazette, Aug.* 23, 1873, *p.* 204.

4.—ON A PURE WATER SUPPLY.

By the Editor of the Medical Press and Circular.

[There can be no question as to the convenience of using soft water, but there *are grave doubts as to the* suitability of soft water for the supply of large towns.]

On reference to a report of Dr. Letheby's, we find he gives the mean mortality per 1000 in an average of five years of twenty English towns, and with a water-supply of ten degrees of hardness, and in another group of thirteen with a water-supply under ten degrees. In the first division we find placed nine large towns—Portsmouth, Wolverhampton, Nottingham, Hull, Southampton, Reading, Ely, Chelmsford, and Gloucester, and each, it appears, have an average death-rate of 21·9 per 1000. In the second division are the names of the towns we will not specify, but the average death-rate reaches the unenviable position of over 28· per 1000, so that here we once more meet with an undeniable proof of the superior salubrity of those towns supplied with hard water as compared with those obliged to put up with a soft-water supply. And, furthermore, as to the health of the inhabitants of such towns, we find it stated, upon good authority, that in the towns last referred to, diarrhœa prevails as a chronic complaint. In other countries also this is the case; take, for instance, the Netherlands, or the City of Boston, United States. The latter town derives its water-supply from Lake Cochituate, and the health of the inhabitants is in a most unsatisfactory condition at the present moment. Dr. Hayes, in a paper published in Selliman's American Journal of Science tells us that the water of this languid lake contains "numerous animalcules, fresh-water sponges, crustaceans, and an abundance of ochry-matter." "It is only consistent to find," says the same authority, "an affinity to putrid water, and there are periods of the year during which it closely approaches to this character." In October last "the water-supply had become most offensive, and the water-fleas had attained to an unusual size and quantity, so much so that the cotton filters were closed by their dead bodies." After such a statement as this, it will surprise no one to find by the fourth "Annual Report of the State Board of Massachusetts" that Boston is most unsanitary, and that typhoid fever, diarrhœa, and dysentery, are sweeping off thousands; that there is filthiness within and squalor and disease without; and that the waters from the foul streams are mingled with human excrement, nevertheless they are habitually used for drinking, and washing the clothes of the people who live in more aristocratic quarters.

Few who have followed the water inquiry with any attention will fail to see that collateral evidence and argument in every way demonstrate the superiority of hard against soft water, and will understand why the Commissioners of Paris and Vienna decided in favour of hard water for the inhabitants of these cities. Of the waters selected for Paris we may point to those of St. Philbert and Armentières, brought from a distance of about eighty miles, and each with an average hardness of 12·46 per gallon; while for Vienna, the famed Kaiserbrunnen and Stixenstein show an average hardness of 10·82 per gallon. It is scarcely credible, with such facts staring us in the face, that any man, or any body of men, should be found ready to bring surface water into our towns and pronounce it fit for every domestic purpose. Nevertheless, this was attempted by the Town Council of Edinburgh, and it was only after days of anxious and expensive work that the Bill for the St. Mary's Loch scheme was rejected. The experts, among whom we find the names of Professors Sir Robert Christison, Voelcker, Dr. Letheby, Mr. Jabez Hogg, and Mr. Hawkesley, proved to the satisfaction of the Lords that the water of this lake is largely contaminated with organic matters, and totally unfit for domestic purposes. Dr. Letheby, and the chemists with him, supplied the chemical analysis of the St. Mary's Loch waters, and with the following results. It must be first explained that at least 7,000 acres of peat land drain into St. Mary's Loch, and there is no means adopted for separating the pure water, which arises from certain springs, from that made impure further down, and therefore the dwellers in the cottages standing near the edge of the Loch suffer very much from diarrhœa and fever. Dr. Letheby demonstrated that the water taken from St. Mary's Loch acted injuriously even upon lead, and when left in contact with it for a time it became quite milky in colour. Upon adding a few drops of sulphuretted hydrogen to a sample standing on the table before the Lords, it immediately turned brown, which at once proved that the water contained a most dangerous quantity of lead contamination. In Glasgow at one time there was a good deal of lead colic and lead poisoning from impregnation.

Sir Robert Christison says of the St. Mary's Loch water, that there was always danger in using it, and this remark applies to all the water of the Scotch Lochs, as such waters "have a tendency to cause affections of the bowels of a virulent character, stomach fever, &c." He, in support of his views, cites the authority of a distinguished French physician, who was with the Poles in their patriotic war of 1831, and who furnished him with a number of remarkable instances of illness throughout the campaign. In his opinion, "the only purpose for which such water could be applied was for cleaning the streets."

Mr. Hogg's microscopical examination of the water of this lake quite confirms the view entertained by the eminent authorities already quoted. He says, "it is almost impossible to believe that the water of St. Mary's Loch can be converted into a wholesome potable water. It certainly could only be made fit for domestic uses by costly chemical and filtering processes. The impurities in it, and which result from the large amount of organic matter held both in suspension and solution, are common to most surface waters; during the warm weather the decomposition of the various vegetable and animal matters must always be largely productive of diarrhœa, typhoid, or gastric fevers."—*Medical Press and Circular, Aug.* 27, 1873. *p.* 193.

5.—ON THE DANGERS OF WELL WATER.

The dangers of bad milk are engrossing so much attention just now that there is reason to fear lest the far greater dangers of bad water should, for the time, be overlooked. We trust this serious error will not be committed. For one sample of dangerous milk a thousand of dangerous water could be obtained in almost any part of the country. Let it never be forgotten that very few rivers or wells are safe sources of water-supply, and that many are as unsafe as loaded fire-arms. The shallow wells of villages are among the worst pests of the country, and it is high time that a zealous and well-organised crusade should be brought to bear upon them. It is sickening in most country places to observe the uniformity with which the cesspool and well are made to stand side by side, as though each was necessary for the other; and to think of the twenty feet or so of foul sewage-reeking soil through which the water percolates to its fetid bed. The question should engage the earnest attention of every health officer, and will in too many cases tax his energies severely, for it is one of the hardest of sanitary problems. It is always possible to provide a city or town with good water, but in a village, where houses are few, money scarce, and intelligence scarcer, it is often a matter of exceeding difficulty.—*Lancet, Aug.* 30, 1873, *p.* 312.

6.—ON POISONING BY LEAD IN DRINKING WATER.

By Dr. J. W. Von Tunzelmann, Wimbledon.

[It becomes a very serious matter for the public when water passing even only through leaden pipes can become impregnated with lead. We have had a similar case in our own practice.]

Case 1.—I was requested, on June 11th this year, to see the cook, in one of the best houses in the outskirts of Wimbledon, and found that she had vomited every thing that she had taken (according to report) for about three weeks. She vomited also a green bilious-looking watery fluid frequently, but mostly at night. There was a yellowish tint of the skin of the face, and the conjunctivæ were decidedly yellow. There was also constipation, but there was no pain either in the right hypochondrium or at the epigastrium. The region of the liver was not tender to pressure or even to strong percussion, and the region of hepatic dulness was not increased. The urine high-coloured, but not saffron-coloured. The case seemed a puzzling one. I was told that she had had a similar attack four years previously, which was then attributed to the smell of fresh paint in the house. In a few days the vomiting was considerably relieved, and she was able to retain some cold beef-tea, but only in very small quantities at a time. This went on till July 14th, when she was so much better—the bowels also acting regularly—that I expected soon to leave her quite well, and found her still progressing on the 21st. But on the 23rd the vomiting returned, and on the following day she was worse than ever. The weather meanwhile had set in very hot, and I could only attribute this relapse to that; but still there was no pain in the region of the liver or at the epigastrium, and none in the abdomen except when she retched much, in its lower part, evidently of a myalgic nature, from fatigue of the abdominal muscles. The vomiting continued for some days, and she became so weak that I was obliged to give her champagne, which she relished greatly, taking about half a bottle a day without vomiting it. This she continued about a week, and then began to take beef-tea in small quantities; and, subsequently, the weather becoming cooler, she improved rapidly, and up till August 6th there had been no vomiting. On the 9th also she was still free from it, but complained of a loss of power in her hands, so that she could scarcely do anything with them. All this time the patient had not been confined to bed, though obliged to lie on the bed the greater part of the day on account of excessive debility. As this form of paralytic weakness of the arms is a well-known effect of lead among painters, and as this illness had been very anomalous throughout, I examined her gums and found the "blue line" most distinct along the alveolar border of the lower jaw. I therefore took home some of the drinking water furnished by a well in the house, and, testing it with a solution of sulphuretted hydrogen, found that the water became of a distinctly brownish tint, showing the presence of lead. I also took a specimen to Dr. Frankland, of the Royal College of Chemistry, who tested it, and stated in his report to me about it that it contained "so much lead as to render it quite unfit for domestic use;" but he also told me that he had never had to analyse well water for lead before.

The house is a semi-detached one, and there is a common well midway in front of the two houses, from which a leaden pipe leads to the kitchens, a distance of about twelve yards on each side.

Case 2.—In the adjoining house, the housekeeper had in the winter (in February) anasarca of both legs, which continued for about three months. There was no albumen in the urine. The anasarca was relieved by treatment, and she continued free from it from the end of May till the beginning of July, when it returned, and was again relieved in about a fortnight; this also, I have no doubt, was caused by the lead. She used to complain frequently of excessive nausea. She has not the "blue line" on the alveolar border of the inferior maxilla, but she has a leaden hue of skin, and the buccal mucous membrane also has a leaden hue.

August 18th. I was sent for to-day in great haste to this patient because she was suddenly seized with a loss of power in her hands while occupied with household duties (she awoke last night with a numb feeling in them which frightened her). The mistress feared a paralytic stroke; but I was able to relieve the mind of the lady, by telling her that the state of the servant was owing to the lead with which her system had become saturated, and that I did not expect that it would get worse now, as the water from the pump had not been used for four days.

19th. Better, but complains of a numbness in her legs as well as in her arms.

After this she quickly improved, and now (September 18th) only complains of neuralgia in her legs.

In the adjoining house (No. 1), where I had the case of "lead jaundice," I have been since told that a young gentleman, a relative of the family, was seriously ill last summer with was described as "most severe colic, almost amounting to inflammation of the bowels"—evidently lead colic, though not recognised as such by the medical man in attendance on him (from London).

The lady told me that at Christmas, having for some time had great difficulty in getting the pump to act, they had it examined, and found that the pipe (a leaden one) was completely "corroded away" in some places. It was replaced by a new leaden one. The owner of the houses is now going to have the leaden pipes removed, and replaced by patent pipes of lead lined with tin, on the recommendation of an architect of eminence.

Case 3.—I have at present also under my care a young lady who has had diplopia to a most alarming extent for about three months, owing apparently to a sluggish action of the muscular apparatus of the eye (it is very apparent in the movements of the iris), which at first puzzled me completely, the health otherwise being perfectly good. I now find that this case also is owing to the water of the well being strongly impregnated with lead. She suffers from excessive languor. The house is about half a mile from the other two, but on the same hill, and no doubt the water is of the same quality. When the symptom first showed itself, after having existed for about three weeks, she was sent to Hastings for a while (about three weeks) and improved while there. When she returned home the improvement ceased to progress. She then went away again (to Chiselhurst) for three weeks, and improved so much as to be able to drive a pony phaeton, which she could not do even while at Hastings, on account of the double vision. After being away for three weeks she returned, and again the improvement ceased to progress. She is now going to the Lakes for six weeks, which will, I have no doubt, remove the remaining trouble, while the well will be made fit for use by having the leaden pipe, by which the water is drawn from it, replaced by a pipe of galvanised iron.

Case 4.—The mother of this young lady consulted me on August 15th for pain in the back (lumbar region), with very abundant pale urine. I had not then tested the water of this well, but suspected it to contain lead. After I had tested it, and found the lead, I told her of the other cases which I was attending, and she then informed me that she had for some time had an unpleasant numb feeling in her hands and arms, evidently the commencing lead palsy.

These cases remind one strongly of the celebrated Claremont poisoning cases which happened in the same county, being almost within sight of this place.

August 30th.—Dr. Frankland has determined the quantity of lead contained in the water of the well which supplied the semi-detached houses in which cases 1 and 2 occurred. He finds it is 1·547 grains per imperial gallon —*i.e.*, 55 per cent. more than the water which caused the alarming symptoms among the late ex-royal family of France, at Claremont, about twenty-five years ago.

The general quality of the water is shown by an analysis, with which I have been favoured, of the water of the well of a house situated about ninety yards from the house in which cases 3 and 4 occurred, made by Dr. Medlock at the end of last year. Two specimens were examined. One was dipped directly out of the well; the other drawn through the pump; but the water had been allowed to lie all through the night in the pipes before it was drawn off. "The specimen taken direct from the well contains about twenty-five grains of mineral ingredient per gallon, consisting of chloride of sodium and sulphate of lime; there are only traces of carbonates; there are present traces of ammonia, nitrous and nitric acids, which cause it to act very strongly on lead. *Per se* the water is in every respect fit for domestic use, but the sample which had been left in the leaden pipe all night contains a dangerous quantity of lead, and ought not to be used either for drinking or cooking."

Dr. Medlock advised the substitution of block tin pipes for lead. The owner of this house was led to suspect the prescence of lead in the water of his well by perceiving an unpleasant effect in his throat after drinking the water, together with having heard that in a house close by the leaden pipe of the well had become corroded. This is the fourth well to my knowledge, in a small area, where the same deleterious influence has been at work. The houses in which my cases occurred have been built about fifteen years. The soil of the hill on which they stand is a light gravel of considerable depth resting on clay (the "London Clay" of geologists); it contains, no doubt, lime in very small quantity only.

The importance of these cases consists in the fact that the character of the water of surface-wells is now known to depend entirely on the nature of the soil through which the rainfall percolates; and as there are other districts, especially in the neighbourhood of London (all the different commons and heaths for instance), where the geological formation is essentially the same, the same deleterious influence may be at work in houses supplied by wells in them, though in many cases no doubt unperceived, on account of the quantity of lead held in solution by the water not being so great as in these cases, and, consequenly, not having produced such grave disturbance of the health in any of their inmates.

I should recommend that wherever obscure cases of gastric disturbance or of paralytic or dropsical symptoms occur, the drinking-water should forthwith be tested for the presence of lead.—*Med. Times and Gazette, Sept.* 27, 1873, *p.* 352.

7.—REPORT OF THE ANALYTICAL SANITARY COMMISSION ON DISINFECTANTS.

By one of the Editors of the Lancet.

The possibility of another cholera epidemic renders it our duty once more to invite our readers' attention to the subject of disinfection. In doing so we are not anxious to state new views, or to discuss the subject fully from the point of view of pure science. We merely aim at supplying practical hints for practical purposes, and therefore do not hesitate to place before our readers much that is already familiar to them.

In pursuance of this aim we have collected and examined some of the more important of the disinfecting agents now in use, and propose to point out as briefly as possible the advantages and disadvantages peculiar to each. Before doing so, however, it is necessary to advert to the present state of our knowledge in regard to the action of disinfectants generally, and to the conditions under which they are required. The state of our ignorance would, unfortunately, have been the more accurate phrase, in spite of the great amount of really good work that has been done in the last few years.

The term "disinfectant" is somewhat loosely applied to substances possessing respectively three distinct properties. These substances may be more accurately classified as follows:—

1. *Deodorisers.*—Substances which remove noxious smells without necessarily destroying the true noxious matters which accompany those smells.
2. *Disinfectants proper.*—Substances which destroy the noxious matters arising from fermentation, putrefaction, or the infection of disease, by some direct chemical process which converts them into simple and innoxious bodies. Chlorine and oxygen are typical agents of this class.
3. *Antiseptics*, or *colytics* (as Dr. Angus Smith prefers to call them).—Substances which destroy low forms of organic life, and which therefore prevent or arrest putrefaction and fermentation, and deprive of activity all disease germs. Corrosive sublimate and the tar acids (carbolic and cresylic) are examples of powerful antiseptics.

It is impossible in this place to avoid a passing reference to the zymotic theories of fermentation, putrefaction, and infection, which have been so keenly contested of late years. The brilliant train of research, originated by Pasteur, has left no doubt of the active agency of organised bodies in the two former of these processes, and although the zymotic theory of epidemic disease is less surely established, it has been supported by so considerable a collection of facts as to have acquired a certain degree of probability, to say no more. Indeed, the action of antiseptics is itself an argument of some value in its favour, for if a certain agent—carbolic acid, for instance—stops fermentation—stops putrefaction, and stops the spread of infection, it is difficult to avoid applying to the third case the reasoning known to be valid as regards the first two. For our present purpose, at any rate, it is convenient to assume the truth of the theory, even where proof is still imperfect, and this we have already done tacitly in our definition of antiseptics.

In studying the action of disinfectants proper (for which a distinct name might advantageously be coined), we soon meet with one of the greatest difficulties that beset the subject of disinfection. For disinfection to be effective, a definite and often a considerable quantity of the disinfecting agent must be used. If the supply be insufficient we may, and often do, have deodorisation, but not a true disinfection. For unfortunately, the most deadly—that is, the organised portions of the noxious matters to be dealt with—are not those which are most easily attacked. The foul-smelling gases and vapours formed during putrefaction or exhaled from animal exuviæ are destroyed before the organised matter; as an insect or a bird would be unharmed by the presence of a quantity of chlorine or nitrious acid in the atmosphere, which would instantly destroy or combine with such compounds as sulphuretted hydrogen or ammonia. This is the difficulty which we have so often dwelt upon in regard to water. When sewage is discharged into a river, the organic matter is gradually oxidised at the expense of the dissolved oxygen of the water; but even when the whole of the lifeless organic matter is destroyed in this way, we lack security that the organised or living matter has likewise been destroyed, and this is just the portion that we fear. The disinfectant in this case is oxygen, and we see that its perfect action depends on two conditions—on quantity, namely, and time.

Another great difficulty in the selection and use of disinfecting agents arises from the various circumstances under which they have to be applied. The physician is called upon to disinfect air, water, houses, ships, and clothes, and in every one of these cases the conditions may vary considerably. The offensive cause may be for the time irremovable, as during the progress of an infectious fever. Very commonly the operations have to be carried on in the presence of patients and friends, who must not be poisoned. The disinfection of the floor of a house into which sewer gases find entrance is a very different thing from the disinfection of a house after scarlet fever, and the disinfection of a cesspool or foul pond is different from either. Dr. Angus Smith, with his usual penetration, has drawn attention to the great variations that are produced in the action of disinfectants by the presence of considerable quantities of water, and other similar modifying influences might be mentioned. These considerations are familiar to all who have studied sanitary science, and serve to remind us of the vast difference between mere theoretical knowledge and the practical skill which comes of common sense informed by study and trained by practice.

We now pass to an account of the chief chemical agents which can be made available for purposes of disinfection, selecting those only which have, or claim to have, practical value and importance.

1. *Chlorine.*—This agent deserves the first mention, on account of its extensive use, and great value under certain circumstances. As before said, it is a disinfectant, and it acts either by removing hydrogen from organic compounds, or by promoting oxidation by withdrawing the hydrogen from water. It is commonly used in the form of chloride of lime, a compound which readily yields chlorine when treated with strong acids, and which slowly yields hypochlorous acid, an agent still more powerful than chlorine, when exposed to the carbonic acid of the air. Nevertheless it has been distinctly proved that the value of chloride of lime, and even of free chlorine, has been greatly over-rated. It is true that very small quantities of chlorine will deodorise, but a good deal is required for the destruction of organic life. The common practice of placing saucers with chloride of lime in bedrooms, closets, &c., is not only useless but dangerous, as tending to induce a false confidence, and even when a faint chlorous smell is produced by the addition of hydrochloric acid to chloride of lime, or peroxide of manganese, or by the immersion of crystals of chlorate of potash in hydrochloric acid (all of which processes have been recommended), the result is equally ineffective. The only true use for chlorine in disinfection is for the disinfection of houses, clothing, and bedding after disease. For the purpose the rooms must be closed up, and the chlorine generated in such quantity as to prevent the possibility for human beings entering for many hours.

The following is the formula given by M. Regnault, and used in Paris after the siege for the disinfection of linen and bedding. We give English weights and measures approximating to the originals. One pound of chloride of lime sewn up loosely in a strong canvas bag is immersed in a mixture of a pint and a half of common hydrochloric acid, with four pints and a half of water. The room is then closed, and the goods exposed to the gas for twenty-four hours, after which the room is ventilated for forty-eight hours. Everything may then be considered safe. The action of chlorine on delicate metal work and coloured fabrics must of course be borne in mind.

2. *Nitrous fumes.*—The brown fumes of peroxide of nitrogen, produced by the action of nitric acid on copper in the presence of air, were found by a recent special commission of the French Academy of Sciences to be more powerful than chlorine as a destructive disinfectant. For a room of 1000 or 1500 cubic feet the following are the proportions recommended. Water, half a gallon; ordinary nitric acid, three pounds and a half; copper, three quarters of a pound; the whole in a two-gallon earthenware pan. Doors, windows, and the fireplace must be carefully closed with gummed paper, and extreme care used in entering the room, as the fumes are most deadly. The commissioners, however, go on to say that carbolic acid, which is far less expensive and noxious, and more convenient, is, when properly used, equally efficacious.

3. *Ozone.*—Although oxygen is nature's great disinfectant, its operation is too slow to render it available for the purposes of our present study. The condensed and peculiarly active form of oxygen known as ozone, has, however, long been known to be a rapid and energetic disinfectant of the destructive kind. But ozone was, until lately, only to be procured in small quantities, and by inconvenient or expensive methods. The invention of Siemens' tube with the modifications which it has since undergone, renders it possible, at the present time, to procure considesable volumes of ozone at comparatively moderate cost. The question has therefore arisen in many minds whether this agent may not now be utilised for purposes of disinfection. Without pretending to dogmatise on the subject, we venture to think that any such application on the large scale is at present improbable. The cost of producing ozone would still be considerable; it would be useless except in very considerable quantity. In a concentrated state it is extremely disagreeable and irritating, and we are not aware that it possesses any great advantage over less expensive and more convenient agents. We should not have included it in this survey but for the prevalence of loose talk about it; talk which is calculated to foster extravagant ideas of its value.

4. *Sulphurous acid.*—Hitherto we have considered only gaseous disinfectants proper—that is, of the destructive kind. We now come to a gaseous antiseptic, very powerful in its action, cheap, and easy to apply, but requiring, like the preceding gases, great care in its use. For purposes of fumigation we are inclined to think it superior to all other agents, except perhaps the tar acids, with which, however, it may be associated. In the excellent researches of Mr. Crookes on the use of disinfectants in preventing the spread of the cattle plague (Chemical News, 1866, vol. xiii.), the greatest advantage was obtained by the conjoint use of sulphurous and carbolic acids (the former obtained by the burning of sulphur), and it was a point of special interest in the experiments that marked advantage was obtained by the use of the gas in quantities too small to be prejudicial to men or animals. It would appear from this that an antiseptic may do good in small quantities, when a disinfectant, under the same conditions, would be worthless—a result confirmed by many observations on the action of the tar acids.

For sulphur fumigation, when human beings and other animals are excluded, we are advised to burn 4 oz. of sulphur to every 100 cubic feet of space. This quantity is probably unnecessarily large, and it would be rather difficult to keep the sulphur from going out. It might, of course, be heated over a chauffer, or made up into torches, which last we should think a plan well worth trying.

Some of the sulphites, notably those of soda and lime, are used as antiseptics; but for purposes of disinfection they do not appear to be very valuable. —*Lancet, July 26, 1873, p. 119.*

8.—THE PERIOD OF INFECTION IN EPIDEMIC DISEASE.

From the "PROCEEDINGS OF THE EPIDEMIOLOGICAL SOCIETY, 1872-3."

An important and suggestive paper, on the period of infection in epidemic disease, read before the Epidemiological Society by Dr. William Squire, throws considerable light upon one of the most obscure questions in medicine. It is based chiefly upon personal observation, and opens out a field of research which deserves to be much more fully cultivated than it has yet been. Commencing with the proposition that in contagious maladies the infection begins as soon as the disease begins, and lasts for a variable time after it is over, the inquiry proceeds to elucidate the intricacies of the initiatory phenomena of the more ordinary epidemic disorders, with the view of guarding against infection in the earlier stages of some of them, and to bring together facts illustrative of the persistence of infection in others. The paper thus falls naturally into two parts—the first occupied with general views on the incubation of disease; the second with details of the time occupied in the development and propagation of many of the more ordinary forms. This latter part will not take up so much of our space as the former, for its value consists in embodying exact data of the intervals of infection in measles for upwards of forty cases, with similar facts for a good number of cases of mumps and chicken-pox, besides some suggestive examples in hooping-cough, scarlet fever, and typhoid, sufficient to show how much has still to be done for these diseases. In measles it is not too much to say that the record here given by Dr. Squire is as valuable for comparison and reference as that given for typhus by Dr. Murchison in the St. Thomas's Hospital Reports for January of this year.

A reason for entering at length into the incubation period of epidemic disease is that some diseases, such as measles and hooping-cough, are propagated almost as certainly while in this preliminary stage as when fully developed, and that both these complaints are often insidiously introduced into homes where it would be specially desirable to prevent their entrance, by want of attention to this fact; so that the author is at some pains to enforce the axiom that "diseases may be infectious before they are developed," and that the key to this apparent paradox is in the study of the incubation period. The two separate stages of latency and of invasion, into which the incubation period is divided, give the one the time necessary for the ordinary precautions of quarantine, the other the time at which infection begins. The latter is the so-called "sickening" for the approaching malady, characterised by fever, sometimes by a predominance of the general symptoms over the local; so that though the nature of the special disease may at this time be doubtful, the degree and duration of the fever, as ascertained by the thermometer, aid towards an earlier certainty than could be afforded by the local symptoms alone.

The rapidity with which the poison of disease can become operative varies both as to the form of poison and as to the mode by which it finds ingress: thus, small-pox by inoculation requires nine days to produce its effects; by inhalation, fourteen. The whole of this time cannot be assigned to the period of latency, as in the last three or four days of the interval some of the initial stages of the disease are being accomplished. The same thing is observable in measles, which, developed in seven days after inoculation, requires ten or twelve days to appear after infection. The latent period varies with the mode by which the poison is introduced. There is no such variation after the diseased action has begun, for the phenomena of the last five days, however the poison may have been received, are identical, and of the last four sufficiently obvious to be popularly recognised. These days are shown to be infectious, as far as measles are concerned, by conclusive evidence.

It may be affirmed therefore of some diseases, that they cannot show themselves except after a certain interval, the stages of invasion occupying a definite time; that if to this there be added, as often there must be, an uncertain interval of latency, then consequently the "incubation period" of these diseases must be a long one. On the other hand, where the different processes that constitute the disease are gone through with extreme rapidity, such diseases can be classed together as having a short period of incubation. Small-pox, vaccinia, measles, rubeola, mumps, varicella, and typhoid fever represent diseases having a long incubation period; typhus and relapsing fever forming the connecting link with the class having a short incubation period, which includes relapsing fever, scarlet fever, diphtheria, plague, cholera, diarrhœa, influenza, erysipelas, and, lastly, hooping-cough, the connecting link of this group with the former, for it combines some of the leading features of both classes.

This classification of epidemic diseases is adopted with advantage in treating of their periods of infection. A long incubation period is followed by a definite illness, terminating in a crisis, with sequelæ neither very constant nor of long duration; a short incubation period gives rise to a sudden morbid disturbance, that may have either a long or a short process, very liable to relapses, and to prolonged and indefinite sequelæ, with infection persisting far on into convalescence. It follows that infection is spread most at the end of the diseases of the latter class, generally from impatience of their lingering effects; in the former class, as already stated, the earliest period is often a source of danger from disregard of the premonitory symptoms. Severe as is the morbid process often met with in this group, and great as the amonnt of infection must be while at its height (for the amount of infecting poison must always be directly as the amount of the disease present,) yet when the diseased action is over and the past evidence of disease cleared away, as after the falling away of the crusts in small-pox, infection would seem to cease more quickly and completely than in many less terrible maladies. So also in typhoid fever, where the morbid process is more definite and prolonged than in typhus, infection ceases soon after, if not as soon as, convalescence is established; while typhus is most virulently contagious after more than a week of very complete convalescence. Hooping-cough is associated with the first group more because of the frequency with which it is communicated in its early stages, than from any valid claim it may have to be considered long in its incubation period. The stage of invasion is often considerably prolonged, but it matters little whether we consider this as really part of the disease without the hoop so long as we recognise it as an infectious part of it. The remarkable association of hooping-cough with measles gives it a claim to some notice here; statistics on a large scale show that it is so associated, and partial observation very generally remarks the connection. There is also a close parallelism in their natural history, so that sometimes a doubt may remain whether in a particular instance the two diseases contracted at the same time have been running their course together, the hooping-cough at first marked by the more prominent phenomena of the eruptive disease; or whether some of the pulmonary complications of measles would account for certain persistent symptoms common to both. Of these two explanations, the former is supported by several cases of mixed measles and hooping-cough, as well as by the analogy found in one or two cases where hooping-cough was complicated with varicella and vaccinia; while the latter would imply that the one disease might be only a phase or further development of the other. This would appear to be less likely than that after one disease has run its course there should remain a special liability or susceptibility to suffer from the other. Regarded by the light of the instances adduced, measles and hooping-cough may be taken as the two members of totally distinct groups or circles that approximate the nearest to each other, but are individually as separate and distinguishable as are measles and mumps.

When the infection of both measles and mumps is received at the same time, the measles makes its appearance first, and the mumps shows itself about ten days after the measles has subsided. In this conjuncture, as in that of measles and hooping-cough, the ordinary incubation period of measles is prolonged; and that of varicella, judging from one of the cases, would seem to be prolonged also. Hooping-cough taken with measles is seldom fully declared in less than three weeks after the eruptive disorder, the one disease interrupting the other; the more nearly allied diseases, measles and mumps, can run part of their course together, for allowing for some mutual retardation, the time at which the mumps follows the measles is very nearly the same as is often occupied in the development of mumps when no eruption of measles has intervened.

The period of incubation in twenty-five cases of mumps was from 14 to 18 days in fifteen of the cases; of the remaining ten cases, six occupied from 18 to 20 days, three 21 days, and one 22 days; so that for a preventive quarantine, when the poison of mumps has to be guarded against, fully three weeks must be allowed. It is noticeable in these cases, as in those of measles, that a short limited exposure to infection does not serve to fix the shortest period of incubation, but has been followed by a longer interval of freedom from all signs of disease than when the exposure has been more continuous. When measles and mumps have been received at the same time, measles, having the shorter incubation, shows itself first. To those susceptible of taking both infections, who are in contact only until the eruptive stage of the first is over, measles only is communicated. It is possible when measles and hooping-cough are combined, for measles only to be communicated to others susceptible of both. When a child insusceptible of either, from having previously gone through one complaint, takes the other, that alone is communicated to children who have previously had neither; thus, by a process of clinical analysis, these different diseases, in whatever degree they may be related to each other, are shown to be essentially distinct. The true alliance of the epidemic and contagious roseola or rubeola is with measles, from which it is sufficiently distinct, and certainly is not merely a spurious or second attack of true measles morbilis.

The concurrence of scarlet fever with measles has been very rarely observed. Even where they have prevailed simultaneously in the same establishment, the susceptible have taken the one or the other with very little modification of type. In one case a girl was thought to have slight symptoms of scarlet fever, and was removed from a school where a servant had measles. This girl soon lost her sore throat, but fell ill with measles in the following week. Some of the girls who remained at the school were ill with scarlet fever at the same time. They of course were isolated till they could be safely removed, and did not take measles, one of them having measles in a very characteristic manner a year afterwards.

The incubation period of scarlet fever is a short one, so that a week or eight days may be all that is requisite for quarantine purposes; and early separation often suffices to prevent its spread; but the length of time infection may linger is shown to be a much longer one than is generally supposed. Two cases are given where personal infection persisted for sixty-six and seventy days respectively. It would seem, however, that infection may be received and either lie dormant for a limited time, till some shock starts it into activity, or that its effects are intercepted at an early stage, as in a case extracted from a clinical lecture of Sir James Paget's, where scarlet fever, just appearing after lithotomy in a boy, was delayed for nearly a month, till the wound had healed; and three weeks after scarlet fever hooping-cough made its appearance. A similar prolongation of the incubation of typhoid is given, where some premonitory diarrhœa, already existing a week, ceased while the patient was under an operation for fistula. Three weeks afterwards, as soon as the wound had healed, a rigor occurred, followed by high fever and diarrhœa, characteristic of typhoid, which then ran its usual course.

The old maxim of "two diseases not co-existing" still retains much of its force. In the concurrence of two diseases we see an inhibiting power on the part of the predominant malady; the diseased processes already established interrupting or retarding others that are imminent, thus vitiating to a certain extent our calculations as to the period or duration of infection.

Another possible source of infection, sometimes overlooked, is in the second or modified attacks of many of these diseases. Modified small-pox, in its mildest form, is known to be capable of conveying the disease to others who may be susceptible of its worst effects. Persons who are said to have had scarlet fever and hooping-cough at some previous time are not safe from the liability of giving the disease to others by means of personal infection from slight sore throat or merely tickling cough, as well as by conveyance in their clothes or by neglect of personal cleanliness. The length of time that infection may cling to chambers, surfaces, and clothes, is well known.

One deduction of the greatest practical importance is drawn from observing how constantly even the most diffusible infections, such as scarlet fever and hooping-cough, spread to those nearest together, whether occupying the same bed, the same class, or the same room, if that be a small one; and how greatly the fatality is augmented where there is either over-crowding, or where after a death from one of those diseases delay occurs in effecting the requisite separation. Our deduction is this: that even though the spread of these diseases cannot be arrested by the most careful watchfulness, the injury they inflict upon the community or the individual might be reduced to a degree that is almost inconceivable, if, to the ordinary improvements in drainage and water supply of the district, we could have sufficient sleeping accommodation and breathing space in the habitations of the family.—*Practitioner, June* 1873, *p.* 406.

9.—ON THE TREATMENT OF ENTERIC FEVER BY THE USE OF INTERNAL DISINFECTION.

By Dr. Stephen Skinner.

[In this paper Dr. Skinner draws our attention to what he considers the real value of sulphocarbolate of sodium as a mode of treatment for enteric fever. He says,]

In the Practitioner for June 1872, I read Dr. Arthur Sansom's able article on "The Antiseptic Treatment of Small-pox," and I was then so much impressed with his views on the subject of internal disinfection that I resolved on giving it a fair trial in every case of smallpox or enteric fever I could meet with. Since then I have been able to treat twenty cases of the latter disease on this principle, and the result has been so satisfactory that I consider it justifies my confidence in the method. A word as to my mode of administering the drug. I generally commence with twenty grains every fourth hour, as a dose for an adult, and then I gradually increase during the next few days to the full dose of thirty grains. Beyond this I have found that I cannot go without producing cerebral symptoms as the result, which have, however, quickly disappeared upon the diminution of the dose of the drug.

Children, I have found, require much smaller doses in proportion than adults. Where the diarrhœa has been excessive, I have combined small doses of Batty's liquor opii sedativus with each dose of the remedy, until the bowels have been quieted.

With the exception of one case, which terminated fatally on the fourteenth day of fever, all the remainder progressed favourably towards recovery.

The length of time the fever lasted in each case appears to me to bear a direct ratio to the promptitude with which the drug was administered. In those cases in which I was able to exhibit the remedy during the period of incubation, the fever disappeared quicker than when it was given on the first or subsequent days of the disease. I have also administered the sulpho-carbolate of sodium to people with premonitory symptoms of typhoid, occurring in four instances in houses where I was already in attendance, and I have been glad to find that the disease has never developed itself; it has, as it were, aborted; after from six to ten days of general discomfort, the bowels have assumed their natural tone and the excrement its normal appearance, the tongue has cleaned, and the patients have declared themselves to be in their usual health again. In these cases I fully believe that if the drug had not been administered, typhoid fever would have been developed.

This is, however, a matter of conjecture only, and beyond proof, and therefore I hope that every one who has opportunities of observing the action of this remedy on the class of cases alluded to, will give the profession the benefit of their experience.—*Practitioner, Sept.* 1873, *p.* 176.

10.—OBSERVATIONS ON FEVER.

By Dr. Thomas Wrigley Grimshaw, Senior Physician to the Fever Hospital, Cork Street, Dublin.

[In the following paper the leading symptoms of the several forms of fever are compared. It is an abstract of a clinical lecture delivered in Steevens' Hospital.]

The essential cause of typhus is overcrowding. Many examples of this are to be found in the various works on fever, and my recent investigations as to the distribution of fever in Dublin have led me to give this statement my fullest concurrence.

I have adduced several most remarkable and well-established cases of typhus originating from this cause. It may also arise directly from contagion, and it is by far the most contagious of all the continued fevers. A proof of this is furnished by the fact that nurses employed in fever hospitals rarely escape unscathed. Thus we find that the majority of those in Cork Street Hospital are attacked with typhus fever shortly after entering on their duties there, while cases of enteric are scarcely ever met with under like conditions.

The exciting cause of enteric fever is now well known to be connected with the introduction of decomposing fæcal matter into the system by the mouth. What may be the exact product of decomposition which gives rise to this formidable disease has not yet been ascertained. It has, however, been satisfactorily proved that this principle, be it what it may, can enter the system in the form of contaminated food or drink, or as a miasma in the air itself. In the lecture before alluded to I have drawn attention to the fact that enteric fever has been propagated by means of water tainted by sewage, by milk adulterated with animal matter, and, as a matter of course, by fæcal matter. The recent illness of H.R.H. the Prince of Wales has been justly attributed to fæcal miasm. Many examples of the connection of this disease with the drinking of water, impregnated with sewage matter, are to be found in the reports of the Medical Officers of the Privy Council for England.

I may here adduce, as an example, an instance which I recently met with when in attendance on a large educational establishment in this city. I need not mention the institution by name, but it will doubtless be recognised by some. In one of the buildings connected with this establishment lectures are delivered, which are attended by pupils of both sexes for several hours each day. An epidemic of enteric fever broke out among the males, but none of the females were attacked. It was manifest, therefore, that the cause of the fever affected the males exclusively. These were divided into two classes; one set occupied the building in which the lectures were delivered, and the other lived in a separate house in an adjoining street. Both sets, however, caught fever, so it was manifest that the exciting cause was common to both. Now, the only condition common to both was the frequenting of the building where the lecture rooms were, but the females were constantly in the same building and did not get fever. Therefore, it was not merely the building itself, but some particular part of it, where the cause existed. This was soon discovered, for I found that the latrines which were used by the males, but not of course by the females, were so badly drained that twice daily, at the rise of the tide, the drainage was driven back into the area in which the latrines were situated, thus poisoning the atmosphere of the place, and the result was, as might be expected, enteric fever, and that too of a very fatal type. Enteric fever may spread by contagion; this, however, seems to be of a peculiar character, viz., to be derivable only from the discharges from the bowels of an infected person, which can of course be obviated by using ordinary precautions. The physician, however, in treating cases of this disease, may fearlessly approach his patient's bedside, as cases are but rare where the fever is caught in this manner; indeed, I only know of one instance where I believe enteric fever to have spread from one patient to another; that case I am sure some of you recollect.

Having disposed of the causes of typhus and enteric, we will now consider shortly the origin of relapsing fever. We naturally ask, "what gives rise to it?" The answer is, beyond all question, *starvation*. It is scarcely ever heard of, except in times of want and scarcity of food. In Europe it has always followed periods of famine and dearth. In our own country its ravages aggravated the horrors of the famine years, 1817, 1846, and 1847. Later than this, it appeared in Silesia under similar circumstances, and I believe that Lancashire was only saved (and that not entirely) from a similar visitation, as a sequence of the cotton famine of 1862 and the following years, by the munificence and liberality of the British nation during that great crisis.

With all due respect to the great physicians who were my predecessors in the treatment of fever in this country, I must candidly express my belief that they did not sufficiently distinguish between typhus and relapsing fever.

We hear of the famine fever relapsing, and we hear of it "relapsing into typhus." Now, I cannot give my concurrence to this statement. That patients labouring under relapsing fever were attacked with typhus I certainly do believe, but I am convinced that they caught it, and too often died of it, in hospitals, whither they had been brought to be cured of their first disease.

It is no argument to say that the relapse was typhus with maculæ, although the first exacerbation had been fever without maculæ, when the patient had passed through his first fever alongside a typhus patient, from whom he had every opportunity of contracting that disease; and I must also assert, in opposition to certain authorities, who maintain that one form of fever is a certain prophylactic against another, that I have seen a patient suffering from enteric fever get typhus in precisely the same way.

The true relation between typhus fever and relapsing is this: that both are liable to spread at the same time, since dearth, giving rise to starvation, promotes one form, while overcrowding, another result of the same lamentable state of things, produces the other, and accordingly both prevail at the same time and are liable to attack even the same individual. This is also true of typhus and enteric, for where we find the causes of both in existence at the same time, we may, as in the former case, find both diseases attacking the same persons contemporaneously. In proof of this statement, I may mention that I recently published, in the Irish Hospital Gazette, cases of a family, of whom some members had typhus, some enteric, and some both forms of fever contemporaneously, and when I went to examine the place from whence the patients came, I found in the overcrowded condition of the house and the bad drainage, not to go into further particulars, quite sufficient evidence to account for the fact. I have now endeavoured to point out the main features in the etiology of fever, and our next lecture will be devoted to the examination of the symptoms of the varieties of continued fever, and their differential diagnosis.

Let us examine the special symptoms of each form of fever. In doing so, our attention is naturally directed first to the mode of invasion. This may be sudden, slow, or even so gradual as to be almost imperceptible. In typhus, the patient is generally unwell for some days, and then usually a distinct rigor comes on, decidedly marking the accession of the disease. Enteric fever comes on much more insidiously. On questioning the patient, you will probably learn that he has been out of sorts for a considerable period, that he has had a bad taste in his mouth, and perhaps that his bowels are too free. He will complain generally of chilliness, but will seldom have had a shivering fit sufficiently distinct to arrest his attention. In relapsing fever a specific rigor invariably ushers in the disease, and is followed rapidly by a hot and then a sweating stage, especially if the disease does not directly result from starvation.

In treating of the special diagnostic signs and symptoms of the three continued fevers we shall consider them from fixed standpoints, viz., their connection with the various systems.

The symptoms referable to the integumentary system are those connected with the eruption. In typhus fever, about the fifth or sixth day from the commencement, a dusky reddish brown mottling becomes apparent. It is generally first seen over the folds of the axilla and front of the chest, and then spreads *over the sides and the* abdomen. It is not usually found at first *on the extremities, but when it* is, you will almost invariably find it on the *arms, more seldom on the legs,* and comparatively rarely on the face.

This eruption is much redder in the young than in adults. It gradually becomes more defined, and distinct spots—*maculæ*—appear. These may be raised and irregular, and even disappear on pressure. There is likewise frequently observed a kind of greyish marbling, supposed by some to have been the precursor of these brownish spots. After the lapse of a day or two the maculæ become thicker, do not disappear on pressure, but continue for days, leaving permanent marks, called *petechiæ*. These are simply a series of minute ecchymoses caused by the extravasation of blood from the weakened capillaries. When the fever begins to subside the mottling gradually fades away, but the petechial markings persist for a considerable time subsequently.

Enteric fever differs from typhus in respect to its eruption. In many cases no rash whatever can be discovered, but in the majority it is present. There is invariably some difficulty about fixing the precise date of its appearance, owing to the uncertainty that usually prevails as to when the disease actually commences. We may, however, as a general rule, look for its advent from the seventh to the twelfth day of the fever.

The character of the eruption is—A few rose-coloured spots, often not very easily discoverable (especially on the dirty and flea-bitten skins of our hospital patients). They are about an eighth of an inch in diameter, slightly raised, and disappear on pressure; they vary in number from one to twenty or thirty. They are usually first found on the abdomen and chest, but especially on the sides of the abdomen. In females, a usual situation of these spots is the breast. They keep coming out in successive crops for a fortnight, or even longer, and never change until they disappear; they may even be present for a portion of a day and then vanish. You can observe these phenomena by tracing a circle with ink round one of these rose spots, and watching it from time to time. The average duration of each spot is two or three days. They seldom, if ever, appear after the thirtieth day of the illness.

It is very different to distinguish the eruption of enteric fever from that of typhus at first, but after a couple of days the typhus eruption will be found assuming a darker color, and failing to disappear on pressure, and, furthermore, no fresh crops ever come out. The opposite is, as we have seen the case, in enteric fever. In some cases, however, the two eruptions have been found to co-exist, but in such cases, other symptoms of both fevers are also present.

With regard to relapsing fever we have nothing to remark on the subject of rash, owing to the fact that there is no characteristic eruption connected with this disease.

In the continued fevers, as in many other affections, the pulse is a most important index as to the patient's condition.

In typhus the general range of the pulse is from 100 to 120 beats per minute. If it be much under 100 (80 or 70), it becomes a very serious and often dangerous sign, and augurs badly as to the termination of the case. It presents the usual febrile characters, is soft and undulatory, especially about the sixth or seventh day of the disease. The tension is lower in typhus than in enteric fever. Two causes have been assigned for this low tension of the pulse. First, a flaccid condition of the capillaries; and, secondly, weakness of the heart itself. We find the force of the heart becomes diminished, the first sound is but faintly heard, and in many cases is completely lost. This is our great criterion in estimating the strength or weakness of our patient, and hence the vital importance of constant and careful auscultation. In enteric fever the pulse is frequently dicrotous, but its febrile characters are not so well marked unless the disease assumes a distinctly typhoid form. Marked weakness of the heart, similar to that so common in typhus, is not frequent, but when it does occur, it usually points to a fatal result. With regard to the indications afforded by the pulse in relapsing fever, I have not seen a sufficient number of cases of this disease to be able from personal observation to put you in possession of its special peculiarities.

The respiratory symptoms would next demand our attention, but as we shall have to deal with them somewhat at length when we come to treat of complications and sequelæ, we shall for the present defer their consideration.

The digestive system suffers *comparatively* little in typhus. There is, as we might expect, great dryness of the mucous surfaces and suspension of the normal digestive functions. The condition of the tongue, however, is most important to notice. At first it becomes coated with a whitish yellow fur, extending from the centre to the sides; this becomes gradually brown and then black, and the teeth loaded with sordes. When the fever begins to subside the parched tongue begins to moisten, cracks appear on the surface, the sordes disappear, and it gradually reassumes its normal condition. In enteric fever, the symptoms referable to the digestive system are very well marked. To commence with the tongue. It is usually white at first, and red at the edges and tip, but if the case assumes the typhoid form the tongue presents all the characters of a typhus tongue. Sometimes, instead of being furred, the tongue becomes red all over, commencing with a strip of red at each side. This is not, however, to be relied upon as a diagnostic sign.

Vomiting frequently occurs early in the course of enteric fever, and this, taken in connection with the other symptoms, affords a useful sign for distinguishing this disease from typhus, in which vomiting is not nearly so common.

Diarrhœa is almost invariably present in enteric fever; this is connected with inflammation or ulceration of the glands of the intestine, but of this we shall speak more particularly hereafter. In typhus the bowels are nearly always confined, but diarrhœa may be present.

The course which the temperature follows differs in each form of fever. In typhus, the range of temperature follows the "continuous" variety, that is, continues to rise steadily during the first few days to about 104·5° or 105°, then gradually falls, without any sudden variations except such as may be due to the occurrence of complications. The highest temperature is usually reached about the seventh or ninth day.

In relapsing fever, at each exacerbation, the temperature rises quickly and suddenly to its full height, from 105° to 107°, and as quickly falls, and therefore the range belongs to the "acuminated" variety.

In enteric fever the range of temperature belongs to the "interrupted" variety, that is, rises and falls frequently. It may reach the height of 104° or more, frequently during the course of the disease having extensive daily fluctuations (sometimes of many degrees), the highest daily temperature usually but not always being in the evening.

The height of temperature is of very different import in the different forms of fever. In typhus, a temperature above 104·5° is severe, above 105° dangerous, and 106° is seldom compatible with life. In enteric fever these rises are not of such serious import. In relapsing fever temperatures of 106° or 107° seem to be of little consequence so far as prognosis is concerned.

Sleeplessness, delirium, &c., we shall discuss in our next lecture in connection with the subject of complications, and I shall conclude for the present with a few words on the duration of the three forms of fever.

Typhus usually lasts about fourteen days. Defervescence takes place by crisis, which is most commonly accomplished either by sleep or profuse sweating. There is seldom a crisis to an attack of enteric fever. The duration of the disease is most uncertain. Three weeks is the average, but it frequently lasts much longer, and the patient may continue in a feverish condition—sometimes better and sometimes worse—even for months. In relapsing fever the first attack lasts seven days, and is followed in a few days by another exacerbation. There is no rule as to the number of relapses. They may vary from two to twelve. The first attack is the worst, and each succeeding one is less severe. We have now briefly considered the more important of the three varieties of continued fever.—*Med. Press and Circular, June* 18, *and July* 2, 1873, *pp.* 529, 3.

11.—A FEW WORDS ON THE MEANS NECESSARY TO BE TAKEN FOR PREVENTING THE SPREAD OF ZYMOTIC DISEASE.

By Dr. F. PAGE ATKINSON, late Surgeon St. Bartholomew's Hospital, Chatham, and Royal South London Dispensary.

We all, no doubt, believe in the truth of the saying, that "prevention is better than cure," but how to prevent disease we are often at a loss to understand. Epidemics are not unfrequently looked upon as direct visitations of Providence, and as such to be quite incapable of being warded off; but to this it may be replied, that Providence has laid down certain laws for the preservation of health, and that if we transgress these, we must expect to suffer sooner or later. Half the illness which takes place may be shown to arise from ignorance of, or a want of respect for, the laws which nature wishes us to follow, and to be, truly speaking, as preventible as scalds and burns. It may be asserted, therefore, without fear of contradiction, that by a proper and careful study of nature's laws, we may escape from several of the numerous ills that flesh is now considered heir to. Whenever infectious disease of any kind makes its appearance, we should take a careful survey of the premises where it exists, inquire as to the ventilation, drainage, and water-supply, and see that the latter is not contaminated by infiltrations from water-closets, dust-bins, manure-heaps, &c. We should then take into consideration the number of persons occupying each room, and the means taken for separating the healthy from the sick. If there is insufficient accommodation for the sick, they should be removed to a hospital in one of the public conveyances which are kept especially for the purpose. Where death occurs, the corpse should be removed to a public mortuary, and the house and all things in it, in either of the above-mentioned cases, should undergo a thorough cleansing and disinfecting. The Sanitary Act of 1866 provides very wisely, among other things, for the prosecution of those who—1st, Use public conveyances while suffering under contagious or infectious disorders; 2nd, Knowingly convey such people in any public conveyance; 3rd, Wilfully expose themselves in any street, &c.; 4th, Knowingly let a house or part in which any person has been suffering from any contagious or infectious disorder, without having had it and all the articles in it previously disinfected.

In order to prevent the spread of zymotic disease from house to house, it is necessary in all cases to provide for—1st, Complete separation of the sick and those in attendance from those who are in health; 2nd, Disinfection of the sick-room and all articles in it, both during its occupation and after the removal of the patient; 3rd. Disinfection of the secretions; 4th, Thorough ventilation.

In the first place, then, the patient should be moved into a separate apartment near the top of the house, with the nurse who is to be in attendance. All curtains, bed furniture, carpets, &c., should be removed prior to its occupation, and no persons should be allowed to enter the room except the medical attendant. Outside the door a sheet should be hung, thoroughly soaked with a solution of carbolic acid, and the nurse should never pass this under any circumstances whatever. All articles of food should be placed under the sheet, and not taken in by the nurse till the servant has gone away. Inside the apartment, disinfection should be carried out by means of sprinkling sulphur on a live coal two or three times a day, and wafting the fumes all around, till sneezing and a feeling of suffocation are produced. By the bedside a basin should be placed for the patient to spit into, containing a solution of carbolic acid. Rags should be used for wiping the nose, &c., and should be burnt immediately afterwards. All soiled bed-linen should be placed in a solution of the acid as it is removed. Discharges from the bowels and kidneys should be disinfected by the same means; and all plates, dishes, glasses, &c., should be washed in boiling water, and thoroughly cleansed with Condy's fluid or chloralum before leaving the room. As soon as the patient is perfectly recovered, he should wash the body thoroughly over with carbolic acid soap. He should then pass out at *the door quite naked*, and wrap himself in a fresh blanket, which has been left *on the outside. In this way* he may pass into another room and dress, and *then he may safely mix* with the other members of the household. The nurse should act in the same way; but before leaving the room she should see that all the linen articles are placed in a solution of carbolic acid, and that the other things are hung upon lines, in order that they may undergo a thorough fumigation. She should next place an old saucepan, containing some hot coals and sulphur, in the middle of the room, and then make her exit, taking care to close the door after her. At the expiration of twenty-four or forty-eight hours, the windows should be thrown open, and disinfection may be then said to be complete.

These rules should be carried out in the case of all infectious diseases; but certain other precautions are necessary to be adopted peculiar to the particular disease we are called upon to treat. For instance, in the case of *scarlet fever*, as soon as the eruption begins to fade, or as soon as there is the slightest appearance of peeling, the patient should be thoroughly oiled all over, night and morning, for three days. He should then have a warm bath *on going to bed*, and the same process should be repeated at the same interval of time till all signs of peeling have disappeared. When the skin is perfectly clear, he may, after taking a bath, pass, in the manner directed, into another room and dress, and then mix with the other members of the household.

The chief thing to fear in these cases is the thin skin which peels off from the various parts of the body, and floats about imperceptibly in the air. The oiling process and disinfection, if strictly attended to, entirely prevent the spread ot the disease.

In order to prevent the spread of *smallpox*, it is necessary, on its very first appearance, to vaccinate all in the same house, and, if other cases occur, all in the neighbourhood. The patient should not be allowed to leave his room till all appearance of suppuration has passed away. All articles of bed furniture and clothes that have been worn, should be burnt as soon as the case is well. Keeping the apartment dark is of great use in lessening the formation of matter, and so the spread of the disease.

In *enteric fever*, the chief thing one has to do is to see that the secretions are thoroughly disinfected, for it is by this means chiefly that the disease propagates itself. Care must be also taken whenever there is the slightest tendency to a relapse.

To prevent the spread of *typhus fever*, it is absolutely essential that there should be strict isolation and thorough fumigation, as the power of infection in these cases is exceedingly great.

In *cholera*, it is the breath, vomit, and stools, that are so particularly infectious; but the disease spreads mostly by the drinking of contaminated water. The stools, therefore, should be thoroughly disinfected, and care should be taken that they are not thrown into any place where they are likely to come in contact with the drinking water. In India, the chief thing to do is to try and prevent the natives from depositing their fæces in the soil all around the villages, and those who have the disease in an incipient stage from bathing in the tanks.

Diphtheria, *erysipelas*, and *puerperal fever*, are highly infectious and contagious, and it is highly important to see that the hands are thoroughly washed in disinfecting fluid after each examination. It is also as well to avoid going from a case of erysipelas direct to examine a healthy wound. When in attendance upon a case of puerperal fever, midwifery practice should be given up for a time altogether. The nurse also in charge should be warned not to engage herself for another case until a month or two has elapsed after leaving the house. The same advice also should be given in the case of *scarlatina*.

As regards the nature of the poison or poisons which cause the outbreak of fever, a good deal has already been written, and will still continue to be written. Some say that germs are the originators of disease; others (as Dr. Elliott of Hull), that these are only the carriers of the poison, since we cannot distinguish between healthy and unhealthy pus, the skin that comes from a healthy body and one affected with scarlatina, &c. Béchamps considers the body to be made up of minute creatures called microzymes, and that when these act harmoniously, the body is in health, and the fermentative processes go on regularly; but that when these act *inharmoniously*, the fermentative processes are deranged, *and ill health is the consequence.* The microzymes, he says, are not ferments *in themselves, but they produce* very small creatures called bacteria, and these produce cells. The cells and the bacteria are capable of returning to the microzymic state. After death, all organic matter returns to its original elements, and the microzymes are there to carry on the work of putrefaction. According to this idea, the living animal contains within itself the essential elements of life, disease, death, and total destruction; and in order to accomplish these last-mentioned ends, it is unnecessary to suppose the presence of living germs floating in the air.

As to whether germs are or are not the cause of disease, it is still uncertain; but, nevertheless, we all admit the benefits that arise from the use of antiseptics and disinfectants, and proper sanitary supervision.—*Edinburgh Medical Journal, June* 1873, *p.* 1083.

12.—ON THE GENERATION OF TYPHUS FEVER BY OVERCROWDING.

By Dr. James Martin, Physician to the Portlaw Hospital.

About the middle of September, 1872, an epidemic of measles set in, in Portlaw, no case having occurred for several years previously.

The summer had been healthy, and no case of typhus-fever had occurred for six or seven months. Early in October, the weather being damp and foggy, measles had reached nearly every house in the town, over 450 cases being registered before the 1st of November.

Every practitioner knows the prejudice the poorer classes entertain against the practice of cleanliness during an attack of measles, it being impossible to persuade them to change the clothes or wash the person of the sick. Further, amongst a working population the beds are almost always overcrowded. The consequence therefore was, that three or four children ill of measles often lay in the same bed for ten days or a fortnight, without any ablution or change of bed clothes or garments.

The second week in October four children lay ill in one bed, all being very dirty. They were tended day and night by their mother, who took what sleep she had in the same bed. After ten days had elapsed from the setting in of the measles, the mother was seized with very malignant maculated typhus, under which she sank on the eighth day.

At the same time, in a house about 200 yards distant, with which no communication could be traced, six children lay ill also of measles. In this house the beds were tolerably clean, but fifteen persons, young and old, slept in four beds. The eldest daughter, aged eighteen years, who had measles some time before, took ill of fever, was admitted into hospital, and passed through a heavy attack of the maculated type. Each of the children, as they recovered from the measles, took the fever, were admitted to hospital, and were all maculated. The parents and grandmother, whom I remember to have had maculated typhus in past years, escaped.

At the same time also, in a clean, fairly ventilated, roomy house, fully a quarter of a mile distant from the two former, a woman, just a week after her confinement, lay in the same bed with four children in measles, trying to nurse them herself. After a week or ten days she became ill with fever, and died on the tenth day, being also maculated. No communication could be traced between this family and either of the two former. I could follow up these statements with the history of six other families where fever seized on the members, who, having had measles before, were similarly exposed to the bad effects of dirt and overcrowding, but typhus having been then established in the town, I could not be sure of the freedom from communication with infected cases. All the cases of typhus, eleven in number, recovered well, except the first, which was exposed to the dirt poison in a most concentrated form, and refused to go to hospital; and the third, who was seized with the fever within a month after parturition. A marked feature in all the cases (which, with the exception of the two fatal ones, were treated in hospital) was the severe pulmonic symptoms under which they suffered.

The facts, 1st, that so many cases of typhus occurred under precisely similar circumstances, without being able to trace any communication between them; 2nd, that there had not been any typhus in the district for a long period, and that I could not discover circumstances to link them with cases occurring elsewhere—lead me to the conclusion that this outbreak of typhus was a clear case of ochlesis in the first three cases, and perhaps in several others.—*Dublin Journal of Medical Science, July* 1873, *p.* 36.

13.—ON THE OBSCURE ALLIANCES OF TYPHOID.

By Dr. J. P. H. Boileau, Surgeon, Army Medical Staff.

In the Lancet of the 21st November, 1868, it was stated by the editor, in his review of my article on "Fever in Malta," published in the Army Medical Reports for 1866, that to complete our knowledge of the gastric remittent fever of the Mediterranean, we only wanted pathological information. In Barbadoes a fatal case of fever was treated by me which forcibly recalled to my mind the experience I had had in Malta, and the cases of fatal typhoid recently published in the Lancet have revived in my mind the whole subject. It is very desirable that the nature of that obscure disease described by me as the "long" fever should be clearly established, and I believe the following case will help to elucidate the subject.

A young sailor of the Royal Navy was admitted into hospital at Barbadoes. A detailed account of his illness would be merely the repetition of a subject already exhausted by Dr. Marston, R.A., Dr. Chartres, 100th Regt., myself, and others who have observed and studied Mediterranean remittent, so similar was the sailor's case to that fever. I will therefore omit my notes up to the thirty-fourth day of his illness, the thirty-second of observation.

Thirty-fourth day. Progressing favourably; tongue perfectly clean and moist; bowels regularly moved twice daily; he complains only of debility; temperature has been as high as 103·2° F. (evening of ninth day); abdomen as a rule has been hot and dry. I have not once satisfied myself as to the existence of any rose spots. The boy's age is nineteen. His tongue has never had the slightest appearance of the typhoid state, nor has he had typhoid dejections. His temperature for the past three mornings has been 99·2°, and under, but—significant fact—his evening temperature has not been below 100° since the commencement of his illness.

On this day he was to have had a little chicken, so well did he appear to be, and so anxious was he to have some; but the order I countermanded, and was assured that he did not get any. I am particular in recording this matter, because if he had got the chicken we could not have done otherwise than attribute to that solid food the untimely issue of the case. On the following morning his temperature was 103·8°, and on the following evening 104·2°. In two cases of fever I have seen an equally sudden and great elevation of temperature follow the ingestion of jelly.

Thirty-sixth day. He has relapsed a second time. He has not diarrhœa; tongue remains clean; he has not any pulmonary complication.

Thirty-seventh day. As usual, no complaint.

Forty-second day. With the exception of his temperature he appeared to be progressing favourably until 3 p.m., when he was seized with a sudden and most violent pain in the abdomen. At 5 p.m. his axillary temperature had risen to 105·2°, pulse 120, and was in great agony; abdomen extremely tender. At 7 p.m. he was considerably relieved, having taken ninety drops of Battley's sedative solution.

I need not further detail the symptoms of peritonitis following perforation. He survived for no less than six days after that fatal event. The autopsy, *made six hours after death,* showed the peritoneum in a state of universal *inflammation, in several places* adherent; the cavity containing a quantity of sero-purulent fluid; the intestines healthy, with the exception of the lower portion of the ileum, where there was attenuation and superficial ulceration. One ulcer, having the area of a threepenny piece, had penetrated through the peritoneum, and here there was a scarlet peritonitis, from which, as a centre, the general inflammation extended.

Now, the indisputable facts are these: that in a tropical island I have had one case of fever under my care which so closely resembled that known as the gastric remittent of the Mediterranean that had it been under my care in Malta it would have been described as such; that the subject of this fever died about the fiftieth day of his illness from the effects of a perforating ulcer of the lower fifth of the ileum, and that I did not see any reason to doubt my diagnosis of the case as typhoid to be correct.

From my experience in Malta, I was led to a conclusion which I expressed in my paper on "Fever" in the Army Medical Reports for 1866—viz., that quinine might be banished from the island without much harm. Since that time I have had many opportunities of observing the nature and course of typical, malarial, periodic fevers, demonstratively amenable to quinine. Remarks on those cases were published in a medical publication on July 14th, 1869. I have also had the opportunity, during a residence of more than three years in the West Indian Islands, of observing a large number of cases of fever in their climate. I may, therefore, claim, to be now in better position to judge of the nature of the fevers I observed in Malta six years ago than I was then. And I cannot but conclude that if the *gastric remittent* is not the *typhoid* with which we are familiar in this country, it must be some variety of it; and, if this is the case, it illustrates the truth and the necessity of the warning given to us by the Professor of Military Medicine in the Army Medical School at Netley, whom I well remember cautioning us not to adopt the evil practice of calling fevers by local names. As to the fever being in any way malarial, I must still maintain the view I have hitherto held—viz., that it is not so in the slightest degree. And I now attach a greater force to the words of the editor of the Lancet than I did when first I read them. His words were these:—"After reading Dr. Boileau's contribution . . . we confess to having strong doubts as to whether some of his cases were not in reality examples of enteric fever."

It follows that in the treatment of the long gastric remittent fever of the Mediterranean we must ever bear in mind the complications which may arise in *enteric* fever. We should not deceive ourselves by the idea that we have a fever to deal with which is amenable to quinine. We should be scrupulously careful in the administration of diet; and not fail to mark the temperature of the patient at least every evening, and by its indication to "govern ourselves accordingly." I need scarcely say that I fully endorse Dr. Latham's opinion, that it is only after the evening temperature has remained on at least two successive days below 99° F. that we can be sure the ulcers have healed, and that solid food may be given without risk. I may add that I do not remember ever having seen a relapse after such a condition.

As a result of many thousands of observations on the temperature of the white man in various climes, with shade temperatures as high as 105° F. and as low as 20° F. below zero, I have been led to the conclusion that the normal axillary temperature of the healthy adult is between 98° and 99° F.: in fact, it seems to me that this temperature is as valuable and as constant a characteristic of his human identity as is the structure of his bones. The fact may be already established; but as the idea that human temperature is one degree higher in the tropics than elsewhere has been widely entertained and supported by high authority, I here mention the fact that I have observed.—*Lancet, Aug.* 23, 1873, *p.* 258.

14.—ON THE DISSEMINATION OF ZYMOTIC DISEASES BY MILK.

By Dr. JOHN DOUGALL, Medical Officer of Health for the Burgh of Kinning Park, Glasgow.

That enteric fever has been disseminated over wide areas by milk supply is now an established fact, in support of which we have Dr. M. W. Taylor's paper on an outbreak of enteric fever at Penrith; Dr. Ballard's and Dr. Robinson's reports on similar outbreaks, one at Islington and two at Leeds; also Dr. Russell's recent report on an outbreak at Parkhead. That scarlatina has been disseminated by milk was satisfactorily shown by Professor O. Bell, of St. Andrews, and quite recently by Dr. Robinson, of Leeds. Such being the case, I have attempted to examine the matter scientifically, and if I have partially succeeded in placing it in a clearer light I shall be satisfied. It may be here remarked that it seems to me highly probable that, if milk can propagate typhoid and scarlatina contagia, it can also propagate the contagia of the other zymotica. Proceeding on these premises, I shall consider—1st, The probability of zymotic poison being generated in milk *de novo*; 2nd, The probable nature of zymotic poisons; 3rd, Is milk a congenial soil for the propagation of zymotic poison? 4th, How may milk get infected with zymotic poison?

The Probability of Zymotic Poison being Generated in Milk.—In regard to this, I may first state that many distinguished physicians believe in the spontaneous origin of specific physical contagia. Cow's milk consists chiefly of casein or curd, fat, sugar, water, magnesia and calcic phosphates, sodic chloride, potash, &c. The colostrum, or milk, secreted for a few days after parturition, differs from normal milk in containing 15 to 25 per cent. of albumen, with less of the other constituents. We have then, in normal milk, a fermentable or putrescent body—casein and a fermentescible body—sugar. Were the casein absolutely removed from milk, the sugar would remain unchanged, because sugar in aqueous solution only and always ferments by contact with a body fermenting or putrefying. Were the sugar entirely removed from milk, the casein would still putrefy, the cause of which will be explained under the third head. But in the manufacture of cheese, which consists largely of casein, its putrefaction is prevented by the abstraction of water, the exclusion of air, and the rendering of the mass homogeneous by pressure. Milk then, from its proximate chemical composition, must be, and as we know is, eminently prone to decompose. Let us examine briefly the rationale of this decomposition. Milk newly drawn from any herbivorous animal is faintly acid, or neutral, or slightly alkaline in reaction. In the carnivora, the milk is said to be always slightly acid. When allowed to stand for a few hours, the fatty matter or cream rises to the surface, which, under the microscope, is seen to consist of highly refracting fat globules, known to have albuminous envelopes. If the weather be warm, it may be found at the same time that the milk has become acid and coagulated, the sugar converted into lactic acid (by that portion of the casein nearest the surface having become putrid, and acted on the sugar as a ferment), and the casein, being insoluble in an acid solution, ultimately separates in clots and flocci. We have now a markedly acid organic solution to deal with; and as I have clearly proven, in my recent paper on "Putrefiers and Antiseptics," in the Glasgow Medical Journal (Nov. 1872, and Feb. 1873), all acid organic solutions (if not hyperacid) constitute the most congenial soil for the propagation of fungi. Now, as many hold that zymotic diseases are propagated by cryptogamous spores, it is worth considering whether it is probable that the fungi which appear (generally in about two days) in fermenting milk may at any time be the cause of these diseases.

The Probable Nature of Zymotic Poisons.—These, of course, must either be vegetable, animal, or mineral in their nature, and appear either in a solid, liquid, or gaseous condition. For many obvious reasons it is impossible they can be mineral or inorganic; hence, they must either be vegetable or animal; and, assuming that there are no gaseous nor liquid animals nor vegetables, it follows that they must exist *per se* as solids. Whether vegetable or animal solids, however, it does not follow that they must necessarily be either fungi or microzoa, or both; nor because their normal conditions are neither gaseous nor liquid, that they are incapable of suspension in air or in fluids. All analogy in the vegetable and animal kingdoms points to fungi and microzoa as the effects, rather than the cause, of the conditions of their *habitat*, and indicates that their functions may be benign rather than malignant. Quite recently Drs. Cunningham and Lewis, in India, have carefully examined the blood of persons suffering from cholera, and failed to find in it either fungi or microzymes, actual or potential; and Dr. Beale, a most distinguished microscopist, and who has devoted great attention to this subject, says that "when bacteria and fungi do appear, they are but harmless concomitants of the disease, and that they cannot be regarded as its cause any more than the vultures which devour the carcase of a dead man can be looked upon as the cause of his death." Furthermore, "that the active contagious virus does not exhibit the properties of any vegetable or animal parasitical organism yet discovered: neither can any organism, having specific and peculiar characters, be developed from any specific virus." Hallier, on the other hand, contends that fungi and microzymes are not only the cause of putrefaction, but also of the various kinds of infection, each kind having its own specific microzyme. Micrococci are found by him in every infectious material—in the alvine discharges of diarrhœa, cholera, dysentery, and enteric fever—sometimes slightly, sometimes in vast numbers. In measles, they are found in the sputa, in small-pox, and cow-pox, in the pustules, in certain fevers in the blood, and there is no disease in which they are so abundant as scarlet fever. As an example of Hallier's views, he states that cholera in India is caused by swallowing a fungoid organism, urocystis, which constitutes the smut of rice. He found that by steeping rice in cholera stools, and after sowing it on suitable soil, urocystis was developed; and he concluded that, therefore, the fungus was the cause of cholera. These experiments were suggested by a theory of the cause of cholera, which may be stated as follows:—Cholera originates on the banks of the Ganges, where the rice plant abounds and flourishes. The contagium particles of cholera are manufactured in the cysts of a urocystis, which is parasitic on the rice plant, by the conversion of the protoplasm they contain into microzymes, so that the same material which, when brought into contact with the germinating rice grain, produces urocystis, generates cholera when introduced into the intestines. Dr. Lewis, however, easily and effectively overturned Hallier's theory, by showing urocystis could be successfully cultivated on the dejecta of persons in perfect health. It may further be stated, in confirmation of the view that microzymes do not constitute specific zymotic poisons, that in all cases where putrefaction is at its maximum stage in infecting matter, the property of infection in the given substance is lost, and in this respect the functions of bacteria are certainly benign.

Zymotic units must be excessively minute, whether composed of living solid particles, or of dead albuminoid putrefacts. Though not gaseous they are capable of being suspended unseen in air, and, though not fluid, they are susceptible of being mixed invisibly in water. According to Dr. Burdon Sanderson, bacteria germs must be less than the 100,000th part of an inch, because in water known to be zymotic they were not seen with the highest powers of the microscope, which is just saying that they only exist inferentially. If particulate, it is certain that they must be smaller than a blood corpuscle—that is less than the 1-4000th of an inch in diameter—because, if larger, or of equal size, they could not penetrate the membrane of the pulmonary cells when inhaled, nor the vascular tunics when swallowed, to pass into the circulation; as what keeps the blood corpuscles *in* must keep particles of larger or of equal size *out*. If, however, zymotic contagia consist, as some think, of minute entities of putrid amorphous protoplasm, or, as I should call it, *pathoplasm*, then they need not be so small as a blood corpuscle, because mere contact with the pulmonary membrane, or any moist absorbing surface of a susceptible organism, would be sufficient to initiate, according to the physical theory of Laplace and Bertholet, and to the physico-chemical theory of Liebig believed in by Bastian, Beale, and ably differentiated and correlated in all its pathological ramifications by Richardson, a specific blood metamorphosis conform to the nature of the primary contagium, which may be that of small-pox, cholera, scarlatina, measles, typhus, typhoid, &c. Still the simple question, What is a zymotic poison? it must be confessed, has still to be answered. The latest solid contribution to our knowledge of the subject has been made by Chauveau, and confirmed by Sanderson. These physiologists have clearly proven that the only part of vaccine and variolous matter which is contagious *is the solid* particles, which are not larger than 1-20,000th of an inch in diameter, *and not the liquid*. By an ingenious application of the law of diffusion *of fluids, they managed* to separate the solid from the fluid parts, and it was found that a child may be vaccinated or inoculated successively with the fluid parts, but without effect, while the most minute portion of the solid parts that can be separated is amply sufficient to originate all the phenomena of the cow-pock, or of variola. But, supposing zymotic contagia to consist of minute particles, as in the case of vaccine and variolous lymph, we have still to answer, What are these particles? Are they vegetable or animal organisms of definite structure, descended from a lineal parentage, and propagating similar or dissimilar offsprings? Or, are they mere simple colloidal structureless atoms—a "leperous distilment" from morbid glands, arising *de novo* under certain conditions? Supposing we give an affirmative to these questions—conceding that some are animals, some vegetables, and some formless albuminous units, of biogenetic and abiogenetic origin—then we come to water, which seems indeed to have no bottom. How is it that they cause so many distinct specific diseases? How is that they cause disease at all? How is it that, when they have caused one disease (say typhus), it cannot generally be evoked again in the same organism? How is it that they multiply so enormously in an infected organism? How is it that the specific phenomena of all the zymotica may arise successively in the same individual? And how is it that vaccinia is antagonistic to variola?

We now come to consider the 3rd part, viz.:—

Is Milk a Congenial Soil for the Propagation of Zymotic Poison?—Liebig was the first to show that all ferments possess one character in common, viz.—they all contain nitrogen, but there are certain bodies which contain nitrogen and yet are not ferments, as ammonia and hydrocyanic acid. Casein, however, acts as a ferment, as already stated, on the sugar of milk. Casein, also, in the form of pure curd, if exposed to the air, soon putrefies, and in this state has also the property of inducing a similar condition in other nitrogenous bodies. The casein acts on them as other such bodies had acted on it. The *rationale* of this process is "a transformation or new arrangement of the atoms of a compound yielding two or more new groups, and caused by contact with other substances, the elementary particles of which are themselves in a state of transformation or decomposition. It is a communication or an imparting of a state of motion which the atoms of a body in a state of motion are capable of producing in other bodies whose elementary particles are held together only by a feeble attraction."

It is well known how the mammalian offspring, though receiving no other nourishment than the mother's milk, rapidly increases in weight during the first few months after birth. This proves that milk contains all the elements necessary for building up the tissues of the young animal, and so it does, and that these must also be present in proper proportions and in suitable combinations, and so they are, to be easily assimilated by the tender organs. Milk, then, is simply a solution of the young animal's body as regards the identity of all their chemical elements, and also with reference to most of their chemical compounds. If the foregoing be postulated, it follows that, just as a child or adult is susceptible to be poisoned with small-pox, scarlatinal, or typhoid virus, so also, and in a much higher degree, must milk when exposed to the same influence. Because the living organism has vital force wherewith to oppose the attacks of toxic germs; and just in proportion to the great or little amount of that force contained in the organism will it be able entirely to withstand, or, being infected, successfully to cope with them. But milk is dead organic matter, and having no *vis vitæ*, must be a much more favourable *nidus* for the nurture of contagia than even a sickly organism. "That milk has also the peculiar property of imbibing various effluvia to a great extent, is well known to those accustomed to dairy management. There are many articles liable to impart a taste and odour to milk; boiled fish is particularly obnoxious to the dairy-maid as being sure to taste the cream and hasten decomposition of the milk. Any meat the least tainted will do the same. Onions, even from another room, will give their taste to milk. So, also, will paraffin, turpentine from paint, and varnishes, the smoke of tobacco, and the smell of lime from plaster or whitewash."

Furthermore, the additional physical property of fluidity possessed by milk *greatly increases its fertility* as a zymotic soil. It is familiar knowledge that the presence of moisture is as necessary in fermentation and putrefaction as the presence of nitrogen. This is well exemplified in many processes for the preservation of animal tissues by the freezing or exclusion of their water, and it is well known that water is a favourite *habitat* of cholera and typhoid poison.

Milk may thus be looked on as a highly favourable soil for the propagation of contagious germs—1st, In respect of the organic matter it contains; and, 2nd, in regard to its consisting largely of water.

How may milk get Infected with Zymotic Poison?—It may get infected—1st, by impure water; and 2nd, by impure air. For many years past, large quantities of milk have been sent from near and distant farms to Glasgow and other large towns. In these farm-yards the manure—human and bovine—is commonly placed in the most convenient situation for deposit and removal. Generally, at a short distance from the manure heap stands the water-pump. This dangerous juxta-position of ingesta and excreta might not be of much consequence were regard had to the "dip" of the underlying strata (which in Scotland, at least, is seldom horizontal) in determining the safest hygienic position of the well and the "midden." But as such is not the case, it must frequently happen by chance that the manure is placed exactly so that the fluid which percolates through it will gravitate into the well. Doubtless, however, in some cases there may be a substratum of brick clay impervious to liquids, or a deep and porous subsoil which will have the effect for a time of oxidizing the organic matter during its passage through it, as in sewage irrigation, and thus to a trifling extent render the liquid innocuous. But this process cannot be permanent, as the best constructed filters or water-works in course of time have their interstices choked with filtered matter, and require renewal. Many must remember how, previous to the introduction of Loch Katrine water into Glasgow, the water then supplied was frequently muddled with a conglomeration of alluvial debris. Hence it is easily seen how the water in farm wells may get seriously contaminated with putrid organic matter. This is more likely to take place during a drought, as then the wells frequently get dry or nearly so, and the proportion of organic matter to the water must be much higher than when the well is full; excepting, perhaps, at the beginning of wet weather, which has been preceded by a drought, when the amount of decayed material washed into the soil is greatest. It is unnecessary to say more on this point, as all cases, and they are numerous, of tainted water supply are traceable to one cause, viz., the infiltration of organic matter, call it liquid manure, human excreta, or sewage, through some defect in the drains, or from a want of drains, or from the relative situations of the polluting and polluted substances.

A very important question here presents itself in reference to the origin of, at least, enteric fever—namely, whether in order to produce this fever, it is or is not necessary that there must be present the specific excrement of a previous case of enteric fever. That this fever is engendered by its own excremental pollution of air and drinking water is an established fact, but, as I have previously stated, many distinguished physicians are of opinion that it may arise *de novo*. Dr. Murchison cites numerous instances of sporadic and limited epidemic outbreaks of enteric fever, the spontaneous origin of which in sewer emanation, and in contaminated drinking water, was proved to a certainty—there being no proof of previous pollution with enteric excreta. "This," he says, "explains why fever arising from sewer emanation is always enteric and not typhus or relapsing fever; why enteric fever is endemic in many places and epidemic in circumscribed localities; why it attacks the rich as well as the poor; why it occurs in isolated country houses as well as in large towns; and why it is most prevalent in autumn and warm seasons." In the eighth annual report of the sanitary commissioners of the Government of India, recently issued by Dr. Bryden, the following occurs:—"What I have shown is, that the spontaneous origin of enteric fever is a fact; but that a zymotic element is generated when typhoid is so developed, and that every case of enteric fever should be looked on as a focus from which enteric fever may spread." Premising, then, that typhoid may be elaborated *de novo* in the manner stated, we see that the peculiar conditions favourable to its origin and evolution exist nently at the very source of a considerable portion of our milk supply, and that, independent of any cases which may occur in the farm-houses themselves. In such an event it is obvious that the danger of infection would be greatly aggravated from the presence of enteric excrement, which all admit contains the special poison in its most virulent form.

It is thus easy to see how the milk may get contaminated with enteric virus. Water from a tainted well may be added to the milk intentionally. But, from all we know of zymotic poisons, a small quantity of such water left in the vessels after washing may be sufficient to infect the milk. In Dr. Ballard's report on the outbreak of enteric fever at Islington—causing 167 cases with 25 deaths, and one at Leeds causing 107 cases with 11 deaths, both traced to the use of impure milk—he has clearly shown that the Leeds ontbreak was caused by tainted water mixed with the milk, because, when the handle of the pump which supplied the water was tied, the fever ceased. In the Islington outbreak the balance of evidence points to the milk having been infected by the small quantity of foul water left in the vessels after washing. Dr. Ballard says—"I confess that to my mind this is not an impossibility; we all know how small, almost infinitesimal, an admixture of sewage will poison a well or stream. Nor is the idea of reproduction of the typhoid contagium out of, within, or in presence of an appropriate organic material, at all foreign to the prevailing opinion upon the subject. Future experience may show that milk, which has remarkable relations to chemical ferments, is a substance peculiarly adapted also to the reproduction of morbid contagia, or to the contagia of typhoid in particular."

(2.) *By impure air.*—That milk, by exposure to air, suspending certain specific contagia, is capable, for a time at least, of preserving, probably of reproducing, whatever portions of the contagia may settle on its surface, either from the air, or from the bodies of infected persons, so that these may evolve their specific morbid phenomena in individuals swallowing the milk, seems to me sufficiently proven by the outbreak of enteric fever at Parkhead, by the second outbreak of enteric fever at Leeds, by the outbreak of enteric fever at Penrith in 1857, by the outbreak of scarlatina at St. Andrews, and also the one of scarlatina at Leeds. My excellent and esteemed friend, Dr. Russell, officer of health for the city, clearly demonstrated the foregoing in his report on the Parkhead outbreak. Many eminent authorities are of opinion that enteric fever is not infectious in the sense that small-pox is. Others hold that the alvine dejections constitute the chief, if not the sole medium of communication. Be that as it may, Dr. Russell has proven (I need not go into details, his report is so recent), that individuals may contract the fever who have not inhaled the emanations directly from enteric excreta,—who have not been exposed to the exhalations of an enteric patient's lungs or skin,—who have not swalled enteric water, nor respired enteric sewer gas, but, who have partaken of milk rendered zymotic by exposure in the vicinity of a typhoid patient. Of the Leeds outbreak, Dr. Robinson, the officer of health, gives, in his last annual report (1873), the following remarkable details which I have abridged:—"A farm-house in the country became infected with typhoid fever in September last. The head of the house died, and subsequently five members of the household suffered from the disease. Milk, from this infected source, is supplied by a dairyman to a respectable district of the town, where a virulent outbreak of typhoid takes place, and eighty persons who obtained their milk from this dairyman contracted fever, fourteen of whom died. No fever existed at the milk-vendor's house when the cause of the outbreak was found (though subsequently his daughter took typhoid). The farm is visited, the water is found blameless, being obtained from a source at a higher level than the house, and analysis showed it pure. In the house itself the sick were placed in a room communicating with the kitchen and dairy. Those engaged with the milk attended the sick. Unclean linen was observed placed side by side with the lids of the milk-cans. Outside, an immense fluid reeking mass of manure adjoined the stable where the cows were milked, on to the surface of which the drain from the kitchen sink and slope stone poured its contents. The common privy, which received the typhoid excreta, was overflowing and draining. Thus *innumerable particles of poison* must have been floating around the *house, and from their close contiguity*, permeating the cow stable, in which vicinity the milk remained till sent away." This, Dr. Robinson thinks, is sufficient to show not only the possibility, but the almost certain probability of the sad results which followed. It seems to me worthy of remark that Dr. Robinson found those who habitually took milk, or large quantities of cream in their tea or coffee, to suffer from the fever. This proves what I have before stated, that cream is a suitable nidus for zymotic poison, and also that the poison is not destroyed by the usual temperature of hot infusions of tea or coffee. I would further remark that it is highly probable the cause of the fever breaking out in the farm-house originated in the polluted conditon of the farm-yard.

It is, however, obvious that the mere proximity of milk to aerial contagia, or mixture with a small quantity of water impregnated with the poison, might be of little consequence, were it not that it constitutes a congenial soil for their multiplication. Admit this proposition, and the rest follows. Every medical practitioner knows, and can furnish ample proof, how inconceivably subtle contagious or infectious media are. I remember once visiting a young girl residing with her parents in the classic grounds of Ballochmyle. I found her suffering from scarlet-fever. Of all the spots on earth one would have thought this the last in which contagion should be lurking, and the question was, How did she get it? She got it thus :—Workmen were repairing the mansion-house. One had a boy whom I had recently attended for scarlet fever. This boy one day brought his father's dinner from Catrine, about half-a-mile distant. The girl and boy, previously acquainted, happened to meet. They amused themselves a short time on the lawn, and about a week after she took the fever, and gave it also to her brother.

If it be postulated that milk is a congenial zymotic soil, we know that, like all such soils, it will be sure to produce a very large crop from a very small quantity of seed. Hence our care should be to keep the seed from the soil, and the soil from the seed. Practically, however, this, though comparatively easy in farm-houses, must be a work of difficulty in large towns. In Glasgow, for example, as Dr. Russell has said, "It is very common for parties to live and rear families in rooms behind shops through which the sole access lies, and in which groceries, milk, provisions, fruits, sweetmeats, &c., are sold; and when infectious disease enter such a family it cannot fail to be the source of quite peculiar risk to the public." I need hardly say that I unhesitatingly endorse this statement, and may add that many of the articles sold in such places—as ham, cheese, fish, &c.—from their composition are likely to form suitable zymotic *habitats*, though in some cases the poison may be destroyed by subsequent cooking. At anyrate, it has been shown that milk—which, of course, requires no cooking—is favourable to the preservation, and doubtless also to the reproduction, of enteric, scarlatinal, and I think we may safely include choleraic contagia; such being the case it is extremely probable, in fact I should say certain, that it is also as favourable to the preservation and reproduction of the individual poisons of measles, small-pox, typhus, &c.

To isolate the specific principle of contagia synthetically or analytically, to study their natural history, their physical properties, their chemical affinities, to discover suitable preventives and sure antidotes, is a task which many have undertaken, but hitherto have almost entirely failed in accomplishing. Besides being of absorbing and unflagging interest to the hygienist, biologist, and pathologist, it is one of immense importance to the human race. In England alone the zymotica cause above 100,000 deaths annually, and though their excessively attenuated atoms have hitherto practically eluded the most refined methods of research, failing to be ensnared by the most subtle and delicately-contrived experiments, still I cannot but think that, with higher microscopical powers, greater facilities for original experimental investigation, increased and more exact knowledge of physiological and pathological chemistry, assisted by fresh discoveries in the various allied sciences, their total subjugation to the will of man is simply a question of time.—*Glasgow Medical Journal, May* 1873, *p*. 312.

15.—ON THE ORIGIN OF TYPHOID BY INFECTED MILK.

By the EDITOR OF THE LANCET.

In illustration of the milk theory of the origin of typhoid fever, Mr. Alfred Smee communicated to the *Times* some observations which, though not entirely novel, are of very great importance. Mr. Smee's London house is, it appears, supplied with milk, cream, and butter from some cows of his own at Wallington. For a short time in the spring these cows were fed on sewage grass, when the butter became so offensively rancid that the family could not bear it on the table. When the sewage was discontinued, "the cream, milk, and butter resumed their former excellence." The experiment was repeated without notice to the family, with the same result as before; and Mr. Smee declares that he has reason to suspect that the fact "is thoroughly well known to our dairy proprietors." Mr. Smee introduces his remarks with the succinct statement that he is "in a position to explain the occurrence of typhoid fever from the use of milk."

It will be familiar to many of our readers that fears have frequently been expressed of the effect of sewage grass on the lower animals, and so, indirectly, on man. The point has been considered by almost every one of the commissions and committees which have investigated the sewage question; and we believe we are correct in stating that in every case the result has been a conviction in the minds of the reporters that these fears were illusory, and that, practically, there was no more danger in applying sewage to grass than in applying the liquid manure of a farm-yard to a growing crop. We are by no means anxious to overrate these conclusions. The observations upon which they rest are not sufficiently numerous or exact to justify anything like dogmatism in regard to them. It is proverbially difficult to prove a negative, and the question is one of such vast importance that any statements bearing upon it are entitled to the most respectful consideration. But, nevertheless, the fact remains that to the present time no distinct evidence has been brought forward of evil arising from sewage irrigation; and when we consider that irrigation is pretty generally allowed to be on the whole the most satisfactory method for the disposal of sewage, we find ourselves compelled to ask for very distinct evidence of its danger before we venture to discard it.

The oldest and in some respects the most remarkable example of British sewage irrigation is afforded by the well known Craigentinny and other meadows near Edinburgh, where four or five good grass crops are yielded every year by land which was formerly barren sand, and where the system has been in partial use for about two hundred years. In these meadows the grass receives an enormous and excessive amount of sewage, complete purification being unneccessary, as the effluent water goes to the sea. The remark made in regard to them by the Rivers' Pollution Commissioners has therefore considerable force, and may well be quoted here: "If sewage irrigation had really bred a special class of diseases, it would hardly have been tolerated for two centuries on so vast a scale near Edinburgh."

Even more satisfactory, because more systematically and scientifically observed, are the large experiments on irrigation which have for years been conducted at Rugby and Croyden, those at Rugby having been framed and carried out by the scientific members of a Royal Commission. No grave objection was urged against these experiments by anyone. At Rugby cattle were fed extensively and continuously with the sewage grass, and the milk was regularly analysed for two years. No important difference was observed between the milk from sewaged and unsewaged grass, and it is scarcely possible that any could have existed, as it could hardly have escaped the notice of such experienced and impartial observers as Professor Way and Dr. Gilbert.

We will turn now to another point of evidence. Some time ago the Sewage Committee of the British Association commissioned Dr. Spencer Cobbold "to examine and report as to the presence of parasites in the flesh of a cow which was reared and fattened exclusively on the products of sewage irrigation by W. Hope, *Esq., V.C., of Breton's farm.*" Dr. Cobbold evidently held a strong feeling of the danger of sewage irrigation, but nevertheless he found "that the animal in question was entirely free from internal parasites of any kind." This observation, though on a different point to that now under discussion, has great value as showing that the cow, at any rate, is not injuriously affected by sewage grass.

Since the publication of Mr. Smee's letter, several letters from medical officers, managers of lunatic asylums, and others who have had experience in sewage irrigation, have appeared in the *Times*. They all agree that no harm is done to the milk by the use of sewage grass. The South Metropolitan School may be taken as an example. The population in these schools is sixteen hundred, and the daily consumption of water twenty thousand gallons, the whole of the sewage being applied to land. The cows eat the grass with relish; and Mr. Hales tells us that, after an experience of four years, he "can assert with much confidence that no evil has arisen from the use of this grass."

No doubt sewage irrigation requires great care and skill in its management; and it may be conceded that even in the most carefully regulated sewage farms noxious emanations cannot be entirely avoided.

Sewage irrigation is at the best an imperfect solution of the great problem, and the greatest praise that can be accorded to it is that it appears preferable to any of the schemes which have been proposed in its place. Perhaps the best statement of the objection to it is to be found in a recent report by Herr Lefeldt, a civil engineer who was appointed by the Prussian Minister of Agriculture to investigate the various English systems for the disposal of sewage. A remark which he makes with regard to the grass of the Edinburgh meadows may be quoted as lending some support to the theory adopted by Mr. Smee. He says: "The capillary tubes for some inches above the roots are found to be filled with unassimilated fæcal matters;" and adds that, if the irrigation is continued till within two days of the mowing, the cattle will not eat the grass. Here clearly is a source of danger; but it must not be forgotten that in these meadows the sewage is not so much applied as misapplied to the grass.

In conclusion we must remark that, while the evidence of the sanitary safety of sewage irrigation is less perfect than it should be, we decline to throw it all on one side for the sake of the still more imperfect, and, as far as we know, isolated evidence adduced by Mr. Smee.—*Lancet, Aug.* 23, 1873, *p.* 271.

16.—AFFECTABLE AND INFECTABLE SUSCEPTIBILITY OF MILK.

By Dr. H. Willington.

[Some years ago the family of the writer of this paper suffered from an unaccountable series of gastric attacks, sometimes typhoid, and once attended with malignant thrush. The notes made at the time have unfortunately been lost.]

It is something like fourteen years since my attention was painfully called to the putrid element to be met with too often in London milk. My own senses of smell and taste are, unfortunately, morbidly great for my comfort. To this I attributed the puking sensation arising instantly I smelt some London milk. In consequence of this, and of other members of my family not perceiving the mawkish sickening odour I did, I willingly attributed my revulsions of taste and smell to my own morbidness, until on one occasion of meeting, in the mawkish odour, with a distinct evidence of putrescent odour, I was induced to more carefully inquire into the facts—firstly, as to the cleanliness on the part of our servants and of the jugs used, and then as to the condition of the milk as it came from the "milkman." The milkman delivered the milk possessed with the odour of putrescence, the milk itself presenting no unnatural look, yielding also a fair result to the lactometer. The milkman had the milk from his own cows, pent in a small crowded shed. I accused them of possible scrofula, and *ceased to* have their milk. My family were sent to the seaside and recovered *health, the incidence* of causation of their gastric attacks and fevers being *forgotten or given up.*

Some time afterwards I was called to the house of the milk-vendor to a case of sudden illness, from faintness and gastric pains, so far as I now recollect. The odour of the house was such as follows a day after a cesspool has been emptied. I complained, "It was quite poisonous," to which was answered, "It was dreadful—from the drains; but the landlord would do nothing, and they could not. They were sure it was not from want of washing, &c." This seemed correct, if appearances might judge the case. I was struck by the appearance of eight or nine open pans of milk standing on chairs; so mawkish the odour that I could not tolerate it. The sink was open. Their water-supply from the waterworks. The house was drained into a cesspool to which the sink had no alternative but go. The drain through the house, as was subsequently proved, was choked—eighteen feet full length—and was virtually eighteen feet of superficial cesspool. I arrived at the conclusion that the milk was affected by the odorous poison of the atmosphere, wherein lay the cause of the putrescent odour of the milk that had entered my nursery when I was supplied by this dairy.

The short street this house was in was proverbial for fevers, and every epidemic that showed up in season. The drainage of it, after much trouble, was improved, fevers ceased in great measure, and my inquiry into the milk as a disseminator ceased after I had done the best I was able in ascertaining the affectability of milk—under given circumstances of temperature—by putrescent odours.

A little was exposed in a saucer all night near a sink that had the trap removed. In the morning an odour mawkish and unnatural to milk was discovered by my sense of smell, but others thought it was my fancy.

The late outbreak of fever through infected milk has aroused the slumbering theory—Milk is an affectable body, and, being an animal substance or product, is essentially susceptible of affection for putrefying causes, like as veal is, and especially so when there is a thundery atmosphere. That milk is attractive of electrical conditions, or, if it be preferred, is susceptible to them, is too patent to need the assertion they are so; and so is veal—a meat fed on milk—remarkably so, and to the extent of utter decomposition of the fluids of the meat in a few hours, causing an attraction to flies that collects them from all parts of the place. Taking all these things into account, it has appeared to me a most feasible conclusion: Milk is affectable by atmospheric poison, and is capable of incubating the worst morbid principle, and of becoming thereby an infectant of those who drink it.

It requires much experiment to prove putrid water, unless it has had such time as never is possible between milking the milk and the sale and use of it. Was it the infectious element *per se* that affected the milk which affected the Marylebone families?

The temperature of newly-drawn milk, given certain atmospheric conditions and sewage odour, favours its affectability by a tainted atmosphere, especially if the latter come up in volume from an open sink into the dairy in the night time.

If zymotic causes follow currents and deposits, where susceptibility is suitable, in tanks of water, why not on open dairy milk-pans of milk, if the dairy be close to open cesspools the holes and depressions are made into in every farm-yard?

Farmers for the most part, if they live long, are windy or pot-bellied—a fact I have long since attributed to gastric poisoning by the open cesspool system of their premises. Again, when the hot season comes the supply of water is reduced, and cows are driven to drink from ponds that are little less than sewage or bilge-water reservoirs.—*Lancet, August* 23, 1873, *p.* 284.

17.—THE ELECTROLYTIC TREATMENT OF CANCER.

By M. Neftel.

This paper contains the description of a mode of treatment which, if experience confirms the results stated here, must be considered one of the greatest boons which have been conferred on suffering humanity. It is no less than a cure for cancer. The paper begins with a discussion as to the nature of cancer. Following the school in which he was educated, the author at first looked on malignant disease as primarily a constitutional affection, of which the tumour was merely the local manifestation. He has been, however, convinced by the arguments of Virchow, that cancer is really at first a purely local disease, and that any generalisation which may occur is a secondary result of the infection of the system from the local disease. He treats of the traumatic origin of cancer, and gives two cases in which mammary scirrhus was distinctly traced to the effects of a blow. Coming to treatment, excision is generally of little or no advantage. He has found electrolysis alone useful. Before giving the results of his treatment and the method, he discusses its *modus operandi;* and states it as his belief that the action is not that of a caustic; but that the electric stimulation has some influence on the protoplasm of the cancer-cells, causing them to alter their mode of life. He believes in Waldeyer's views as to the development of cancer, namely, that it always originates from epithelium, and does not distinguish clinically between carcinoma and cancroid. The first case in which he tried the treatment is given in detail, and is of considerable interest. A gentleman had a tumour of the left mamma, with secondary affection of the axillary glands. It was diagnosed as cancer by Nelaton, Moore, Marion Sims, &c., and was finally extirpated. A similar tumour appeared on the right breast; while the glandular enlargements in the left axilla recurred, and the patient became markedly cachectic. This tumour in the right breast, as well as the axillary glands, were treated in the manner to be afterwards described; and the result was that the tumour completely disappeared, and the cachexia was entirely recovered from. A variety of other cases are given, all showing how speedily many cancers can be made to disappear, and especially how, after the first electrolysis, the lancinating pain passes off. In accordance with Virchow's views, he believes that there is an initial stage in which cancer is purely local, and in this stage he asserts that it can be completely cured by electrolysis. When the disease has advanced beyond this, the cure is difficult, and its success depends on whether all the affected parts can be reached. When the tumour is adherent to the deeper parts, or when distant organs are secondarily affected, the treatment is generally unsuccessful. Even in the most aggravated cases, however, this is the best means of reducing the pain, and improving the general condition of the patient. The method of applying the agent is described with great care. Suppose, in the first place, that the tumour is of moderate size, hard, and slow of growth. With or without chloroform, as circumstances may determine, two, three, or four needles are introduced at equal distances into the tumour, and connected with the negative pole or cathode of the battery. The positive pole or anode is connected with a broad plate, which is applied with a wet compress to the skin at a certain distance from the tumour, and moved from place to place. The current is gradually increased up to thirty-five or forty elements, and after twenty to thirty minutes, gradually lessened, and needle after needle withdrawn. This operation is repeated for a limited number of times at intervals of three or four days, the needles being introduced at different parts of the tumour. After this energetic treatment, a weak current of four to eight elements is to be used daily for months. The negative pole, formed of a flat plate and a wet compress, is placed over the needle punctures, and the positive at a distance from the tumour as before. The sloughs produced by the needles come away after a time, and their track serves as a conductor for the current. If, in the second place, the tumour be a large one, then the only modification is that, during the operation, needles should be extracted and placed in different situations. The number of the needles should not, however, be increased above four at one time. If, in the third place, the tumour be large and ulcerating, then an attempt is made to produce sloughing of it. In this case, the node (or a positive) needle is thrust into the centre of the tumour, and its base transfixed with a number of needles connected with the negative pole. These are left for about twenty minutes, and then inserted in a different position. In from seven to ten days the whole or part of the tumour generally sloughs away, and if it is not entirely removed, the operation may be repeated. After this rather severe treatment, there is never any fever or special constitutional disturbance. In this case, also, after the tumour has sloughed, the treatment with weak currents should be continued for months. Of late, the author has sometimes introduced a slight difference in the treatment of the first and second class of cases. Instead of inserting cathode needles throughout, he has occasionally begun with an anode needle, which he has only kept in for a short time, and he thinks this an improvement on the original plan. The anode needle should only be kept in for a short time, as otherwise it is apt to cause sloughing; after this single application of the positive needle, he continues as before. It is to be noted that the author attaches great importance to the efficient carrying out of the treatment with weak currents, after the energetic currents have been used preliminarily; and he generally entrusts this latter treatment to the patient. The author has also used a similar mode of treatment, as others have done, in aneurisms, varix, &c. His method is in this respect different from the usual, that he does not insulate the stems of the needles which he introduces. —*Edinburgh Medical Journal, Oct.* 1873, *p.* 381.

18.—THE CO-EXISTENCE OF CARBUNCLE AND GOUT.

By Dr. J. M. Winn, London.

[A correspondent of the Lancet (Dr. Ellery) having asked whether any member of the profession had ever noticed a connexion between gout and carbuncle, Dr. Winn in reply refers him to an essay on Hereditary Diseases, which he published in 1869, and in which he states in reference to the correlation of force in disease :]

For many years I have been struck with the frequency of the occurrence of carbuncle in individuals of a gouty diathesis. The worst case of carbuncle I ever met with was that of an individual who had suffered from repeated attacks of podagra. The usual symptoms of gout had, however, been for some time in abeyance before the commencement of his fatal illness; and the carbuncular disease of which he died was evidently the vicarious action of the same subtle morbific force as that which had previously caused his suffering from gout.—*Lancet, May* 3, 1873, *p.* 653.

[There is another connexion which is perhaps more interesting than this, and is not, we believe, known to the profession—viz., that in carbuncle there is generally temporary diabetes.—Eds.]

19.—ON THE INTERNAL USE OF CHLORALUM IN CHOLERA.

By Dr. Henry Blanc, F.R.G.S., Surgeon H.M. Indian Army.

During the months of May, June, and July, 1872, cholera was raging all over the Mahratta country (the plateau above the Western Ghauts, Bombay Presidency), and at Sattara, where I was acting civil surgeon. I had occasion to see a few cases. We did not have many, but the cholera cannot be blamed for having treated us so leniently, and doubtless we would have numbered the victims of the epidemic by hundreds, instead of by tens, had I not been most cordially assisted by the civil officers in charge of the district. Thanks to them, the sanitary measures I recommended were carried out most vigorously, and the consequence was that cholera was stamped out before it could get a footing; moreover, we succeeded in protecting entirely the camp from its influence, although the military station is situate less than a mile from the town of Sattara. I do not intend recording here the hygienic measures which saved us from the heavy death-bill, because if they are not more generally applied, they are, nevertheless, perfectly known. I wish merely to say a few words on the treatment I adopted. The type of the disease was most virulent. Generally no premonitory symptoms, hardly any vomiting or purging; persons in perfect health, after vomiting once or twice, were at once seized with rapid and deep collapse.

I tried every posible remedy, from the orthodox cholera mixture and pills to the injection of liquor ammoniæ in the veins. The hypodermic injection of morphia, recommended by a writer in the Lancet as having given good results at Constantinople during the epidemic that prevailed there, totally failed. Salt-and-water enemas and exhilarant stimulants were also unsuccessful.

"*En desespoir de cause*," I thought of destroying the poison in the body itself by means of some powerful disinfectant, believing that if this were possible, we ought in many cases apparently hopeless, to save life, and once the cause removed, by merely directing and assisting nature, obtain a good percentage of successes. I selected chloralum—the chloride of aluminium—not only on account of its freedom from smell and taste, but also because it could not, diluted with water, do harm if it did no good.

I made a mixture of one part of liquid chloralum to twenty-five of water, and directed that an ounce of the mixture be given every half hour, and that an enema, containing sixteen ounces of the mixture, be administered every hour. Undiluted chloralum was to be sprinkled frequently on and around the bed. In the two successful cases the remedy was given less and less frequently as the reaction became more completely established.

I can only regret that I did not think of the chloralum mixture sooner, as it is difficult to judge of the value of a remedy on such a limited number of cases. Still it should be remembered that it was employed in *very desperate cases*, quite as unfavourable as those previously treated by various other means, and which had all of them ended fatally. Theoretically we have a better chance of success with an antidote than by the employment of drugs, which are directed merely against the symptoms of the disease. We know that opium and alcoholic stimulants do much harm, and as for purgatives—castor oil or calomel—they are of more use in the druggist's bottle than in the stomach of an individual rapidly sinking under the deep collapse of a severe attack of cholera. I am far from asserting that we have in chloralum "the remedy" for cholera; still, should this dreadful scourge visit the shores of England, there can be no objection, in presence of the cases here recorded, to give the remedy a fair trial.

[A tabular summary of three cases treated is given.]

The second case given above, and which was followed by death, should not be considered as an unsuccess. The patient refused in my presence to take any European medicine, and it was only after much coaxing that he swallowed a dose. Doubtless, after my departure, he was allowed by the native medical subordinates to have his own way. In order to ascertain the real value of chloralum in cholera, it should be given before other medicines have been administered. In cholera we have but too often to contend against two evils, the disease itself, and the previous indiscriminate use of a host of remedies, most of them, if not all, adding their noxious influences to the many difficulties we have already to contend with.—*Lancet, Aug.* 16, 1873, *p.* 226.

DISEASES OF THE NERVOUS SYSTEM.

20.—ARE MORBID FUNCTIONAL STATES OF THE CEREBELLUM, MEDULLA OBLONGATA, AND SPINAL CORD, CAUSES OF ALBUMINURIA AND BRIGHT'S DISEASE?

By Dr. THOMAS LAYCOCK, Professor of the Practice of Medicine and of Clinical Medicine in the University of Edinburgh.

A year ago an interesting polemic arose in the Royal Medico-Chirurgical Society as to the nature and causes of certain pathological changes in the minute arteries of the body in general, and in the kidneys in especial, found in cases of chronic Bright's disease. Differences of opinion were expressed by eminent physicians whose views will be always welcomed by the profession, both as to the facts and the theories involved. A committee of the society was appointed to judge between them, but it resigned office, and the whole matter is still undetermined.

Looking at the questions in dispute as a spectator anxious to derive useful knowledge from the very vivid discussions at the society, and in the lectures subsequently published, to elucidate and round it off, I find the controversialists agreed as to this general fact of observation—that there is a change in the structure of the smaller arteries throughout the body generally, as well as in the kidneys, in a certain class of cases of Bright's disease. The difference in opinion was as to the nature of the change. Sir William Gull and Dr. Sutton reported it to be a hyaline-fibroid or fibroid thickening of the arterial wall, whereas Dr. Johnson held that it was a muscular hypertrophy. The obvious method of solution of this part of the question is that each observer should prepare demonstrations from the same subject, for if they be from different subjects, it is quite probable that in the one may be hyaline-fibroid and in the other hypertrophic. In the absence of this procedure, it is fair to conclude at least that both observers may be correct.

But the polemic extended far beyond the anatomical facts to the causes of the changes observed. Dr. Geo. Johnson is an earnest humoral pathologist, and affirms that Bright's disease is not a mere local malady, and that "the proximate cause is in all probability a morbid condition of the blood." In proof, he observes that the disease is bilateral, and that it "occurs in association with morbid states in which a morbid condition of the blood may be confidently assumed to exist. Albuminuria, varying in degree and in duration, has been found more or less frequently associated with scarlet fever, measles, small-pox, erysipelas, pyæmia, typhus and typhoid fever, rheumatic fever, malarious fevers, cholera, purpura, scurvy, diabetes, syphilis, certain forms of pneumonia, pregnancy, the absorption of secretions from the interior of the uterus after parturition, gout, the abuse of alcoholic liquors, excessive eating, certain forms of dyspepsia—resulting, as may be supposed, in the passage of crude materials into the circulation,—a poor and insufficient diet, purulent and other discharges, and, lastly, suppressed action of the skin by exposure to cold, and especially to cold and wet combined.

It will be observed that in this copious exposition of causation, albumen in the urine, or "albuminuria," is taken as identical with, or necessarily leading to, Bright's disease. Further, that while in all these varied diseases the nervous system as well as the blood must be involved—in some more primarily than in others—it is wholly ignored in causation. And exciting causes of albuminuria traceable directly to the nervous system—viz., that kind following upon excessive brain work, and due to lesions of the medulla oblongata and spinal cord—are omitted. In short, I do not find in Dr. Johnson's papers any reference whatever to any morbid state of the nervous system in general, or that of the kidneys in special, as an antecedent to the morbid changes in the kidneys or to those of the blood, which are supposed, "in all probability, to be their cause."

In reference to this omission of an important element in causation, I may also refer to the recent discussion as to influence of "alcoholic" drinks on the production of Bright's disease. Neither Dr. Johnson nor the other disputants seem to realise the fact that the drinks of the drunkard are not all "alcoholic" in a definite meaning of the term, but contain other agents of a poisonous or injurious character, as fusel oil, bitters, &c., and that these have all a direct and primary action on the nervous system, and so their use leads to morbid conditions thereof which may be predisposing causes. Lead is another of these neurotic causes. What Dr. Johnson dwells upon is the series of diseases of the nervous system which coincide with, or are consecutive to, chronic Bright's disease, but which, according to his humoral pathology, also depend on a poisoned state of the blood, poisoned by the retention of urinary excreta, and known by the term of "uræmia." Yet he states a well-known clinical fact, which is of itself sufficient to upset the current theories of uræmic poisoning. "Let me add," he observes in one of his lectures, "that in some cases, notwithstanding the scantiness, and ultimately the almost complete suppression, of urine, uræmic symptoms are almost entirely wanting."

But how, according to the humoral theory, is the arterial degeneration or hypertrophy of the small arteries generally caused? and how the coincident hypertrophy of the heart, noticed by Bright? "About six years ago it occurred to me," Dr. Johnson observes, "that the hypertrophy of the left ventricle of the heart in cases of contracted kidney might be a result of increased contraction of the small arteries throughout the body, this contraction being excited by the abnormal quality of the blood." And this kind of arterial change is also the cause of the neuroses in certain cases of Bright's disease—excessive contraction of the arteries of nerve-centres, "excited by the pressure of impure blood," leading to arrest of supply of blood to the nerve-tissue. Here, again, it is observable how entirely the vaso-motor nervous system is ignored. Sir William Gull differs wholly, and I think rightly, from Dr. Johnson in respect to his humoral pathology.

For many years past I have directed attention to the influential part which the nervous system plays in all these tissue changes whatever, showing the facts both systematically in my college course and at the bedside. I have drawn the conclusion from a large and varied series of facts, that the cerebellum, with its connexion downwards along the medulla oblongata and spinal cord and upwards into the hemispheres, is the great trophic centre; and that this centre is put into functional activity, not only by afferent impressions from the body, whether consciousness be coincident or not, but also by afferent impressions from the vast periphery of the hemispheres, and in this case also whether consciousness be coincident or not;—in short, that the hemispheres are not functionally restricted to intellectual processes, as is generally held, nor the cerebellum to muscular co-ordination, but that both conjointly influence the whole of the organic processes in a way which physiologists and pathologists have, so far as I know, wholly overlooked. It is in accordance with these views that I regard the changes in the vascular system in general described by Dr. Johnson as due to concauses having their seat in the cerebellum, medulla oblongata, and spinal cord; that a special neurosis of the renal nervous system is a concause in all cases of albuminuria and consecutive structural changes in the kidneys; and that the dropsies, thirst, cardiac, vascular, lymphatic, pulmonary, gastric, and other phenomena, must be reconsidered from this point of view to understand in any reasonable degree the true pathology and the prevention and cure of this large and common group of diseases. Hence I have put for consideration the question at the head of this note—Are morbid functional states of the cerebellum, medulla oblongata, and spinal cord causes, predisposing and exciting, of albuminuria and Bright's disease?—*Med. Times and Gazette, July 12, 1873, p. 30.*

21.—ON PARALYSIS, CONVULSION, AND OTHER NERVOUS AFFECTIONS IN SYPHILITIC SUBJECTS.

By Dr. Thomas Buzzard, Physician to the National Hospital for the Paralysed and Epileptic.

Ricord's views as to the most appropriate treatment for the different stages of constitutional syphilis may be thus epitomised. In the primary stage, mercury. For secondary symptoms, when these are uncomplicated, mercury; when they are blended with those of a tertiary character, the treatment is a mixture of mercury and iodide of potassium. In the tertiary stage he relies upon iodide of potassium. And this scheme would probably represent very fairly the views of those who have had large experience of the disease. In the class of cases, however, with which we are specially concerned, as I have before described, it is not by any means always possible to say whether the symptoms of nervous affection which are observed belong to the secondary or tertiary stage. Our treatment therefore must be to a certain extent tentative. My own plan is always to commence, in a case of this kind, with iodide of potassium. In a majority of instances this drug, which will often require to be pushed to large doses, will be sufficient to bring about a cure of the symptoms, or at least a very important improvement. If, however, after three or four weeks of such treatment, employed with sufficient boldness, the improvement is not marked, I give mercury, which will sometimes then act with remarkable effect. The preparations which I chiefly employ are, the perchloride and the red iodide; and I do not know that preference, for any particular reason, is to be given to one or other of these forms. As a matter of practice, I think I have found that the red iodide, given in solution of iodide of potassium, is less apt to cause irritation of the abdominal organs than the perchloride. It is very seldom that either causes salivation. Where it is desired to produce, and to keep up for some time, a slight action upon the gums, this can be effected in the usual manner by appropriate doses of blue pill or grey powder. The French very largely employ the green iodide, a preparation but seldom used in England, probably because it is unstable, some biniodide being apt to form in it when it has been long kept.

A word as to the dose of iodide of potassium. After a good deal of hesitation, and trial of various quantities in a considerable number of cases, I feel convinced that in syphilitic affections of the nervous system it is often necessary to employ doses of this drug which are far beyond those usually ordered. In several instances I have observed something of the following kind to take place. An improvement up to a certain point has been produced by doses of iodide of from ten or fifteen to twenty grains three times daily. The patient has then remained at this stage, or progress has been very slow, whilst continuing to take this amount. On increasing it, however, by rapid steps to thirty, forty, sixty, or even ninety grains three times a day, the case has responded immediately and *pari passu* to the additional quantity of the drug. There are two patients in the hospital at the present time who are striking instances of this effect. In each the dose was pushed gradually to ninety grains three times a day, with marked beneficial effect, and I may add that the patients themselves express an unhesitating opinion upon this point. There is nothing new of course in the employment of large doses. Forty years ago the late Dr. Elliotson used to give as much as two drachms three times a day, and with remarkably good results. But of late years, as a general rule, the dose has been so moderate that to many practitioners the employment of ten grains at a time is only gradually arrived at, and with some caution. No doubt in a very large number of cases a comparatively small dose is all that is required, and in practice, therefore, it is well to begin with a dose of ten grains, and increase it if necessary. I feel tolerably sure from repeated experiments that the iodide may be used, if occasion require it, as freely as the bromide of potassium, and that the opportunity of doing great good in syphilitic nervous affections is nearly as often missed by the employment of inadequate doses of the former drug as used notoriously to happen in respect to epilepsy from the exhibition of too small doses of the latter. Symptoms of iodism, very like those of severe nasal catarrh, will occasionally present themselves, but just as frequently, so far as I have seen, when the amount of the salt employed is small as when it is considerable, and it is very rare indeed that the use of the drug has to be given up on this account. Occasionally, too, a kind of large pustular acne is a disagreeable consequence of its administration, but this is not so common as where the bromide has been given.

Whether iodide of potassium, or mercury, or both be employed, a very long continuance of the treatment seems always to be required, and an immediate resumption of it at the slightest indication of relapse. In certain cases, arsenic and iron may be used with advantage in addition to these means, bromide of potassium may be added in cases of convulsion, and electricity may be employed to help the nutrition of wasted muscles; but practically the treatment of this class of nervous affections is the treatment of the constitutional disorder.—*Lancet, May* 17, 1873, *p.* 693.

22.—NOTE ON THE PARALYSING ACTION OF ACONITE ON THE SYMPATHETIC NERVE.

By Dr. FREDERICK BAGSHAWE, M.A., Assistant-Surgeon to the East Sussex and Hastings Infirmary.

The topical action of aconite was displayed in the following case in so marked a manner as to be worth recording.

A young lady, aged nineteen, came under my care in November 1872, who had long been suffering from spinal irritation and neuralgia. During the previous summer she had passed some time at St. Moritz with benefit, and could then walk four or five miles. A recurrence of severe pain, sometimes in the spine, and sometimes in the face, head, and neck, had induced her to seek further change at Hastings. On the 3rd of November, having recovered from an attack of spinal pain, the patient took a short walk while a north-east wind was blowing, and came home with severe pains in the head and neck, chiefly on the left side. The effect of the subcutaneous injection of ¼ grain of morphia was to induce sickness, while it failed to relieve pain or to procure sleep. Liniment of chloroform and belladonna was applied, but she passed a disturbed night, and next day was suffering much pain along the course of the left occipito-cervical and trigeminal nerves. I applied the liniment of aconite (B. P.) behind the left ear and down the neck, carefully avoiding a small place which had been blistered by the chloroform liniment. Some relief was obtained, and the liniment was again painted on twice that day (5th Nov.), and produced some numbness and tingling of the lips. On the morning of the 6th more liniment was applied, as this was found to be "the only thing that drove away the pain." At 9 a.m. the eyes and nose felt sore and tingling "as if she had a cold in the head;" at 1 p.m. burning soreness of the eyes was increasing, the skin of the neck was numb, and the lips tingled much; at 3 p.m. she was screening her eyes from the light, but rejoicing in being free from the neuralgia. The left eyelid was much swollen. The vessels of the ocular conjunctiva were enlarged, bulging, and tortuous, and of a bright pink hue. The forehead, face, ear, and neck were hot to the touch, while the right side was cool. The pupil was widely dilated, and vision was greatly impaired, so that she was unable to read good letterpress, but could read large print of a title-page.

The lips were dry. The tongue was projected somewhat to the affected side. This was tried twice. There were no general symptoms, no weakness. The pulse was 88. At 6 p.m. pain in the eyes and intolerance of light were still greater, but she was free from neuralgia and perfectly well, the eyes excepted. The pupil being widely dilated, I examined with the ophthalmoscope, and saw the arteria centralis distended and the retina generally congested. Sensibility, tested by pricking, was greatly impaired over the whole side of the neck and face to which the aconite had been applied.

The symptoms abated at 9 p.m., and next morning (11 a.m.) the congestion *of the eye had almost* subsided, slight redness and swelling only remaining.

The left pupil was still larger than the right; both contracted well to the stimulus of light. There was no return of neuralgia.

It is not uncommon to meet with less marked cases of the effects produced by the topical use of aconite, thus :—

Mr. W. S., aged 50, suffering from facial neuralgia, rubbed aconite on the cheek-bone. It caused numbness and "twittering" sensation of the part, which lasted some hours, while the pain was dulled. There was sensible increase of warmth of the muscles of the face. The eye became weak and watering, while the lid drooped. The state of the pupil was not noted. This condition lasted about twenty-four hours.

Mrs. W. D., aged 57, used aconite liniment with temporary relief to her neuralgia, and with increased warmth to the facial muscles and manifest congestion of the conjunctival vessels.

These and similar cases point to the fact that aconite applied to the skin in the neighbourhood of the cervical ganglia and branches of the sympathetic, produces consequences similar to those brought about by section of the nerve in that situation in animals. In both cases a swollen and congested state of the conjunctival vessels and abnormal warmth of the part is the result.

In both the threefold office ascribed to the sympathetic is interfered with: (1) sensation is blunted in the parts supplied by the nerve; (2) nervous control over the blood-vessels is withdrawn; (3) motive power is reduced.

Aconite thus becomes a powerful local remedy in our hands, not only as a controller of morbid sensation, but as a retarder of vascular action, and by consequence as a modifier of muscular action.

But although generally a painless remedy, is it a safe and innocuous one? Happily, it appears that the small quantities of the drug capable of being absorbed through the skin, attack the nerves and muscles directly in their neighbourhood, and show little disposition to be absorbed into the general current of the circulation, and so paralyse the great central circulating organ. In the above detailed case the local action of the remedy was powerfully exerted, but neither stomach, kidneys, or heart seemed to sympathise. None the less, however, is caution needed in the use of so powerful an agent, lest its action should pass on to the cardiac nerves and vessels, and so produce grave symptoms.

The following case of general aconite poisoning illustrates some of these symptoms, and shows at the same time how large an internal dose of aconite may be recovered from. In December 1871 Mrs. G. was attending the Hastings Infirmary as out-patient, suffering from pain in the shoulder, and was ordered a liniment composed equal parts lin. opii and lin. aconiti (B. P.): she was duly cautioned as to the poisonous nature of the application. On January 5 she drank off about six drachms of the liniment, *i.e.* three drachms of lin. aconiti. Immediately afterwards she felt a burning sensation in the throat and tongue; in about ten minutes she was unable to speak, and is said to have made a stertorous noise in breathing; she suffered from a strangling sensation in the throat. The face was partly pallid, partly flushed (over the cheek-bone). A neighbouring chemist who was called in encouraged vomiting, which had already set in, with mustard emetics, followed by ʒ ss. of sulphate of zinc: he did not yet know what poison had been taken. I saw the patient an hour and a half after the dose had been taken; she was then agitated and weak, with a pulse of 84 and a cool skin; her only complaint was of burning at the throat and stomach, and weakness. *The pupils were larger than natural.* She took some brandy and raw egg, and a stimulating draught from time to time. Next day she felt faint and low, and some amount of burning sensation in the throat and stomach continued; the face was flushed, the pulse 80 and good; the urine remained normal, and much milk was secreted. On the 7th she appeared to be well.

In cases of fatal aconite poisoning the symptoms seem to have followed this order :—

1. Tingling and numbness of the tongue and throat, soon extending to the stomach, with local sensations of heat.
2. *Failure of the circulation*, evidenced by cold extremities, cold sweats, great *feebleness*, &c.

3. Pupils dilated and sight dimmed.
4. Final syncope.

[As a pendant to Dr. Bagshawe's cases of the local action of aconite upon the vaso-motor nerves, I beg to mention the following, which occurred some months ago in the out-patient room:—E. M., a small and thin woman, aged 29, married, but has never had children; she had had threatenings of phthisis, and had also suffered intercostal neuralgia. Just after Dr. Bagshawe had informed me, verbally, of his first case, this woman applied one day at the hospital, suffering from severe pain in the left trigeminus (first and second division), which was evidently excited by a very bad carious tooth. She would not have the tooth extracted, and, as an experiment upon the powers of aconite, I ordered the lin. aconiti to be painted over the painful parts. The effect in arresting pain was immediate and complete, but on the next hospital day she came complaining of "erysipelas." The parts which had been painted were swollen and bright red; moreover the conjunctiva of that side was severely congested, with *tortuous vessels*. The pupil in this case was contracted rather than dilated; and this leads me to notice the curious fact that in Dr. Bagshawe's cases the pupils were dilated, in his first case to a remarkable extent. I need not say that, while the rest of the phemomena agree well enough with the idea of sympathetic paralysis, dilatation of the pupil is exactly the reverse of the condition of iris produced in Bernard's and Brown-Séquard's experiments.—F. E. ANSTIE.]—*Practitioner, July* 1873, *p.* 24.

23.—ON THE ANATOMICAL INVESTIGATION OF EPILEPSY AND EPILEPTIFORM CONVULSIONS.

By Dr. J. HUGHLINGS JACKSON, Physician to the Hospital for the Epileptic and Paralysed, and to the London Hospital.

[In this paper Dr. Jackson pursues an anatomical line of inquiry with the intention of showing the bearings of Ferrier's researches on the methodical study of the seat of the lesion in epilepsies. There are certain principles in the constitution of the nervous centres in man.]

The nervous centres represent movements, not muscles; chords, not notes. This is evident from the effects of destroying lesions of the corpus striatum. From a small lesion of this body there does not result paralysis of a small part of the arm, nor of any such group of muscles as flexors, or extensors; there results partial paralysis of the whole arm, the most special parts of it suffering most. There is loss of a certain number of movements of the limb. Let us take a more striking example: in cases of very grave lesion of the corpus striatum (that is, of a centre far above the supposed deep origins of the ocular motor-nerves), there is, besides palsy of the face, arm, and leg, an ocular palsy. Now this palsy is not of the sixth nerve, nor of the third nerve, nor of the fourth, nor of any one muscle, nor of any random grouping of muscles. It is a loss of a highly special and widely associated movement; the patient has lost power to look to that side on which his body is paralysed; there is what is commonly called lateral deviation of the eyes. Similarly, in convulsion there is a development of movements. In a convulsion beginning in the hand, the spasm creeps up the whole limb, developing first the movements of the most special parts of it, but not picking out such groups of muscles as flexors or extensors. Among other movements, there is at a certain stage a development of that of the eyes for "looking" to one side. In this case the two eyes are turned to the side of the body convulsed. We must, however, draw attention to a very important qualification with which the expression "development of movements" is to be used.

Both in Dr. Ferrier's experiments and in cases of convulsion from disease in man, the results of the discharges (since they are sudden, excessive, and very local) are only exhibitions of the movements represented in the parts discharged in the rough. A great number of different movements are developed at once.

And it must not be forgotten that not only are the discharges unnatural in being excessive, sudden, and temporary, but also in that they are very local. We are reminded of the effects of putting one muscle into strong action by Faradising it; the result is a mere caricature of a normal movement. Duchenne insists that a muscle is never singly in action in health, except perhaps in the case of the facial muscles.

But in spite of these drawbacks, the study of discharging lesions, whether induced by Faradisation or by disease, is of great value. My own opinion is that there is no other way of finding out what movements parts of the convolutions near to the corpus striatum represent. The reason for thinking so is that the other process of experimentation—that by destruction of small parts of the cerebral hemisphere—produces no obvious symptoms—no obvious loss of movements, or no special loss at any rate. But it would be a great error to infer that the part destroyed did not represent special movements. The bearing of Ferrier's researches is very direct on this matter. For if we discharge that part, destruction of which produces no loss of movement, there will be a presentation of a mass of movements. See, then, the clearing of the paradox. Disease of the convolutions sometimes does, and sometimes does not, produce symptoms. The word disease is used vaguely; so far as it involves destruction there are no symptoms, but there are symptoms from discharge. The speculation I have put forward to explain these paradoxical results is as follows:—

To begin with a "motor" centre. The study of cases of hemiplegia shows the constitution of the corpus striatum to be such, that each part of it represents movements of the whole of the parts which that organ governs. So to speak, the corpus striatum is a mass of corpora striata, each one of which represents faintly, and each in some slightly different manner, the whole of the parts which the corpus striatum in full represents in greater degree. Now, to pass to the hemisphere. The convolutions in the region of the corpus striatum are the corpus striatum "raised to a higher power." Each part of the brain in this region re-represents the whole of the movements which have been represented in the corpus striatum; so then, if any one part of the brain in this region be destroyed, there is no obvious loss of movements, because the movements it represented are still represented in each neighbouring part, although in different degrees and orders. But for this very reason, if any one part be strongly discharged, vast numbers of movements are developed.

I now mention illustrative cases. Although I can only take enough space to give a mere outline of the cases, I think they show plainly that some of the "experiments of disease" on man are, notwithstanding the special differences I have insisted on, fundamentally like the experiments of physiologists on animals; that a large and definite part of the study of nervous diseases, of convulsion in particular, must be put on an anatomical and physiological basis; and that Ferrier's researches will be a most valuable help in thus methodising our work—in making it less empirical and more scientific.

The first illustration is an outline of a case I have already published. A man had convulsions, each of which began in his left thumb. He died: we found no other disease in his brain than a tubercle, the size of a hazel nut, in the hinder part of the third right frontal convolution. It will be particularly interesting to me to know what effects Dr. Ferrier will obtain by Faradising the homologous part of a monkey's brain, for he tells me that these animals will shortly be subjected to experiment. Theoretically, I should not expect an identical convulsion, but an homologous one. I will, before giving the second illustration, remark on this point, and, at the same time, state some facts which show that the study of cases of convulsion bears on what is conveniently, if not correctly, called the physiology of the mind. One constantly hears, however, that the convolutions are not for movements, but for "ideas," "memory," &c. Yet those who use psychological phraseology to describe symptoms of disease—"loss of memory for words," "chorea the result of disorder of volition," &c.—have as much as other people to seek the anatomical and physiological substrata of mental phenomena.

Among the fits which begin unilaterally, the commonest are those in which *the spasm starts in the index finger* and thumb. This is significant. It is an illustration of what I believe to be the law of the effects of lesions of the brain. In evolution (development, education, &c.), the progress is from the general to the special. In the opposite process of dissolution the more special parts suffer first. I generally use the term "voluntary" instead of "special;" it is a convenient counterpart to the term automatic. In physiological language, a voluntary part—the hand, for example—is one which has the greater number of different movements at a greater number of different intervals—shortly, the most varied uses; an automatic part—the chest, for example—is one which has the greater number of similar movements at the greater number of equal intervals—shortly, more similar uses. In brain-diseases parts suffer the more as they are voluntary, and the less as they are automatic. Now the thumb and index finger are the most voluntary or specialised parts of the body; hence the suggestiveness of the case I mention. The thumb in man has a distinct flexor longus pollicis. In the Anthropomorpha, Huxley says: "The flexor pollicis is more or less closely connected with the flexor communis perforans, or with that part of the muscle which goes to the index digit." On the intellectual importance of this muscle Duchenne insists strongly. "En somme, ces faits cliniques demontrent que le long fléchisseur du pouce est l'un des muscles qui sont essentiellement destinés, chez l'homme, aux usages manuels les plus delicats; à tenir et â conduire la plume, le crayon, le pinceau, l'aiguille, &c.; qu'il aide, en un mot, a l'exécution des travaux manuels qui sont â la hauteur de son intelligence supérieure." And in his work on Electrisation, he says that when the small muscles of the thenar eminence are atrophied, the hand loses its distinctive human character and approaches that of the monkey. The thumb in the monkey is less specialised than in man. If, then, we discover in a monkey the homologue of the part discharged in my patient, we shall expect from its discharge a fit of a less special kind; for example, not a fit beginning in its pollex, but more likely one beginning in the whole of its five comparatively little differential digits at once, if not in the whole arm.

I have yet to publish the case of a woman who had fits beginning in her left great toe (where fits beginning in the foot nearly always start); there was a small tumour in her right hemisphere. My colleague, Dr. Gowers, made a careful examination of the brain for me: he found that the tumour involved the lower part of the ascending frontal convolution. Although there were other lesions in this woman's brain—local indurations—there is a strong probability, amounting almost to certainty, that the fits depended on discharge of that part in which lay the tumour. Here, again, I await Dr. Ferrier's further researches in comparative physiology of the convolutions of the left hemisphere.

In the Medical Mirror of September 1869, I published the case of a man who had fits affecting the right arm. In this case there was a tumour in the hinder part of the first (uppermost) frontal convolution. (There was also a tumour in each lateral lobe of the cerebellum, to which I traced no symptoms.) I did not see this man in a fit; his arm was paralysed after the first seizure.

Lately I was allowed by Mr. Soutter to see a patient of his who had literally innumerable fits limited to the right arm. Mr. Soutter witnessed many; I saw several. The spasm passed down the arm except in the later fits, then it passed up. Shortly before the patient's death she had, Mr. Soutter tells me, universal convulsion. Here I correctly predicted disease of the hinder part of the first (uppermost) frontal convolution—not from physiological knowledge, but because of what I found in the other case just mentioned. Once more I ask, What would be the homologous series of convulsions from the artificial excitation of the homologous parts in a series of animals lower and lower in the scale?

[Dr. Jackson adds in a foot note:]

The significance of the fact, that the hand is the part in which convulsions, beginning unilaterally, most often start; that the arm suffers first, or most, or both, in the greater number of motor affections from brain-disease (hemiplegia, chorea, paralysis agitans), will be better realised after reading Herbert Spencer's remarks on lacteal organs, in chap. viii., vol. i., p. 359, of his Psychology (second edition), from which I give these extracts.

He points out and shows the significance of the "striking instances which *the animal kingdom* presents of unusual sagacity co-existing with unusual *development of organs*, which, by the help of complex muscular arrangements, give complex tactual impressions." After remarking, that it will perhaps be difficult to understand why touch, the simplest and earliest sense, should in its higher form be more than any other sense associated with the advance of intelligence, he says: "The explanation lies in the fact that tactual impressions are those into which all other impressions have to be translated before their meanings can be known." Of the human hand:—"All that we need here notice is, the extent to which, in the human race, a perfect tactual apparatus subserves the highest processes of the intellect. I do not mean merely that the tangible attributes of things have been rendered completely cognisable by the complex and versatile adjustments of the human hands, and that the accompanying manipulative powers have made possible those populous societies in which alone a wide intelligence can be evolved. I mean that the most far-reaching cognitions, and inferences the most remote from perception, have their roots in the definitely combined impressions which the human hands can receive."

The study of cases of disease of the nervous system appears to me to supply continual illustrations of the correctness of many of Spencer's deductions.—*British Medical Journal, May* 10, 1873, *p.* 531.

24.—MIGRAINE FROM INJURY TO THE HEAD.

By Dr. Douglas Morton, Physician to Louisville Hospital, U.S.

I will offer a number of facts, drawn from my own personal experience of migraine, that throw, I think, a flood of light upon part of the subject out of which arose the discussion between Dr. Anstie and Dr. Dale.

In my judgment these facts very distinctly support the theory of a neurotic origin. The symptoms attending all my attacks, in order and character, have been just the same; and each attack, as far as I have been able to learn, may be properly regarded as a *typical* expression of the affection. But the point to which I would especially call your attention is, that *my first attack was directly and immediately induced by an injury.*

About fourteen years ago, while a student at college, I had a severe fall from a swing in a gymnasium, my head striking with great violence the hard, beaten earth beneath. At the moment I felt no pain, but experienced a horrible sensation, which I suppose is best described by the word *stunned.* I went at once to my room, and when, after a short time, my symptoms had improved a little, I took up a Greek book in which I had to prepare a lesson for that day. To my dismay I found that I could see only the left half of the open book; and if I directed my attention exclusively to a line or a word, only the left half of that. My first impression was that my right eye had been injured; and when I looked in a mirror to find out if this were the case, I was startled at not being able to see any part of the right side of my face. I held my hand over one and then the other of my eyes, and discovered, as I supposed at the moment, that the injury to my eyes consisted in destruction of vision in the right halves of both, for with either closed I could see the left halves of objects I looked at with the other. As yet, as well as I recollect, my headache had not commenced. I returned to my book and tried to translate, but found this impossible—the meanings of the words had escaped my memory almost entirely. I could scarcely pronounce a word—the difficulty seeming to be that I could not well retain a conception of the sound represented by more than one letter at once. In a short time I began to suffer headache, but at first it was not severe, and I went out and joined a group of students on the college grounds, in whose conversation I ventured to participate. I found, however, that I was the subject of a peculiar kind of aphasia—I could intelligently follow their conversation, but had not the power to select words suited to the expression of what I wished to say. I hit upon words in a most curiously random way—a verb, perhaps, for a noun, or an adverb for a preposition. My headache continued to increase in severity, and in another hour became almost intolerable. Now I began to suffer nausea, and vomited, but the disturbance of vision had entirely passed away. I soon recovered from the sickness, though the headache lasted the remainder of the day—about eight hours.

Had I suffered no subsequent attack of migraine, I should now regard the condition which I have described as simply that to which "concussion of the brain" gives rise—the subjective symptoms of which, by the peculiar circumstances of my case, I could observe closely, and give a better account of than we can usually elicit from patients.

After an interval of about seven years I had my second attack, and on this occasion noticed phenomena that escaped my attention in the preceding one, viz. the appearance of such a figure as I have represented in the enclosed drawing, trembling in space, seemingly a few feet from my eyes, and a sensation of numbness in my right cheek and the right side of my tongue, which came on with the headache (if my memory is correct) after the optical phenomena had disappeared.

Since that time I have had several other attacks, each being expressed by essentially the same set of symptoms. The form even of the phantasm which quivers before my eyes in the outset of every attack has continued almost exactly the same.

On studying the etiology of my case, I have not met with the success Dr. Dale appears to have met with in his. In no instance have I been able to connect an attack with any deviation from what is my usual mode of life, whether as to diet, amount of work, mental or physical, amount of sleep, or exposure to weather. Nothing whatever in my experience has seemed to point to disturbance of digestion as a cause. I have suffered headache from the latter cause, and I suppose from most of the causes that usually occasion headaches in other people; but my *sick*-headache stands alone—*sui generis*—in its peculiar group of symptoms and in the obscurity of its etiology.

In my experiments in treatment I have been more fortunate; indeed my results have been positive, and, happily, decidedly favourable. I have experienced benefit from taking a cup of strong tea at the occurrence of the first symptoms; so, also, from a tumbler of sherry wine; but *the* remedy *par excellence* I have found in guarana. The specimen of this drug that I got was put up in a package, containing twelve powders, of, I suppose, about thirty grains each. I took four of those in two hours, and, though perfect relief was not afforded, my suffering was yet diminished to such a degree, that the attack, whose prodromes foreboded unusual severity, proved to be a very small affair indeed. Having tried the guarana only once, it may appear unscientific to speak so enthusiastically of *it* as *the* remedy; but so intense had always been my suffering, and so great was the relief following the medicine, that it is impossible for me to regard their connection as otherwise than a *propter hoc* relation. I have seen one or two other cases reported in which great benefit was derived from guarana. The remarks I have made upon treatment, here, however, are of secondary consideration, my main object being to give the circumstances attending my first attack, as, to my mind, they appear to throw light upon the true character of the affection.—*Practitioner*, *Aug.* 1873, *p*.100.

25.—ON GUARANA AS A REMEDY FOR SICK-HEADACHE.

By Dr. W. Macdowall, Assistant Medical Officer and Pathologist, West Riding Asylum.

The cases of sick-headache treated with guarana are as follow:—

Case 1.—M. N., aged 34. This lady has suffered from sick-headache for a great number of years, indeed from childhood. Her attacks were very severe until about eight years ago, when she removed from an inland country town to a residence near the sea. She states that until she was about twenty years of age she was in somewhat delicate health; not suffering from any disease in particular, so far as she knows, excepting her headaches, but was languid, weakly, and unfit for active exertion or occupation. After that date her health improved, and has continued excellent up to the present time. She attributes this establishment of her strength to a prolonged residence in the country.

Since early childhood she has suffered from sick-headache, and her tortures have continued with increased severity since she fell downstairs and injured her head. This accident occurred when she was about nine years of age. She was unconscious for some hours after the fall, and was confined to bed for some days.

The following is her account of the sick-headaches as they occurred before they decreased in severity eight years ago:—They occurred, as a rule, every three weeks; and she never escaped for longer than a month. She experienced no previous derangement of digestion; but when attack was imminent, she frequently had uneasy sensations at the pit of the stomach. These would gradually increase in intensity during a few hours, and generally ended in vomiting. As a rule, however, these preliminary stomachic symptoms were absent. Her first head-symptom was a slight throbbing pain, passing, as it were, between the temples or between the forehead and occiput. At the same time a feeling of great depression, with sickness, always appeared. The characteristic symptoms then gradually increased in intensity, and in from three to six hours the attack usually reached its height. Before this stage, however, had been attained, she became quite prostrate, and was obliged to go to bed. These attacks were always, sooner or later, accompanied by vomiting; the act of emptying the stomach increased the pain in the head to an almost unendurable degree; but when it was completely emptied of its contents—usually highly bilious matter—some relief to all the symptoms followed. On an average, the headache continued at its maximum during six or seven hours; an hour or two after vomiting the patient generally fell asleep, and then awoke in the morning free from all her previous miseries, but feeling extremely weak and languid.

Shortly, it may be stated that the attacks of sick-headache during the past eight years have been, in their leading features, similar to those already described, but somewhat less intense.

The following have been the results of treatment with guarana:—

Feb. 1. Slight symptoms appeared shortly after rising in the morning, and continued to get worse until 7 p.m. At this time she presented all the indications of an intense attack, and retired to her room, quite unable to bear up longer against her suffering. She now took ʒ ss of guarana, with the, to her, very unexpected and satisfactory result, that in about twenty minutes, all sickness and headache had disappeared; and she felt, as she expressed it, like another woman. The drug appeared to her to act like a charm; and so much pleased was she with the result, that she presented herself for inspection to some who had but shortly before witnessed her distressed condition. The improvement in her state was most marked and surprising, for, from being almost quite prostrate, she was at once able to resume her somewhat arduous duties. She also escaped without the occurrence of vomiting.

Feb. 22. During the morning, the usual premonitory symptoms appeared. At once she took ʒ ss guarana, the result being that instead of the attack running through all its stages, it disappeared entirely in less than half an hour.

April 8. She awoke in a state of considerable depression; during the day a typical sick-headache became developed; but from circumstances she did not take any medicine until 5 p.m., at which time the symptoms were very severe. No benefit resulted from the half-drachm of guarana which she then took. At 9 p.m., still feeling extremely ill, she took another ʒ ss, and in an hour all the symptoms had disappeared. During the whole day she had been unable to take any food, yet at ten p.m., all nausea had so entirely disappeared, that she was able to take a hearty supper.

Up to the present time (May 8) she has had no return of sick-headache.

Case 2.—R. D., aged 30; nurse. She states that she had suffered from sick-headache from childhood. The attacks, however, have not been quite so severe during *the past four years*. This change in their character she associates *with her change of residence*, for four years ago she left her native county, Leicester, and has since that date resided in Yorkshire. During childhood the attacks were not frequent—perhaps every two or three months; but after reaching puberty, they occurred regularly every month for a number of years. They invariably preceded the appearance of the menstrual flow. As she advanced in age, the attacks occurred at other times besides the menstrual one, so that on an average they came on once a fortnight. None of her brothers or sisters were affected like herself, but her mother suffered severely from sick-headache as long as she lived.

The usual course of attack is as follows:—After slight headache has existed about half an hour, she generally becomes slightly sick. The pain is always confined to the forehead, and is described by her as being of a burning, throbbing character. As the attack proceeds, the pain in the head becomes almost intolerable, and she is in the habit of going to bed, that she may obtain some slight relief through rest and quiet. The sickness rarely terminates in vomiting, but there is intense loathing of food, her appetite having entirely disappeared. On rare occasions the headache has remained at its maximum of severity for from twelve to twenty-four hours; but the average duration of this period may be stated at about six or eight hours. When an attack is disappearing, the sickness always disappears first; the pain in the head then lessens, becoming of a dull and depressing character; then, in about two hours, all disagreeable symptoms disappear, and the patient regains her usual health, being quite free from any depression or exhaustion.

Since her residence in Yorkshire, her headaches have not, as a rule, been so frequent, occurring not oftener than every six weeks; and it has even happened that she has been free from them for three months.

On four occasions she has been treated with guarana, and always with the happiest results.

March 1. She awoke in the morning suffering from severe sick-headache, although at that time it had not obtained the usual maximum of intensity. She took half a drachm of guarana at once, and experienced decided relief in about half an hour. During the remainder of the day she suffered from a dull uneasiness in the head, but was quite able to go about her usual duties.

March 31. After travelling, the premonitory symptoms of an attack appeared. On reaching home she at once took half a drachm of guarana, and in less than an hour was quite free from all inconvenience.

April 27. Awoke with headache, but being inclined to try if it would pass off without treatment, she delayed taking her usual dose of guarana. Feeling no better, however, she lay down in bed about 2 p.m. for a short time, but without relief. She then took half a drachm, relieving the sickness immediately, and the headache very soon afterwards.

May 10. About midnight she began to suffer severely from sick-headache and continued ill all night. In the morning she took a little tea, which made her sick, but with no relief to headache. At 8 a.m. she took the usual dose of guarana, but with no result; at 10.30 a.m. it was repeated with like result; at 2 p.m. another half drachm was taken, which gave relief very soon afterwards.

Case 3.—M. M., aged 25, single, has suffered from sick-headaches from childhood; indeed, she does not remember a period of her life when she did not suffer from them. When she was a child, they occurred about once a fortnight; but when she reached puberty, they became less frequent, the intervals increasing to four or five weeks. There has never been any connection between the occurrence of menstruation and the date of appearance of a sick-headache. For a number of years this young woman has been in delicate health. She labours under phthisis of a very chronic description, though the disease has occasionally assumed a more active form for brief periods, and there have been several rather smart attacks of hæmoptysis. Of late, however, with proper attention to their health, she has continued in moderately fair general condition, and quite able for her work as a nurse, but at the same time very much troubled with headaches.

Since her general health became delicate, her attacks of sick-headache have been much more frequent, and indiscretion in diet brings on one at once. As a rule they begin during the night. The first symptom is a dull throbbing pain in the temples; it gradually increases; then sickness comes on; so that in a few hours she is frequently necessitated to lie down in bed. Should the attack prove a mild one, it may pass off in six or seven hours; but it is more common for it to continue during a whole day. On rare occasions, when particularly severe, it has continued for two days. After a sick-headache has continued at its maximum for about an hour, the patient invariably vomits a small quantity of bilious fluid, sometimes with some slight relief to the pain in the head.

April 16. Patient awoke in the morning with sick-headache, which continued intense during the day, until 6.30 p.m., when she took half a drachm of guarana, and in half an hour she felt quite relieved.

May 10. Awoke this morning: sick-headache in first stage. Shortly after taking the usual dose of guarana, the symptoms began to abate; and by 9 a.m. she was quite free from all discomfort.

[Several other cases are related of the same description.]

—*Practitioner*, *Sept.* 1873, *p.* 161.

26.—ON THE THERAPEUTICAL USE OF ELECTRICITY.

By the Editor of the Medical Times and Gazette.

[The profession in general does not value this remedy as it ought to be valued. In fact, it is not studied or understood sufficiently. Although it cannot renew nervous tissue, and cannot cure some of the worst cases of paralysis, it can do a good deal, if the cases be properly selected. It can "soothe or stimulate diseased muscle and nerve, cure or relieve over-action (spasm) and pain (neuralgia), some forms of aphonia and of peripheral paralysis, renew the nutrition of muscles wasting from disease, restore the circulation, and raise the temperature of a paralysed limb, and recall or lower sensation."]

In relieving or curing pain, its therapeutic value is invaluable, and has been established beyond the possibility of dispute or doubt. In all the true neuralgiæ, and especially perhaps in the very worst of them — the most terrible and savagely intense of all painful affections—electricity rightly chosen and applied, displays often a remedial power not only unsurpassed, but unrivalled by any other known means. But this use of it is not so generally known or understood even yet (among us at least) as is its employment in the different forms of paralysis. And the reason of this is perhaps not far to seek. Duchenne was the apostle of faradisation, Remak of the constant current; and the teaching of Duchenne has always been more accessible to, and better known by, the mass of the profession than has been the teaching of the German school. Moreover, Faraday was the discoverer of the induced electric current, and that fact has also not a little helped to make faradisation more popular and better known with us than the constant current; consequently it came about that for a long time it was generally believed in this country that the rotatory electro-magnetic, or magneto-electric machines, were sufficient for all electro-therapeutic purposes; and, indeed, very many of our brethren still suppose that it is the case, and when they have obtained one of these machines believe that they are fully equipped for the remedial employment of "electricity." But it is absolutely certain that in the treatment of painful affections the constant current has a far wider usefulness and a far higher value than faradisation has; and we suspect that even the strongest believers in faradisation will allow, with Dr. Beard, of New York, that in the treatment of true neuralgia "the galvanic (*i.e.*, the voltaic or constant) current will oftentimes succeed where the best faradic fails, and even in those cases where faradisation relieves, and promises to cure, galvanism may do the work in a much shorter time."

In some painful affections, and some that are often improperly called neuralgiæ, faradisation is undoubtedly of great use; but in the neuralgiæ proper, the *constant voltaic current is* almost always the form of electricity which *ought to be employed, both the* electrodes being firmly pressed upon the skin and held there, neither of them being moved about: that is, the current must be the voltaic, and must be *constant*, not interrupted; and the electrodes must be applied so as to include in the circuit the affected nerve. As to the number of the cells employed, this must be regulated by the sensations of the patient, and according to the region affected; they should be as many as can be borne without pain or serious discomfort; and in the face five cells will be fully enough to begin with. In this part fifteen or twenty cells will most likely be the highest power that can be borne or safely employed, the application being discontinued for a while and the number of cells reduced whenever any giddiness occurs. Elsewhere a higher power may be used. Whether the doctrine that in true neuralgia the posterior root of the nerve is the seat of disease be accepted or not, it is, we suppose, generally held that it is best to place one of the electrodes as nearly as may be over the point of origin of the affected nerves, and the other over the painful spot; or, should there be more than one painful spot, then over the several spots in succession. The direction of the current has by some been considered of importance, and directions have been given to apply always the positive electrode to the seat of disease; but the necessity for this is not clear; and some experts are convinced that the direction of the current —whether it should be ascending or descending—is not therapeutically a matter of any importance whatever. The electrodes should be furnished with well-wetted sponges of large size, and the application be made for short periods—about five minutes—and repeated oftener than once a day if necessary.

Cases of neuralgia of the most intense character have been not only rapidly relieved, but permanently cured by this means. Niemeyer's two cases are perhaps the most striking. Dr. Anstie, in quoting them, observes—"The patients were respectively aged 64 and 74, and the duration of the neuralgia had been respectively five and twenty-nine years; in both the pain was of the most severe type, and in both the success was most striking. In one every possible variety of medication and several distinct surgical operations for excision of portions of the affected nerves had been quite vainly tried." Such brilliant success as this is not always to be expected; but many very striking cases besides have been recorded. Dr. Russell Reynolds has mentioned the case of a lady who for twenty years had suffered from very severe neuralgia of the ophthalmic branch of the fifth nerve, recurring every day, and greatly injuring her general health, and which was not merely relieved, but removed, by a single application of the constant current. Dr. Althaus, also, has published the case of a lady who for twenty-five years had suffered from the most violent neuralgia on the left side of the face. Three applications of the constant current from four cells of Bunsen's battery were, he says, "sufficient to cure her of a disease which for twenty-five years had embittered her life."

While all the best authorities on the use of the constant current agree in estimating very highly its remedial power in neuralgic affections, they vary in their experience of its value in the different forms of this disease. Thus, Eulenberg, whose experience has been very large, thinks very highly of it as a palliative in trigeminal neuralgia, but rather doubtfully as a curative; while Dr. Anstie credits it with more of a curative power. And while Eulenberg has found sciatica the most curable of the neuralgiæ by this means, Dr. Beard has had least success as to *cure* in this form of neuralgia, though he has generally obtained *relief*. His experience leads him to consider gastralgia as more amenable to the constant current than the other remedies.

In migraine, or true sick-headache, the constant current has been found very beneficial, the electrodes being applied to each temple or to the mastoid processes. A very low power must be used, two or three cells. The application must not be for longer than about sixty seconds, and it must be discontinued on the occurrence of any giddiness, or if the patient exhibits any symptom of uneasiness in the head or the epigastrium. In all the true neuralgiæ, then, the diagnosis having once been clearly made, the constant current is the form of electricity to use; and used rightly, so as to act gently and steadily, without exciting pain or other nervous disturbance, it may be employed fearlessly and with immense advantage.

This kind of electricity has also been successfully employed in toothache. M. Bouchaud states (*vide Bulletin General de Therapeutique*, July 15)—"If in odontalgia we employ a continuous or constant current, derived from a mean of ten elements, applying the positive pole to the cheek on a level with the painful tooth, and the negative pole to the anterior lateral region of the neck, we are enabled almost always in the course of a few minutes to procure nearly absolute relief, which in the majority of cases persists indefinitely." M. Bouchaud's paper on the subject is well illustrated by cases; and he has found the constant current useful even when the teeth were carious and the gums inflamed and swollen.

If for any reason it is determined to apply the faradic current in neuralgia—and sometimes it is beneficial—the rough coarse current supplied by a rotatory magneto-electric machine should not be ventured on, but the most gentle and steady current of this kind derivable from a volta-faradic apparatus. In some painful affections, however, faradisation may be employed, not only usefully, but with much greater advantage than the constant current. Such are the pseudo-neuralgiæ called "hysterical," and the hysterical hyperæsthesiæ. In these affections it is desirable or necessary to excite pain, and the ordinary rotatory machines will serve for the purpose. The current must be used to affect the skin only, not to penetrate deeper; the skin must therefore be carefully *dried*, and dry metal electrodes used—one being placed on an "indifferent spot," and the other moved about over the affected part: or, the patient holding one of the brass handles of the machine, a coarse wire brush may be applied over the affected region.

The widest field for the application of electricity, and especially for the "localised employment" of it, is no doubt in the treatment of paralytic conditions; and we will now treat of its application in these various affections.

In paralysis from cerebral disease, great care must be taken not to employ it too soon. If the paralysis has come on suddenly, or if, though it has come on gradually, there is giddiness or pain in the head, or any sensation of weight or fulness of the head, too much caution cannot be used—some fresh mischief may occur under such circumstances, and its occurrence will be attributed to the electrical treatment, though it may be merely and entirely a coincidence. It is far better, in such cases, to wait for a time—for four or more months—before resorting to the employment of electricity, as it is advisable to wait before using any other kind of treatment that would disturb or excite a patient. If none of these head symptoms are present, and the disease has only gradually come on, no such degree of caution is necessary, though then also care should be taken, as we have before said, not to alarm or agitate a patient by a parade of the electrical apparatus or by the mode of using it.

When it is decided to employ electricity, the first step to be taken is to test the excitability of the muscles by the faradic and interrupted-voltaic currents; and the mode in which this is to be done we have already described. The manner in which the muscles respond to the test—that is, the amount of contractility they show—will tell what good may be looked for from the employment of electricity. If the contractility is normal, though voluntary power is abolished, electricity will do nothing towards restoring the power of the will over them; if the contractility is somewhat, but only slightly, diminished, the condition of the muscles may be improved; and when the contractility is found to be markedly reduced, but not abolished, great good may be achieved—the contractility may be gradually brought back till it equals that of the healthy muscles, and then it may be that the paralysis will be found to have been lessened, and now and then even cured. And in all cases this good at least will most likely have been obtained: the nutrition of the muscles will have improved, and colour and warmth have been restored to the skin. This is by itself a gain of great importance, for the muscles and the neighbouring tissues are thus kept in a proper state of fitness and readiness in case the disorder of the brain or of nerve-power that caused the paralysis be restored, and the influence of the will over the muscular part of the locomotive apparatus be regained. And further, it appears to be certain that electrical action on the muscles, peripheral electrical excitement, as it may do no harm to, so also may aid the recovery of, the nerve-centres. That it does act on them is shown by the fact that *peripheral galvanisation* of the limbs is often observed to excite a *peculiar sensation of taste.*

Having tested the excitability of the muscles, it will be best, as perhaps the most useful general rule, to treat them by means of the current to which they most readily answer; or to employ the interrupted voltaic current where the excitability is normal, and the faradic where it is diminished.

If voltaism is employed it must be by the interrupted, not the continuous current. Large-sized sponges should be used, well wetted with salt and water, and one (the upper one of the two—that is, the nearest to the central organs of the nervous system) being kept steadily on one spot, the other should be moved slowly down along the course of the muscles; so that every part of each of them is successively brought into the circuit of the current, and by the movement of the lower sponge the current is *interrupted*. And here we may again remark that therapeutically, it does not matter whether the current is direct or inverse; only it must be remembered that the direct current acts more strongly on the muscles than does the inverse, and that the current must not be so strong as to excite pain: it must be just strong enough to cause contraction of the muscles, not strong enough to hurt.

This rule applies also to the employment of faradisation. The patient is not to be hurt or wearied; and whenever, during the use of any form of electricity, the patient feels faint, or giddy, or sick, or has pain in the head or a sensation of sinking or uneasiness in the epigastric region, the application must be stopped for a while.

If the faradic current is the form of electricity selected, sponges or metallic buttons covered with chamois leather are to be used; and they must be *well* wetted, or the current may only irritate the skin, instead of penetrating to the muscles. Moreover, the rheophores should never be widely separated; it is important to localise the action of the current as strictly as possible; and, with this view, it is best to use poles of such shape and size that both can be easily held in one hand. Then the muscles are to be faradised *seriatim*, each being picked out, and, according to its size, the current being passed through the whole at once, or through different parts successively, so that no portion of it shall escape being electrised. Particular points will be found in the arm and leg (and in other parts, as in the face), at which, on application of the current, the effect on the muscles will be stronger than when it is applied elsewhere. These, called by Duchenne "points of election," are the places at which the motor nerves entering the muscles lie most superficially.

Electrisation may be successfully employed to relieve, or even cure, the "late rigidity" of hemiplegia. In an early stage of it the tendency to cramp may be removed, and the rigidity cured; and even in old and severe cases, when the nails have dug into the palm of the hand, considerable alleviation and benefit may be obtained. Either the faradic or the interrupted voltaic current, applied to the antagonising muscles, will do good, but the greatest benefit will be gained by applying faradisation to the antagonistic muscles, and aiding it by sending a continuous voltaic current down the rigid muscles; voltaise the flexor muscles, and faradise the extensors.

In *paraplegia*, from myelitis, meningitis, or hemorrhage into the cord, electrisation must not, of course, be used while any active mischief exists; and there will be often much difficulty in determining when it may be safely employed. But the affected muscles may be now and then tested by the interrupted voltaic and the faradic currents, and when it appears that the time has arrived when electrisation may be used, one of the forms, or perhaps both, may be applied in the manner already described. In some of these cases, the only good that can be gained is an improvement in the power of the sphincter of the bladder and rectum: if their power for retention has been lost, electricity will sometimes restore it. To act on the sphincter of the rectum, one sponge-pole, well wetted, of the faradic current, is to be placed on the sacrum, and the other applied to the anus; if the vesical sphincter is to be treated, apply one sponge to the perineum just behind the scrotum, and the other above the symphisis pubis. This will in some cases, to some extent at least, prevent involuntary evacuations, and thus give considerable relief. Or the *paralysis* may be incomplete, and then larger benefit may be obtained. The *good to be looked for* may be measured by the state of electro-excitability of *the muscles; if entirely* abolished, no good can be done, and none if it be normal; but where it is diminished more or less, the nutritive condition at least of the muscles may be improved.

In the "essential paralysis" of infancy and childhood electrisation may be of considerable use if employed before the muscular tissue is too much degenerated. In this form of infantile paralysis the affection comes on suddenly, the farado-contractility of the muscles is more or less diminished, they waste, and their tissue undergoes the fatty degeneration; and they are affected by secondary contractions. Not rarely it will be found that when the affected muscles answer but little, or not at all, to the faradic current, they will respond to a slowly interrupted voltaic current, and are even more than normally sensitive to it. The good that electrisation can do here is to improve and so preserve the nutrition of the muscles, and so prevent wasting, degeneration, and contraction; and in some cases even more than this may be done, for if the affection of the cord can be cured, the peripheral electrical treatment may forward the cure. Where there is still response to faradism this form of electricity is the best to use, care being of course taken not to frighten or pain the child. Where the irritability to the interrupted voltaic current is *increased*, it will be best to treat the case at first with this current. In a little time it will be found that the reaction to the faradic current is restored, and that the excitability to the interrupted voltaic current lessens; then faradisation only may be employed. The disease is almost invariably a tedious one, and the treatment must be continued for a very long time—for months, or years even. But so long as there is any response to electrisation there is a hope of benefit. Dr. Radcliffe says that "if the paralysed muscles retain their electro-contractility and sensibility, and so show that they have not passed into that state of fatty degeneration into which they always tend to pass eventually, there appears to be scarcely any limit to the time in which improvement, and even complete recovery, is possible."

In *traumatic* paralysis faradisation is invaluable. Brown-Séquard has proved that even the spinal cord may be perfectly restored after traumatic lesion, and it has long been known that nerves will perfectly reunite after division, and when even considerable portions of them have been excised. But after reunion of a nerve has taken place, the muscles supplied by it will often remain paralysed, and continue to waste, and contract unless faradisation be resorted to. Its value in this form of paralysis was largely tried and well proved in America in cases of men wounded in the Civil War; and in a report on "Gunshot Wounds and other Injuries of Nerves," the surgeons in charge say:—"The only important means in the treatment of paralysis from default of innervation is faradisation by the method of Dr Duchenne. Most of our cases were from wounds that were of old standing when they came under our care, and localised faradisation proved of the utmost value. In some cases at a single application it restored the power of movement to parts long deprived of it, and we have seen very few instances in which there has not been improved nutrition and greater sensibility and strength when we were able to continue it sufficiently long." The faradisation will, of course, will be of no use till after the injured nerve has regained its power of conduction; but in recent cases of injury the interrupted voltaic current should be localised in the affected muscles, and direct faradisation be employed when farado-contractility begins to return.

In cases of *local* paralysis, dependent on cold or on metallic poisoning, as from lead or mercury, or on diphtheria, the employment of electrisation should not be neglected when the affection has resisted other means of treatment. In these cases the faradic and the interrupted voltaic currents, used alternately and localised in the affected muscles, will generally effect a cure. As a rule the farado-contractility is unaltered in muscles paralysed by exposure to cold; but this is not the case in facial paralysis from cold. Here the farado-contractility is diminished or quite lost, while the excitability to the interrupted voltaic current is increased; and faradisation may be employed for weeks without effecting any improvement. In this affection, therefore, the interrupted voltaic current must be used and localised in the muscles till the farado-contractility is restored, and then the two forms may be employed alternately; and in this way cases of even years' standing may be cured or greatly benefited. It will be remembered that in speaking of the diagnostic uses of electricity we mentioned it might be employed to distinguish *lead-palsy* from paralysis caused by cold, for in lead-palsy farado-contractility and sensibility are diminished or lost, while they are normal in paralysis from cold. In lead-palsy the interrupted voltaic current should be applied to the affected muscles. As in other forms of paralysis, where this current acts and the faradic current elicits no response, the sensitiveness to the former is abnormally great, and it is found to lessen as the farado-contractility returns, till each becomes normal. The voltaic current employed must be a slowly interrupted current, and may be gradually abandoned to the faradic current as farado-contractility is restored; and whichever of the two currents is used, it must be localised in the muscles—not indirectly applied.—*Medical Times and Gazette, Sept.* 20, 27, *and Oct.* 18, 1873, *pp.* 327, 356, 436.

27.—ON THE RELATIONS OF FARADIC ELECTRICITY TO PAIN.

By Dr. Francis E. Anstie, Editor of the Practitioner.

One of the most important uses of electricity is certainly the relief of pain. And as pain is so common, and often so troublesome a thing to deal with, it is natural that practitioners should eagerly seize hold of what promises to be a new mode of relieving it, without inquiring very minutely as to the precise conditions under which success is most likely to be obtained. The outcome of this indiscriminate employment of the remedy can only be vexatious disappointment in a number of cases, and, in some, a positive aggravation of the sufferings of the patient.

It is time for everyone to understand that in regard to the treatment of painful affections, faradisation has very distinct and rather narrow boundaries of usefulness; and these it is now our business to define.

1. Faradisation is, in the first place, pre-eminently useful as a *mental counter-irritant* (if we may use the phrase), and therefore will often cure pains of the class vaguely called "hysterical." In the relaxed condition of the nervous tone which belongs to this state, the mere accidental concentration of attention on a particular part of the body will often convert some slightly unusual sensations into the mental perception of actual pain; but this perception is factitious, and can be speedily destroyed by a suitable diversion. For this purpose smart and painful faradisation of the skin is well adapted, and it should be applied to a somewhat larger area than that which the hysterical pain occupies. The surface being carefully dried (and, if necessary, powdered), in order to limit the effects as much as possible to the skin, the current is applied with dry metal or carbon electrodes, one pole being placed on an indifferent spot, the other moved slowly about over the area which we mean to operate upon. The result is, that the mental attention is so scattered and divided among a number of new and sharp impressions, that its morbid concentration is lost, and the sensation of pain disappears. It is very necessary, in the early stages of such cases, to have the electrical apparatus at hand, so that any relapse into the state of actual pain may be promptly dealt with.

2. The pains we have just been speaking of are mainly of a pseudo-neuralgic character; that is to say, they run so far in the direction (or the neighbourhood) of recognisable nerves as to simulate neuralgia pretty closely whilst actually present. Those which are next to be mentioned are not distributed in neuralgic areas, nor do they take the form of spontaneous pain, but that of unnaturally heightened sensitiveness to pressure—the so-called "hysteric hyperæsthesia." This phenomenon, as is well known, is quite superficial, deep and firm pressure giving less pain than that which merely compresses the skin or mucous membrane (for either may be affected). Here, again, faradisation has a most legitimate office; but it needs to be applied with a firm and unsparing hand. Supposing, for instance, that the hyperæsthetic area is on the skin, one uses dry conductors, one being placed five or six inches distant, on an indifferent spot; the other (which should be in the form of the "wire brush")

should be so used as to apply a kind of electrical flagellation to the whole of the affected area. This must be kept up for several minutes, with a powerful current; and if the painfulness of the proceeding becomes intolerable, it is better to give the patient a little chloroform and go on (as Althaus, I believe, first suggested) than to do the thing by halves. It is not often that this procedure fails to cure in one, or at any rate two, sittings: most frequently it is at once successful if properly carried out. If the tender surface be mucous—in mouth, rectum, or vagina—the process is still more disagreeable. We use metal conductors as before, but of course cannot employ the wire brush to the mucous surface, but a solid disc or button instead, moving that electrode about over the whole sensitive area.

3. We come now to the large class of cases in which the pains are evidently situated either in the muscular or the tendinous structures, and are closely connected with the degree in which the muscle has been over-exerted in proportion to its nutrition. These "myalgic" affections are also frequently in part immediately excited by the action of external cold and damp upon a similarly under-nourished muscle. They are distinguished by the violent exaggeration of the pain which is caused by any movement of the affected part. There can be no doubt that local faradisation will very usually remove these pains; the only objection to its use is that for the most part the affection is curable by less troublesome and disagreeable means. The hypodermic injection of morphia, in dose of $\frac{1}{6}$ grain or $\frac{1}{4}$ grain, locally, or the muriate of ammonia (internally) in 30-grain doses, will often give prompt relief; or Dr. Reynolds' plan, of sewing a piece of oilskin outside the jersey over the painful part, will do the rest. If faradisation is to be used, however, it is important to remember that it can only be successfully employed in one way; namely, by strictly limiting its action to the skin. For this purpose we must have the skin very dry, and use dry conductors. If we allow the current to penetrate to he muscle and set up contractions, we shall only add to the agony and ender the malady more tedious than it is apt to be naturally.

This brings us to the consideration of a particular class of muscular pains, viz., those caused by sudden laceration of fibre, an accident which is not very uncommon. Fresh interest in the faradic treatment of these affections has been raised by a recent case under Professor Broca, of Paris, although the method is already some years old. The patient got the injury in some playful scuffle, the arm being pushed forcibly (in a direction which is not very clearly stated), while the biceps was contracted: the latter muscle gave way. Local faradisation was immediately employed, and though painful it immediately restored the power of raising the hand to the head. The reporter (M. J. Lucas-Championnière) remarks on the strangeness of several circumstances connected with these ruptures of muscular fibres. He does not seem inclined to think that the contraction of the muscle itself could produce the accident, but rather (with M. Broca) that the already contracted muscle is torn by some violent wrench in the direction of extension; and is especially liable to this if its tissues be in an unhealthy state. But how comes it, he asks, that a method (faradisation) which causes energetic and painful movements of the torn muscle can at once replace it in a condition to perform the usual voluntary movements without pain? We doubt, however, if this be quite an accurate statement of what takes place. In the first place, we do not think that faradisation *which makes the muscle contract* is the real agent at work. In a case where we personally used faradisation with complete success, the patient had torn a portion of the biceps in holding on to a rope which had a heavy weight at the other end; the machine employed was the common rotatory electro-magnetic apparatus, and one of the ordinary brass handles was grasped by the sufferer, while the other was moved about on the skin over the injured muscle without exciting any notable contraction: yet in a few minutes the arm could be flexed and extended painlessly. The rupture was distinct and visible to the eye. There is also, of course, a probability that muscular fibres which are completely divided, even if their elements be made to contract, would give give far less pain than if their attachments had remained entire.

4. There is a somewhat indefinite class of so-called rheumatic pains which appear to be seated in the ligaments of joints, or in the periosteum of bones,

occasionally also in fasciæ. Of course (and more especially under the periosteal group) there is a great danger of reckoning cases that are really syphilitic; but after careful exclusion of these, a considerable number remain which it is permissible to call chronic fibrous rheumatism. It is very doubtful whether faradisation exercises any direct influence on this affection; for our own part we have not been able to obtain any decidedly positive results, and are inclined to suppose that cases which have appeared to be relieved were really of a myalgic nature. One of the fairest instances in which to test the power of faradisation would be that peculiarly chronic and intractable affection—rheumatism of the plantar fascia: but it must be remembered that, in many persons, the skin of the sole is so dense and resistent, that there will be no chance of applying the electricity with sufficient penetrating force unless the integument has first been thoroughly macerated with warm water.

5. The great question, however, is whether faradisation is of use in true neuralgia; and to this we believe that a decided answer in the negative must be given. It is easy enough, no doubt, to produce evidence of cures by faradisation in what has been called neuralgia by some persons; but it must be remembered that the present tendency of medical classification is to separate true neuralgia very distinctly from the various other pains which have been confounded with it. No pain ought to be called neuralgia unless it obviously follows, on the whole, the track of a recognisable nerve; the affections of this class have a very definite clinical and family history which enables us to identify the disease as clearly as we can identify any malady that exists.

There is ample scope for testing the power of faradisation to relieve the pain of genuine neuralgias, since these are common enough, and, in several forms, are so typical that they cannot be mistaken. Personally we have tried faradisation over and over again in distinct trigeminal, brachial, sciatic, and cervico-occipital neuralgias, and the general result of our experience is quite clear. In a considerable number of cases, probably the majority, the process only aggravates the pain; and we have known instances in which a neuralgia which, to all appearances, had begun to recede, was at once wakened up and made worse than ever, and more rebellious to the influence of ordinary palliatives than before. In other cases we have seen momentary apparent benefit, which seemed due to the nervous surprise, so to speak; but such benefit does not endure (as it often does in hysterical pseudo-neuralgia), the pain soon returns again, and, if anything, with increased violence. In other cases, again, faradisation has produced no effect whatever, good or bad.

On inquiring further into the matter there are at least two reasons why faradisation would be likely to prove either indifferent or hurtful in the treatment of true neuralgia. If the current does not penetrate beneath the skin, the only effect produced will be a superficial irritation which might (in a reflex manner) possibly act beneficially if it could be kept up for a long time (as the effect of a blister is), which however cannot be done. On the other hand, supposing the current to penetrate more deeply, it is likely to do mischief in either or both of the following ways:—If it encounters the nerve, it inflicts upon it the jarring sensation which the very nature of the interruped current necessarily involves, and which we cannot but suppose must be exceedingly inimical to the restoration of the irregular nerve action to a state of equilibrium. On the other hand, it is difficult or almost impossible to prevent the *penetrating* current from exciting the surrounding muscles to contraction: and it is well known that the most certain aggravation of neuralgia is produced by the dragging and squeezing of muscular movement: the strongest examples being seen in the effects of mastication in neuralgia of the third division of the fifth, and of certain complicated muscular actions (*e. g.* pianoforte playing) in brachial neuralgia.

The above remarks give a rough outline of the reasons why faradisation is generally useless or objectionable in true neuralgia. In that disease it is at once desirable to act on the nerve itself, and yet to act with the greatest gentleness and steadiness. The true neuralgias, as we have tried to show elsewhere, *involve a real change* (it matters little now whether we call it atrophy or *rhythmical molecular alteration*) in the posterior or sensory root of the nerve, and *this state is best com*bated by such means as tend gradually and steadily to restore the nutrition of the nerve. And, on the other hand, clinical experience teaches us, very positively, that there are no such terrible excitors and aggravators of neuralgic pains as repeated intermittent impulses. The pulsation of a vessel, for example, which presses upon a neuralgic nerve, sends thrill after thrill of pain through it, till at last the torture becomes unbearable, even to a patient who perhaps could endure well enough a constant though severe suffering. Now, the intermittency of the interrupted current is a matter of degree, and consequently the amount of mischief that may be done by faradisation varies very much according to the apparatus used and the exact mode of using it. This brings us naturally to the final remarks which we have to make, which concern the question of apparatus.

A fairly good faradiser for those cases in which we desire to inflict a good deal of pain (or at least a considerable nervous perturbation), is the ordinary rotatory magneto-electric machine, which, till very lately, has been absurdly supposed, in this country, to be efficient for all electro-therapeutic purposes whatever. There is no question, however, that it does very well for hysterical pseudo-neuralgia and hysterical hyperæsthesia, especially if the patient be made to hold one of the brass handles, and a very coarse wire brush be used as the other electrode, and freely applied to the affected part. But it is usually (except for the mere saving of trouble) better to use a volta-faradaic apparatus —*i. e.*, one in which the motor power is got from a galvanic or voltaic cell; and for the purpose in hand a very simple and inexpensive apparatus will do, such as Hearder's "20-power" machine, though of course it is better, if possible, to supply oneself with a thoroughly complete instrument, like those of Stöhrer or of Messrs. Weiss.

In dealing, however, with painful affections by means of faradisation, there are more considerations than one to be taken into account in the choice of the apparatus to be used. Besides mere questions of convenience in application, there is the question, what degree of frequency and regularity do we desire in the interruptions of the current?

As a general rule, it is true that, "in irritation by induced currents, the more rapid the interruptions the greater is the pain produced." The interruptions are rendered more frequent, or lower, by manipulations with screws, which place the interrupting hammer nearer to or farther from the platinum point. But in dealing with actual clinical cases, it will not be found that the therapeutic facts exactly correspond with this rule. The object being to create intense nervous surprise and distraction, it will not unfrequently be found that, a current of sufficient intensity having been provided, a series of interruptions which is slow on the whole, but has an *irregular rhythm*, is more effective than a mechanically regular series of rapid interruptions. It is not easy to lay down precise rules here, but the following is about accurate as a representation of my own experience:—In proportion as the malady is beyond the patient's own control, it is desirable to employ a regular series of intermissions, and *vice versâ*. Given, that is to say, a patient with none of the common convulsive phenomena of hysteria, but with some definite paralysis or anæsthesia, and whose skin presents a patch of acute hysteric tenderness; then I advise the use of a tolerably intense current with rapid and even intermissions. The patient should be placed under chloroform, and the dried skin should be well fustigated with the wire brush of a Stöhrer's or Weiss's faradiser, the secondary current being employed, of high strength. But if the affection be of that exceedingly common kind in which the morbidness of the will has much to do with the painful sensations themselves, then it is a good plan to proceed differently. For such patients no chloroform should be allowed; the electrodes should be brass discs, and a common magneto-electric apparatus may be employed. The current being arranged at a high degree of intensity, the driving-wheel is to be urged at a varying and irregular pace: it will be seldom that the morbid sensations do not give way after a few sittings conducted in this manner.

In speaking of the limitations of faradisation as a remedy for pain, we have not forgotten, though we have left so far unmentioned, the methods which are called *electric pencil and electric moxa*, and which aim at producing a more *severe and continuous painful* impression. Our own experience is decidedly in favour of the abolition of such methods of treating pain; they ought only to be used, we think, for cases in which there is every reason to think that the whole symptoms are mere malingering. If it be really desirable to push the skin irritation beyond the limits already traced, it will be much better to lay aside electricity and resort to the more prolonged and gradual excitement which can be produced by blisters; and if these fail (especially when used in conjunction with hypodermic injections of morphia), we must resort to the constant current. In all the true neuralgias there cannot now be a question that the latter is essentially the right, and the only right, form of electricity to be employed.

As a parting word of insistance upon the necessity for not employing faradisation except within its proper limits, we must especially dwell upon one variety of malpractice which has not unfrequently been followed by very disastrous results. We have had occasion to know that in cases of rachialgia (spine-ache) it is not uncommon for practitioners to apply one pole of a common magneto-electric apparatus to the nape of the neck and the other to the sacrum or to the feet (in a water bath), turning on a pretty sharp current. Such practice is not merely senseless, it may prove very dangerous; for the shock which is thus given to the system is much more serious than that produced by any amount of pain inflicted upon a limited skin area. There is no reason to think that such faradism has any direct effect upon the spinal cord, but in a reflex manner it may have a most serious effect; and the greatest general prostration, or even paraplegia, may be produced. It is really a heavy disgrace to the profession that such utterly reckless proceedings can possibly occur as some which we have personally known to take place in the supposed electrical treatment of what the practitioner was pleased to call hysterical pains.—*Practitioner, June* 1873, *p.* 351.

28.—TREATMENT OF NEURALGIA BY ELECTRICITY.

By Dr. George M. Beard, New York.

[Electro-therapeutics are exciting a good deal of attention just now, and probably no subject has been more neglected by medical men.

In order to do good by this remedy, you must not confound neuralgia with rheumatism, nor with hysteria.]

The generally accepted notion that the disease is where the pain is, has led physicians to apply the current solely to the seat of the pain. Logic and experience show that the seat of the disease, the central nervous system, should also be treated.

Then, again, it has not been recognised that the different forms of neuralgia are but manifestations of a constitutional tendency, and should be treated accordingly.

The whole central nervous system is invaded by a mysterious foe that darts out along the great pathways of nerve-force in any direction and where least expected. The special manifestations of the neuralgic tendency are often different at different periods of life.

I have now under my observation a gentleman of middle life, who, in youth, suffered from facial neuralgia; in later years, chronic cervico-brachial neuralgia has perpetually harassed him; during the past year he was prostrated by a terrible attack of gastralgia, and just now he is recovering from an invasion of sciatica. To treat such cases by merely local electrisation, whether it be central or peripheral, or both combined, may indeed palliate or relieve; but to expect permanent victory over the neuralgic tendency by merely local treatment where the pain last appears, is unreasonable and usually unsuccessful.

The differential action of the galvanic and faradic currents has not always been distinctly kept in view. For true neuralgia in any part of the body, the *galvanic current is* usually preferable to the faradic. It is not true, however, *that genuine neuralgia* never yields to faradisation; it does so quite frequently;

but everything depends on the *kind of faradic current used*, and the *method of application*. There is as much difference of therapeutical effect in the qualities of faradic currents as generated by different machines, as there is between the galvanic and faradic currents.

A rough, harsh current, such as is generated by Stöhrer's machine, will almost always do more evil than good to neuralgia, especially in the irritation stage. Such a current will pretty surely do harm if it be applied strongly and recklessly, and may do no good when applied gently and cautiously. On the other hand, a rapidly interrupted and pleasant current, such for example as is generated by Kidder's machine, when applied cautiously, may, and in my hands frequently has, relieved and cured true neuralgia.

The physical principles that account for this differential action of the faradic currents from these different machines is somewhat complex and cannot here be discussed. I may suggest that the physiological and therapeutical action of faradic currents seems to vary, not only with the length and thickness of the coils, but with the rate and manner of interruption, and with the details of the construction of the helix. All the leading forms of neuralgia—facial, central, occipital, cervico-brachial, gastralgia, and sciatica—I have treated with more or less success by faradisation with secondary and tertiary currents, from machines such as I have referred to; and yet the galvanic current will oftentimes succeed where the best faradic fails; and even in those cases where faradisation relieves and promises to cure, galvanisation may do the work in a much shorter time. In regard to the question of the differential value of large and small cells in neuralgia, I may say that the idea that large cells send greater quantity of electricity through the body than small cells is opposed to the teachings of Ohm's law, and has no basis in experience. It is, however, not improbable that large cells, especially those various modifications of Daniell's, by virtue of their greater constancy and the great slowness and uniformity of the chemical changes that take place in them, may be less irritating, and therefore better adapted for neuralgia and other irritable conditions.

The difference in this respect cannot, however, be very great, since many of the greatest triumphs of the galvanic current over neuralgia have been gained by small and inconstant single cell batteries. The larger double cells are more steady in their action, but steadiness may not always be necessary even in testing neuralgia, and it is by no means impossible that in some conditions the very fluctuations of the single cell battery may have a therapeutical advantage. Certain it is, that many of the brilliant results in the treatment of diseases of the central nervous system by electricity have been obtained by portable batteries, composed of a moderate number of small, single, and inconstant cells of zinc-carbon or zinc-platinum.

The electric moxa—faradisation with the metallic brush—is a method of treatment which in my hands has not been successful. I have seen the method used for sciatica in Berlin and in France, and I believe that it sometimes accomplishes good things; but it is a most atrociously painful procedure, and I rarely find a private patient in America who will endure it, and for this reason I have almost lost the habit of ever attempting to use it.

Too strong currents have been used, and too much stress has been laid on the differential action of the poles or of current direction. Beginners in electro-therapeutics make mistakes in many directions, but in no one more than in the habit of using too powerful currents. In many diseases and in many temperaments strong currents are harmful, but they are especially so in neuralgia. Better to use a mild current a long time than a strong current a short time; electro-therapeutics cannot be thus concentrated. To attempt to gain time in this way is cruel economy.

In regard to the differential action of the poles and of the current direction, I admit that of course there is a differential therapeutic, as there is a differential physical and physiological action of the poles, but it is far less marked in therapeutics than in physics and physiology. The importance of direction of the current, whether ascending or descending, has been enormously overrated, not only in neuralgia but in all the diseases for which electricity is employed. Pain is relieved, *neuralgia cured, and the* nutrition in every respect improved, by the *passage of the current through the* body by either pole and in any direction, and the differential action of the poles and of the current direction is so slight and so complex that it cannot in the present state of physiological and pathological knowledge be reduced to any formula. I have elsewhere laid down the very general law that in therapeutics the positive pole is less irritating; but even this general statement seems to have many exceptions, in local application at least, for the negative will frequently relieve an irritated nerve as well as the positive, if not better. In central galvanisation and general faradisation, I use, as a habit, the positive pole in making the application to the head or neck, believing this to be, on the whole, the safer.

The treatment has not been used with sufficient perseverance. Perhaps by the use of too strong currents the pain is at first increased; the patient and physician become alarmed and abandon the treatment. This is frequently a mistake; the fault is in the method, not in the medicine. Even when the pain is increased temporarily the permanent effect may be beneficial. Neuralgia is frequently a chronic disease, and like other chronic diseases demands chronic treatment. Neither patients nor physicians should be terrified or discouraged because the first applications appear to aggravate the attacks.

With these general remarks I proceed to speak of some of the special forms of neuralgia.

Migraine.—The prognosis of migraine under electrical treatment is favourable for relief and palliation, but to thoroughly and permanently eradicate the habit is more than we can generally expect. Even relief of the pain during the height of the attack is by no means a uniform result. Here, as everywhere in electro-therapeutics, as much depends on the temperament of the patient, or his general susceptibility to electricity, as on the special disease from which he suffers. The attacks may be forestalled or aborted at the beginning by electrical treatment just as they may be by bromide of potassium, or sodium, or hydrate of chloral, or gelsemium; but even here there is no uniformity.

In some cases the application of a very mild galvanic current to the head or neck at once causes the pain to depart, and it does not return. In other cases the same treatment does no good, and sometimes intensifies the pain. I have been at different times a victim to migraine, and have tried galvanisation before the attacks, while they were coming on, and at their height, and never with success. I have friends similarly afflicted, whom the same treatment for the same kind of attack always more or less relieves.

I think I have observed that migraine, accompanied with gastric disturbances, is more rebellious than when the disease is confined to the head.

Although electro-therapeutics must be based on clinical experience, it is yet an advantage to have some general notion of the pathology of the disease that we treat; and when, as in the case of migraine, we cannot demonstrate the precise nature of the morbid changes in the nervous system that give rise to the symptoms, it is yet desirable to obtain, or to endeavour to obtain, clear and correct ideas of the seat of the disease, even though we can go no further. Our method of applying electricity will be guided, though it should not be controlled, by the theories we hold or the facts that we possess, in regard to the pathology of the disease for which we employ electricity.

Without entering into an extensive consideration of the nature of migraine —a subject which has been well and variously discussed in a series of articles in the Practitioner—I may simply say, in brief, that I regard the disease as a neuralgia closely related to facial neuralgia and gastralgia; that it may be excited by mental influences, by exhaustion, or, reflexly, by digestive or genital disturbances; that nausea and vomiting are the results and attendant symptoms of the disease, rather than the disease itself; and that the seat of the disease is in the pneumogastric and fifth nerves at their origin.

Acting in a measure on these views, which are based on long study of the affection, I treat migraine by my method of central galvanisation, which consists in placing *the negative pole to the epigastrium (the patient holding it by an insulated electrode), while the positive is applied on the top of the head, over the sympathetic in the neck, and down the whole length of the spine, in such a way as to bring the brain, the pneumogastric, the spinal cord, and all the prominent plexuses of the sympathetic, indeed the whole central nervous system, under the influence of the current.* This method is to be used in the intervals of the attacks, with the view of preventing their recurrence or of lengthening the intervals. During the attack, galvanisation of the brain or of the cervical sympathetic and pneumogastric in the ordinary method may be used for merely temporary relief. Faradisation from the machine, that gives a pleasant current, with the hand as an electrode, sometimes acts as well as galvanisation. In all these applications mild currents should be used—so mild as to be scarcely felt, even when the sponges or flannels are saturated with salt water.

It is not indeed necessary that the current should be felt at all, so long as we are sure it is running, and the current should be gradually put on or off. Acting with these precautions, galvanisation of the brain, or of the *neck, is an absolutely safe procedure.*

The mistake of electro-therapeutists is, to apply to the head or neck currents as strong, or nearly as strong, as those that they apply to the muscles in the treatment of paralysis.

During the past few years I have made many thousand applications of the galvanic current to the head and neck of patients of nearly all ages and of every degree of delicacy and susceptibility, without producing any dangerous or alarming effects whatsoever. In two or three instances, through my carelessness in suddenly interrupting the current, unpleasant, though not serious, effects have occurred, which soon passed away.

In using central galvanisation I sometimes move the negative electrode down over the abdomen and the hypogastric region, the positive being on the back, so as to bring the uterus and ovaries under the influence of the current. In cases where there is reason to suspect that the migraine is excited by uterine irritation, systematic electrical treatment of the uterus and ovaries, internally and externally, has been of advantage.

Facial Neuralgia.—This formidable disease yields to electrical treatment only in a certain proportion of cases. The brilliant results recorded by Niemeyer and Reynolds are rare. The milder cases, in patients not too advanced, and brought on by exposure to cold, sometimes are relieved and cured both under faradisation and galvanisation. The more severe and long-standing cases in the very aged may be relieved, but are not usually cured. The exceptions to the statement are, however, sufficiently encouraging to make us try all cases that come under observation. Galvanisation of the affected nerve-branch and of the brain, in various directions, and galvanisation of the cervical sympathetic, are the methods that I employ in this disease. This is one of the forms of neuralgia where it is very easy to aggravate the pain by an incautious use of electricity, and where there is especial need of patience and firmness. The temporary aggravation may be followed by relief, and the patient should not be allowed to abandon the treatment simply because the pain is made worse during the first week of treatment. Patients should be taught that they must give to electricity the same respect that they give to other remedies, and allow both it and the physician who employs it a fair and honest trial. The chances for permanent cure of facial neuralgia of long standing in the aged are perhaps not more than one in ten, if indeed they are as good as that; but such a chance is enough to stimulate our perseverance in every case that appears.

Gastralgia.—The form of neuralgia in which I have found the best results from electrical treatment is gastralgia. Cases where the pain is prevalent and of a most agonising character have been not only relieved for a few hours or days, but permanently cured, so far as can be seen now, by central galvanisation, which is *par excellence* the method of using electricity in this affection.

The two important facts connected with the prognosis of gastralgia under central galvanisation, judging from experience up to the present date, are that very long-standing cases are cured, and the relief is permanent. Two years ago I treated two patients—both physicians—for gastralgia of the most severe character: both were rapidly relieved and completely cured, and both are well to-day. In one of the cases the disease had existed for fourteen years, the attack coming on periodically about two o'clock every night.

In another patient, who for twenty years had suffered from gastralgia and angina pectoris, the cure after a long course of treatment has also been permanent.

Possibly I have been peculiarly fortunate in my cases of gastralgia; but if I were to generalise from my own observation, and from the observation of some of my friends who have employed central galvanisation in this affection, it yields better and more permanently than any other form of neuralgia.

Faradisation, however administered, does not accomplish all that we desire in gastralgia; it relieves, but does not entirely cure. The cases above referred to had all been treated by faradisation, in one instance with some benefit, but not with satisfaction. Galvanisation of the cervical sympathetic and pneumogastric may be used for gastralgia, but these methods have never been my dependence in any form of neuralgia.

Sciatica.—The prognosis of sciatica under electrical treatment, however skilfully administered, is not so satisfactory as is the prognosis of several forms of neuralgia. My experience with this disease does not accord with the highly favourable reports which have been given by various writers.

There is a certain proportion of cases of sciatica that yield readily to the soothing influence of the galvanic current, and after a few applications recover entirely; but such cases are not the rule, although the soothing, quieting, relieving effects are so common as to be almost always experienced. The pain gives way to a proper application of galvanism very frequently during the course of the sitting, yet there is pretty sure to be a recurrence of pain in the course of a few hours, and a very long course of treatment is necessary to make a permanent cure, and there is a certain proportion of cases that never recover, however perseveringly the treatment may be carried out.

There appear to be two reasons why sciatica is thus obstinate.

1. The nerve is larger and longer than any other nerve of the body, and the extent of surface to be treated is, of course, much greater than in other forms of neuralgia.

The disease would appear to be in part peripheral, at least in those varieties that are excited by rheumatism, if not in all the varieties. This view is rendered probable by the very great tenderness, and by the fact that pain is excited by pressure, as in sitting, or by any constrained position of the muscles that cause irritations of the nerve.

2. The nerve, especially near its origin, is so deeply situated that only a very small portion of the current, in external applications, can directly reach it.

My method of treating sciatica is to place one pole—it matters not which—on the spine, near the origin of the nerves that supply the lower limbs, and to pass the other slowly and steadily up and down the whole course of the nerve in the leg, giving especial attention to the tender points. Sometimes I place one of the poles in the hollow of the foot, fastening it there by a cloth or band; but I am not aware that there is any advantage in that arrangement.

I use a current that gives a slightly burning feeling without being positively painful. This method of application will relieve for the time, perhaps for several hours, or for an entire night or day.

Recently I have attempted the treatment of sciatica by *electro-puncture*. The needle, either insulated or not, should be bayonet-shaped, so as to go in easily, and should be inserted by preference at some tender point. I generally try to insert it far enough for the point to touch or penetrate the nerve. The moment when it so touches or penetrates will be revealed to the patient by a tingling or pricking sensation through the leg.

I connect the needle thus introduced with the negative pole of a mild galvanic current, two, four, or six cells, while the positive is applied at the back or some indifferent point. The application should not be made long enough, nor should the current be strong enough, to cause much electrolytic action. It is my custom to use this method for a few moments and then allow the needle to remain *in situ*, to resume the external application for a few moments, closing the sitting by again connecting the needle with a mild current. Thus I combine electro-puncture with the old treatment of acupuncture. I am inclined to think that this method of treatment is more rapidly and more permanently efficacious than external applications. It concentrates the current at the point where it is needed, and perhaps may also act as a counter-irritant. It is probably better that the needle should be insulated up to within a short distance of the point, but I have frequently used uninsulated needles.

The great practical difficulty with electro-puncture is that the introduction of the needle is painful and full of terrors for the sensitive patient.

This difficulty may be overcome in various ways: by inserting the needle quickly and rapidly without forewarning the patient, by using a mixture of carbolic acid and ether, equal parts, or ether spray, to produce local anæsthesia.

But there are some patients who so dread the thought of the needle that they will suffer anything before resorting to it. Two cases of this sort have recently fallen under my observation. One of these was a physician, who is himself accustomed to the use of the electro-puncture, and who has had excellent success with it in sciatica. He had become so reduced by the disease, that when I was called in he refused to be treated by electro-puncture.—*Practitioner, Sept.* 1873, *p.* 187.

29.—CASE OF TRIGEMINAL NEURALGIA TREATED WITH THE CONSTANT CURRENT.

By Samuel Craddock, Esq., Shepton Mallet.

[The patient was 64 years of age, and was seized somewhat suddenly with severe neuralgia of the left fifth nerve. The foci of pain were situated over the malar bone and at the junction of the nasal cartilage and nasal bone; also on the brow, over the supra-orbital foramen.]

The immmediate exciting cause of this attack appeared to be that the patient had stood at an open window during some damp weather that was prevailing at the time. This gentleman had been the subject of severe albuminuria of long standing; it had in fact existed for many years, but there was no evidence of breaking down of the kidney-structure. At the time of the attack the urine was, moreover, very acid, and contained many uric acid crystals; sp. gr. 1025.

The treatment at first consisted in warm fomentations to the face, and the internal adminstrations of alkalies with mild alterative aperients: after a few days he seemed to improve, and the medicine was changed for a mixture containing iron, arsenic, and nux vomica. On October the 26th he went to town and consulted his London physician, who regarded the attack as due to the gouty diathesis, and put him anew upon alkaline treatment. The patient returned to the country on November 19 very much worse; in fact, I never saw anyone suffer so severely from neuralgia. The lachrymation from the left eye was excessive, and there was a copious flux from the left nostril; the nostril was so exquisitely tender that he could not bear to use his pocket-handkerchief. He complained also of severe shooting pains passing through the interior of the eye. These symptoms steadily increased in severity, and the branches of the posterior dental nerve, which are distributed to the gums, became affected, as well as the middle dental branches of the supra-orbital: this was indicated by pain referred to the site of the antrum. All the cutaneous branches of the superior maxillary were now alive with pain, and another focus was developed in the upper lip. The sensory part of the inferior maxillary, distributed to the lower lip, followed suit; and at last, so far as one could see, every branch of the sensory portion of the left trigeminus was more or less affected. The patient became perfectly unable to eat anything at all solid, and lived for two months entirely upon liquids: even these he was only able to take through a reed or a glass tube. It is scarcely necessary to enumerate the long list of remedies which were employed; they included the hypodermic injection of morphia and atropia, but the patient could not tolerate these, and the hypodermic method consequently did not obtain a fair and complete trial. The first remedy that gave any appreciable relief was the external application of aconite liniment, which was done morning and evening, and certainly mitigated the pain: meanwhile, however, the lachrymation and the defluxion from the nose continued in full force, as also the tenderness of the parts. Early in February 1873 *I applied the constant current*, employing a 50-celled Weiss's battery. The current was derived from eight cells, and was applied for a period of five minutes at each sitting. The positive pole was placed at the back of the neck, and the negative applied successively to the several foci of pain, being kept steadily on each for minute or more. This was done daily, at the same hour; after the third application the lachrymation had nearly ceased, and the patient was able to blow his nose freely. From this time the improvement was continuous; mastication was performed easily, and at the end of a fortnight the cure was complete. The patient could not avoid perceiving the remarkable influence of the galvanism, and spoke of it, with the greatest delight, as a "perfect cure." My own satisfaction was equally great, as I had previously, from inexperience of its use, been inclined to doubt the efficacy of the constant current. This case certainly speaks strongly in favour of this method of treating neuralgia, and incidentally supports the pathological theory of neuralgia advocated by Dr. Anstie in his work on that disease.

[The above case interests me greatly: I regard it as a typical example of the mistakes that frequently result from the time-honoured doctrine that neuralgia is usually produced by the gouty diathesis. So long as that theory was acted upon in this case, no really effective steps were taken towards the arrest of the disease, and a very little more continuance in the same course would not improbably have rendered the malady incurable, considering the patient's advanced age. I understand from Mr. Craddock, that there was really no positive reason for supposing that this patient had gout in him: and certainly the mere presence of an excess of uric acid in the urine is no justification for such an assumption. The first remedy that made any impression was the linimentum aconiti, a local application which can only have acted in one way, viz. by temporarily paralysing the peripheral portions of the nerve, and thus blocking the channels by which impressions from without were pouring in upon the irritated nerve-centre. Aconite belongs to a small class of remedies which are often of great service in procuring a momentary pause, during which neuralgic nerves have time to recover their equilibrium a little, while more radical curative measures can be devised.

As regards the action of the constant current, there can be no reasonable doubt that this was genuinely effective, and I beg to call attention to the fact that the direction of the current was what is called "inverse," viz. from centre to periphery. This is contrary to the method strongly advocated by many writers on medical electricity: but the result corresponds with that of inverse galvanisation as seen in several cases under my own treatment. I may especially advert to the cases of a footman (described in my book on Neuralgia) in whom an exceedingly severe facial neuralgia of the first and second divisions of the fifth was found to be perfectly under the control of the constant current, and the effects of inverse and of direct galvanisation were found to be practically identical. I take this opportunity of remarking that evidence is fast accumulating which tends to show that the older ideas as regards the influence of the direction of the current must have been to a large extent erroneous. The opinion of Dr. Reynolds and of Dr. Buzzard coincides with my own upon this matter, and has been arrived at, equally with my own, by sheer experience, in the face of unavoidable prepossessions in favour of the usual ideas as to the respective effects of the two currents.—F. E. ANSTIE.]—*Practitioner, June* 1873, *p.* 338.

30.—SOME OBSERVATIONS ON THE USE OF PHOSPHORUS IN NEURALGIA.

By J. ASHBURTON THOMPSON, Esq., Surgeon-Accoucheur to the Royal Maternity Charity.

[Although there had been previously allusions to the subject, we are really indebted to Mr. G. M. Bradley, of Manchester, for drawing the attention of the profession to the use of phosphorus in neuralgia. (See *Retrospect*, vol. lxvii., p. 51), for three cases successfully treated by Dr. Broadbent with this remedy.]

The following remarks bear exclusively upon cases of nerve-pain, and these have been carefully differentiated from similar diseases. They are arranged in three classes, which are named respectively, acute primary attacks, acute recurrent attacks, and chronic cases. Of the first class there are only six cases, but it is almost characteristic of neuralgia to recur, and it will be interesting to note whether some of the rapidly-cured primary cases are liable to anything like an early return of the disease. The third division of six chronic cases do not present such perfect results as the two former classes; still, if it be remembered in considering them, that two were complicated with phthisis—one being advanced in that disease—while a third was already reduced to a dangerous condition of debility before coming under treatment, I think it will be readily allowed that the result is on the whole eminently satisfactory. In the tables setting out these cases, the conditions and events of them are classified under headings which speak for themselves. Under "Complication," if debility or anæmia be inserted, it has been of a very marked kind. Occasionally the time occupied by the cure seems disproportionate to the duration of the disease; but in all cases ultimately benefited relief followed the first few doses, although the cure could not fairly be considered complete until all soreness or numbness —all symptoms of disturbed sensation—had been removed.

Dose of Phosphorus.—The dose of pure phosphorus is stated in the British Pharmacopœia to range from $\frac{1}{30}$ to $\frac{1}{10}$ of a grain. Dr. Radcliffe and Dr. Broadbent have used the former dose with success; Dr. Anstie with little or none. Dr. Adolphe Waltuch, in his "Dictionary of Materia Medica and Therapeutics," gives the dose as being from $\frac{1}{20}$ to $\frac{1}{4}$ of a grain.

The last quotation alone gives the proper range of dose of this remedy. To prescribe less than $\frac{1}{20}$ of a grain in the first place is to render its therapeutic action apparently variable or uncertain. In some of my earlier cases I am of opinion that the recovery was unduly delayed in consequence of the small quantity with which treatment was begun; at least it will be observed that alleviation of the symptoms commenced much sooner in those cases last under my care. I now invariably begin by giving $\frac{1}{12}$ of a grain every four hours, and this I conclude to be an average dose; it has proved itself a perfectly safe and very efficacious one. In only one case will it be observed that any serious symptoms arose which might perhaps be attributed to the remedy, and in this instance, $\frac{1}{12}$ of a grain was the quantity prescribed. The symptoms appeared, however, to be in a measure critical, and since they have on no occasion repeated themselves, may have rather been induced than originated by the medicine employed. On the other hand, another patient arrived at taking by accident $\frac{1}{4}$ of a grain every four hours, and continued to do so for some days, without harm. On consideration, I conclude that to be on the safe side; and at the same time to secure the proper therapeutic action of the drug, treatment may commence with $\frac{1}{18}$ of a grain repeated every four hours, which should be continued for six doses. If there be now only slight or no alleviation of the symptoms, the dose should be increased to $\frac{1}{12}$ of a grain, and continued at similar intervals. After the latter quantity has been taken for forty-eight hours, I am inclined to think that some good may be effected—*i. e.*, if cure have not already resulted—by increasing the dose still further, either in quantity or frequency of administration; but I do not think that any good will be effected by a larger dose of which $\frac{1}{14}$ of a grain has not given decided promise. I have not, however, had a sufficient opportunity of investigating the action of larger doses.

Of the Mode of Administration.—The mode of administering pure phosphorus is of the first consideration, for two reasons: the ease with which it alters under exposure to certain bodies, and the extreme nauseousness of the fluid preparations.

Phosphorus may be dispensed from its solutions in oil, in ether, in chloroform, in spirit; or it may be given enclosed in capsules or pills (Radcliffe, Broadbent); or in loose chemical combination, as in the hypophosphite of soda (Radcliffe, Anstie), or phosphide of zinc. In the course of the experiments alluded to above, conducted in 1868, M. Dujardin Beaumetz was led to reject the solutions in ether and chloroform, on account of the deleterious effect of the solvent in repeated doses. Spirit he rejected on account of the small quantity dissolved by it—one part in 320, at a specific gravity of ·799. Nevertheless, this is the preparation which I have chiefly used, and *I find that with the aid of heat, ten grains of phosphorus dropped into* 1,000 *drops of absolute alcohol do disappear and are not again precipitated.* M. Beaumetz selected finally *oil of sweet almonds* as the best solvent. This preparation must either be made into an emulsion, in which form it is extremely nauseous, or it may be enclosed in capsules. These were found, however, to deposit after a time an insoluble, and therefore presumably inactive form of phosphorus. The means of avoiding this is to employ oil which has been superheated, which will be found to make a clear and stable solution. No doubt if the capsules and pills now obtainable are guaranteed to be made with oil thus prepared, they afford an agreeable and perhaps sufficiently efficient means of administration ; but I believe it is conceded that there are often advantages enjoyed by fluid forms of medicine which are either largely diluted in the first place, or which admit of easy and rapid solution in the fluids of the body. The pills as recommended by Dr. Radcliffe are made by heating phosphorus and suet together in a close vessel, in the proportion of one part to a 100, making into three-grain pills, and coating with gelatine. The oily solution is a preparation of the French codex (phosphorus 2, oil of sweet almonds 100 ; dissolve with heat), and of the Prussian (phosphorus 1, oil of sweet almonds, 80 ; dissolve with heat). A French formula for emulsion (Soubeiran) is, phosphorus oil 8, gum arabic 8, peppermint water 100, syrup 60 ; make an emulsion.

The four successive cases related by Dr. Radcliffe were treated with the hypophosphite of soda in doses of from seven to ten grains three times a day ; and Dr. Anstie has used the same quantity of this remedy in similar cases of nerve-pain, in which he did not find it successful, although he was satisfied with its effects in cases of chronic alcoholism. It is said to be not unpleasant to take. Phosphide of zinc contains one-fourth of its weight of pure phosphorus ; but two parts of the compound must be administered in order to produce the effect of one part of phosphorus. I have no experience of these forms. Dr. Anstie has used pills containing $\frac{1}{33}$ of a grain of phosphorus ; these were made according to Dr. Radcliffe's formula ; he was dissatisfied with the result. In two cases I used coated pills containing the same quantity, and made by Messrs. Kirby and Co. : they appeared to be without effect, although I gave twice the dose administered by Dr. Anstie, and was afterwards successful with the remedy in other forms. Dr. Broadbent used capsules in his three cases of nervous disorder with good effect.

The discrepancy may now be noted between the results obtained by two competent observers with the hypophosphite of soda used in similar cases. Is this an unstable compound ? Again the results obtained from capsules are opposed to those obtained with pills—which I believe are of the same or similar composition. Can any such deposit of insoluble phosphorus have occurred in the pills such as is indicated by M. Beaumetz ?

I have used the oil (with the action of which also Dr. Anstie was not pleased) in five cases—exclusively, in three. The formula employed was—

Formula No. 1.—Phosphorised oil, 3 drachms ; powdered gum arabic, 6 drachms ; spirits of peppermint, ½ drachm ; water to 6 oz.

I have avoided the use of syrup (as recommended by Soubeiran), or of chloric ether, both of which increase the nauseousness of the mixture. As tincture of orange is the best vehicle for chloral, so spirit of peppermint is incomparably the best vehicle for phosphorus. This mixture keeps reasonably well, and is, I am inclined to think, a very active preparation—perhaps more active than any other. It is, however, so extremely nasty, and, moreover, seems of itself to disagree so with the stomach, that I have not more than one patient who professed to have retained more than fourteen doses.

Tincture of phosphorus should be made by dissolving the material in absolute alcohol with the assistance of heat. The most convenient proportion for dispensing is :—phosphorus, one grain ; absolute alcohol, three drachms. This mixture will, I know, retain its powers for six weeks, although not kept in a full bottle ; I do not know that it loses its efficacy after this period. Before *adding this tincture to* water, it is necessary to dilute it still further with rectified *spirit ; otherwise* hydrated phosphorus will be precipitated. The formula *I have employed is the following* :—

Formula No. 2.—Tincture of phosphorus, 3 drachms; rectified spirit, 2 drachms; spirits of peppermint, ½ drachm; water to 6 oz.

This forms an active, efficient, and convenient preparation. It is not quite disgusting, and will be readily taken, and well tolerated by those who have begun to experience benefit from the first few doses. It is unstable, and should therefore be supplied to the patient in quantities not calculated to last more than twenty-four hours. I have used this mixture in the majority of cases.

Observations.—The first general observation which presents itself is, that chronic cases are cured the least rapidly when they are cured at all; while among them is found the only absolute failure of the remedy employed. This obstinacy may be referred to one of the following causes:—First, the habit of pain into which the nerves have got, and the reaction upon their functional activity which the debilitating effect of constant pain must have; or, secondly, the extreme or particular kind of debility to which the complicating disease has reduced the subject; or progress of a specific nerve disease beyond the possibility of rapid recovery; or, lastly, the necessity for time to recover from the condition of anæmia which in a particular case may be the prime cause of the symptoms. For example, one case was complicated with phthisis, which was advancing slowly but distinctly, and in addition was the true subject of a great deal of depressing mental irritation. Almost constantly for the last eighteen months she had been subject to nerve-pain affecting the cervico-occipital and brachial nerves, and, on bad days, both trigeminal nerves. She was under observation altogether for about three months, being treated in the first place with every kind of remedy, including muriate of ammonia. In my experience this remedy, administered in doses of from one to two drachms every two or three hours, ranks next to phosphorus in efficacy; but it must be given in doses as large and frequent as these; and in those cases which require the remedy—in neuralgia—I do not remember ever to have found it productive of sickness. Finally, since the attacks recurred with added severity every third day, she took large doses of quinine without effect. She was now put upon $\frac{1}{16}$ of a grain of phosphorus dissolved in oil, and she derived some relief from the nine doses which she was able to retain; after that the stomach rejected the mixture. She then took $\frac{1}{12}$ of a grain every four hours in F. No. 2 for about five weeks, when she removed to the country. She was considerably relieved, but not by any means cured.—*Practitioner*, *July* 1873, *p.* 14.

31.—THERAPEUTICAL VALUE OF PHOSPHORUS.

In an interesting paper on the physiological action and therapeutical effects of phosphorus, published in the Bulletin Générale de Thérapeutique, M. Gubler states that phosphorus is a diffusible stimulant of great energy and of dangerous activity. It should therefore only be proscribed with the greatest possible caution, and certain contra-indications may in the first instance be laid down. Thus it should not be used in any affection characterised by nervous, circulatory, or trophic excitation, as in tonic and clonic convulsions, contractions, neuroses having a hypersthenic origin, diffuse peri-encephalitis with general paralysis, phlegmasiæ of all forms, fevers of every kind, exanthematous affections, &c. The indications for its use are the existence of disease unaccompanied by inflammation, fever, and nervous excitation, and especially in such cases as are characterised by depression of the circulation, either local or general, diminished power of generating heat, exhaustion or local asthenia, with paralysis of sensation and movement. Hence its value in cachectic states consecutive to long and exhausting diseases, marsh fevers, protracted convalescence, tabes dorsalis, paralyses of old date, and of cerebral, medullary, or peripheric origin, when there are no signs of irritation; in hemiplegia, paraplegia, amaurosis, and other partial paralyses. Phosphorus again is sometimes useful in making chronic eruptions advance or recede; but it is especially as a remedy for impotence that it has been praised, though it has often disappointed the expectations of those who have prescribed it, and has either proved of no value at all, or its effects have only been ephemeral. Hence it would appear that the real remedial power of phosphorus is considerably restricted, and that it can only be regarded as of great value in paralytic affections. Dr. Delpech, who has studied so deeply the effects of sulphuret of carbon, praises phosphorus highly as an agent to remove the paralysis and loss of power which accompany intoxication by that substance. It is also of service in the so-called rheumatismal and hysterical paraplegiæ, or in other words those forms of paraplegiæ which are not caused by organic lesion, as well as in cases of cerebral disease in which all irritation has ceased and cicatrisation has taken place. It is still more strongly indicated in cases of asthenic and diffused paralysis, consequent on diphtheria or some other acute affection. Gueneau de Mussy, Isambert, and Féréol have all found it efficacious in the treatment of mercurial tremor; it is also believed to be so in paralysis agitans and in the various forms of medullary sclerosis affecting the antero-lateral cords, and above all in sclerosis of the posterior columns, the symptomatic expression of which is summed up in the term locomotor ataxy, which has been applied to it by Duchenne. Dujardin-Beaumetz is he who has most strongly recommended the plan of treatment by phosphorus in this and similar affections; but it is questionable, M. Gubler thinks, how far many of the successes attributed to its uses are really due to its remedial powers. We forget, he says, the natural processes of cure that often take place. Phosphorus is an active agent that may momentarily re-illumine the fading spark and revivify the languishing powers of life; but as it brings no energy with it, it impoverishes rather than enriches, and can do little for a nervous system exhausted by a chronic affection. The amorphous phosphorus is perhaps the best mode of prescribing it, as this possesses no exciting or irritating action. Externally it has been chiefly employed in squamous affections of the skin as a parasiticide in itch, and as a caustic in the place of the moxa. Recently Tavignot has declared that it will render the cataractous lens transparent, but the negative facts obtained by MM. Gosselin and Maisonneuve render this more than doubtful. In regard to the mode of its administration, solutions are usually preferable to pills. Amongst the former are the ethereal tincture which contains one part in sixty, and of which ten drops are a dose; the solution in chloroform, which is now almost abandoned; and the solution in oil, which is by far the best, and especially that prepared by Méhu. Here the oil is dehydrated and decolorised by exposure to a heat of 250° C., and the phosphorus is added when it has cooled. A twentieth part of ether is then added. The proportion is one part of phosphorus to 500 of oil, and it contains two milligrammes in fifteen drops. Each capsule contains one milligramme.—*Practitioner, July* 1873, *p.* 46.

32.—SLIGHT POISONING FROM THE MEDICINAL USE OF PHOSPHORUS.

By Dr. Anstie.

At the present time, when attention is being widely directed to the efficacy of phosphorus in nervous diseases, it may be useful to record every incident that throws light on the as yet undecided question of dosage.

A gentleman, aged 33, single, applied to me first in May 1872. He had been subject to migraine once a month (or oftener, under circumstances of special fatigue) in boyhood; but the pain was more or less diffused, and not very evidently neuralgic in type, till the age of nineteen. Since the latter date it has been strictly confined to the left side of the head (anterior two-thirds); occasionally it extended into the globe of the eye, and sometimes slightly invaded some descending cervical twigs. It never was felt in either *maxilla.*

The family history was decidedly bad. A brother was neuralgic; the mother died mentally deranged; a sister is partly insane.

Up to December 1871 the attacks of pain were always attended with vomiting; in that month there was a very bad attack of typical sick-headache, and then the vomiting altogether ceased to occur. Before the change thus mentioned the attacks were always preceded by vertigo; since that date the vertigo, like the sickness, ceased to appear. There never has been any marked lachrymation; but it is noteworthy that since the attacks have become more like ordinary neuralgia than migraine, the tendency to conjunctival congestion has markedly increased. On questioning the patient as to the nature of the vomiting that formerly occurred, he was quite clear in the statement that no solid food was ever thrown up; the sickness always took place in circumstances of fasting, and the vomited matters were watery and sour. He has not been blessed with a very good appetite; but, on the other hand, there is no history of anything that can be called dyspepsia.

As regards the remedies which had so far been employed, he had taken (among other things) quinine, iron with strychnia, and also arsenic; the latter did him more good for a time than anything else. He had for some years been in the hands of a very intelligent practitioner, and had not suffered from the injudicious kind of treatment to which, unfortunately, migrainous patients are too often subjected.

In advising on this case, it became important to consider the circumstances of the patient's life. He was engaged in a business which often occupied him for many hours in a chiefly sedentary manner, and had likewise evidently undergone much anxiety and worry. I suspended, for the time, the question of his taking an entire leave of absence from business for a prolonged period, but insisted on his taking things more easily. He was recommended to take a steady course of arsenic and cod-liver oil, and to increase his allowance of food.

Early in May of this year the patient's ordinary medical attendant called me to him in consultation. It appeared that he had been better, on the whole, during the past twelve months; still, he had not shaken off the neuralgic tendency, and it became a question whether any further medication should be tried, or whether he should at once be sent on a prolonged sea-voyage. I was anxious to give phosphorus a trial; and on May 5th, accordingly, our patient commenced taking, thrice daily, a pill containing $\frac{1}{30}$th grain. On July 8th his medical attendant brought him to my house, with the following interesting history:—

From May 5th to May 11th or 12th he took the pills regularly, but during the two last days suffered from a constant and increasing burning sensation at the epigastrium. What led to the suspension of the phosphorus was the fact, to which the patient himself drew his medical man's attention, that the urine was dark-coloured: on examination it proved to be both albuminous and bloody, but very careful microscopic examination failed to detect any casts. After a day or two's interval the renal symptoms had almost disappeared, and the stomach was much easier, though the patient felt (and, indeed, still feels slightly) a rather burning pain between the scapulæ. As to the neuralgia, this is remarkable:—On the whole, he had suffered more during the phosphorus-medication than previously, but it was evident that some powerful influence had been exerted on the nervous system, for the rhythm of recurrence of pain was quite changed. There were now two exacerbations daily, but the pain was altogether more of a continuous and less neuralgic type. On the 14th, after two days' rest from the phosphorus, he took one five-grain dose of quinine about an hour before the morning exacerbation was expected, and, singularly enough, never had a recurrence of actual pain afterwards, though there were a few occasional threatenings. He went to St. Leonard's for a little while, and this seemed to help him; and on the 8th July, when brought to me, he was quite free from his neuralgia; but, though fatter than he had been, still undeniably thin. A stranger to the history of the case might suppose that the single dose of quinine had been the cause of the cure; but this seems impossible, for he had been saturated with quinine by his own medical attendant (before I saw him first) without the slightest benefit. At any rate one may say that the quinine would probably not have done him any good, had not a strong impression been previously made by the phosphorus.

I think that it is very evident that poisoning by phosphorus did occur in this case. The burning pain in the stomach, together with temporary hæmaturia, at once relieved by discontinuing the medicine, seem to leave no doubt of this; and thus we must acknowledge it possible that a patient may suffer the slighter degrees of phosphorus-poisoning from less than three-quarters of a grain in minute doses distributed over seven days. At the same time, in view of Mr. Ashburton Thompson's use of the drug in much larger doses, for a large number of patients, without any bad result, we must suppose that the occurrence is quite exceptional, and due to an unusual susceptibility.—*Practitioner*, *Aug.* 1873, *p.* 103.

33.—ON CHOREA.

By Dr. Francis E. Anstie, Physician to Westminster Hospital, &c.

By far the commonest form of ordinary chorea is that which we see in young children, say of eight or ten years. There is usually something delicate in the appearance of these patients; yet this is by no means always the case, and not a few of the sufferers have actually the appearance of blooming health. The choreic movements begin, in the majority of cases, on one side of the body, and most commonly in the arm. Moreover the left side is far more frequently the seat of the original attack than the right; and in the wards you will very frequently see choreic children sitting in a characteristic attitude—namely, grasping the left wrist with the right hand, to prevent the movements of the left arm. Gradually the movements spread to the leg of the side first affected; and often about this time slight facial grimaces, and momentary jerkings of the neck-muscles, with perhaps some little affection of the tongue-muscles, are noted. The tongue may be protruded with a jerk—a symptom which has been much dwelt upon, but is not so common as many writers would have us to suppose. In a very large number of choreic cases there is no tendency at all to spasmodic protrusion, but rather to a partial disability in all the motions of the tongue. Stammering, which is a not unfrequent symptom, must be discussed at a later stage of this lecture. In a large number of cases the movements remain limited to one side of the body: this is called "hemichorea."

The convulsive jerkings, however, although they are the symptoms which most attract the eye, by no means indicate the whole extent of the affection even of the motor nerve-supply. If you test the power of any given muscle or group of muscles which are choreic, you will find it notably enfeebled, the degree of weakness corresponding pretty accurately to the amount of choreic affection. In the case of the leg (especially in hemichorea) this weakness is often plainly shown by the patient's dragging the limb in walking. It is remarkable that, as a rule, the symptoms of diminished power vanish simultaneously with the abnormal movements; though you will hear of exceptions presently.

When we are able to gather exact particulars of the illness from its beginning, we commonly find that the invasion was very gradual. It is true that violent emotion, especially fright, does often precipitate, or apparently even altogether produce, a choreic attack; but I doubt not that by far the most of these patients were already predisposed to chorea, and that many of them had actually had slight and unnoticed symptoms before the occurrence of the shock which is regarded as the exciting cause. Often we find that the system had been previously weakened by an acute illness, and most frequently this illness was acute rheumatism, the connexion of which malady with chorea is so important that we must presently investigate it separately.

In the simplest cases of chorea, such as we have just been considering, there is a very strong tendency to spontaneous recovery. A great number of these patients pass through all the stages of their malady in some four, six, or eight weeks, even if nothing be done in the way of medicinal treatment. But if, from one cause or another, the disease remains substantially unchanged at the end of three months, it must be reckoned (as Jaccoud well observes) among the obstinate choreas, and is likely to give much trouble unless we are able to interfere with it by some decisive treatment. It is especially unfortunate if chorea drags on towards the time at which puberty naturally begins. The already enfeebled nervous system, caught in the new storm which now agitates the organism, is nearly certain to relapse, and perhaps more severely than before, into the ataxic manifestations of chorea.

This brings us to the consideration of a second variety, in which chorea evidently takes its first start from the changed condition of things which has been brought about by the establishment of puberty; and I beg your particular attention to the important differences which separate this variety of the disease from that which has been already described. Chorea which first takes its rise in the turmoil of sexual development is not only far more likely to prove severe, intractable, or even dangerous to life, than the earlier disease, but has a considerable tendency to be followed by permanent damage to the nervous system, which may show itself in other than choreic phenomena. It is among this class of choreic patients that we first meet with those severe and alarming cases which are spoken of *par excellence* as *acute* chorea. In these the muscular movements are neither slight nor limited; on the contrary, they speedily involve the whole voluntary muscular system, and assume the most pronounced character. Limbs, head, and trunk are in perpetual and violent motion; walking or standing is impossible, and the patient can hardly be kept in bed without being fastened down. The mental agitation becomes excessive, and sleep is impossible. In the worst cases the incessant chafing of the limbs against each other, and against surrounding objects, produces sores, inflammations, and even extensive suppurations, that add greatly to the misery and exhaustion of the patient. Meantime she (for it is almost always a girl) cannot swallow, and unless feeding can be regularly performed per rectum, the mere starvation will quickly prove fatal. Delirium, of a more or less maniacal kind, attends such cases towards their termination.

Besides these extremely severe cases, in which the tendency is towards a speedily fatal end, the chorea of sexual development occurs in less pronounced but still very intractable forms. Should there be considerable difficulties in the establishment of a regular and sufficient menstruation, and especially if there is a tendency to neuralgic ovarian dysmenorrhœa, the case is very apt to become complicated with hysteria; and, once the hysterical attitude of mind is superadded to the chronic ataxy of muscles, the patient is placed in a situation, in which the nervous system will have great difficulties in reacquiring a healthy equilibrium. I shall have something important to say presently, under the head of etiology, as to the extremely disastrous influence of conscious sexual irritation upon the choreas of early sexual life; and I may here remark that such irritation may induce puberty precociously, and that the unhappy children whose minds have been unnaturally turned thus early to sexual ideas are apt to have chorea, if at all, in a form which resembles that just described as pertaining especially to a later period.

There are two other incidents of sexual life which, though in a feebler degree than the development of the sexual organs and feelings, may provoke chorea—namely, pregnancy and the "change of life." It is not often, I believe, that first attacks of chorea occur under either of these circumstances, unless some powerful emotional cause be also at work. Usually the chorea of pregnancy and the rarer chorea of uterine involution, are mere reappearances of a tendency which had already manifested itself in childhood. But occasionally we meet with persons of neurotic temperament and family antecedents, in whom the state of pregnancy keeps the nervous system in so unstable a state that the occurrence of any physical, or especially any emotional shock at once evokes choreic troubles. Occasionally it has happened that the choreic agitation was exceedingly severe, and either killed the patient or induced premature labour.

On the whole, then, we may clinically divide the choreas proper into two main groups—those of childhood and those of sexual life. (One might add a third group of cases, extremely small in number, which occur in advanced life, and are evidently a consequence, though an unusual one, of the degenerative processes peculiar to *old age*.) *The choreas of childhood are mainly of a mild and curable kind, though with* a tendency to relapse, sometimes two, three, or *four times, but usually ceasing* altogether before very long. The choreas of sexual life, are, as a rule, more deeply rooted in the nervous system, are apt to be more extensively complicated, and have in most cases a more serious prognosis than the former class.

The pathology and causation of chorea form one of the most puzzling subjects that medical inquiry has ever encountered. When we have stated the very obvious facts that chorea is a disease of some portion of the central nervous system, and that the circumstances evidently favouring its occurrence are all of them of a debilitating kind, we have said very little, as you will allow. It is something to know, in addition, that chorea is very strictly bound up with the inherited disposition to neurotic disease. But when we have got so far, we are confronted with the most formidable difficulties, which may be expressed by the following questions:—(1) Is the pathological seat of chorea always the same, and if so, is it spinal or encephalic? (2) Is the pathological process always of the same general nature, and if so, of what nature? (3) What is the degree and kind of connexion between rheumatism and chorea?

1. As to the pathological seat of chorea. The older belief, that chorea was dependent upon spinal cord irritation has of late years been pretty much set aside, more especially in this country. Dr. Reynolds some twenty years ago brought forward a very able series of arguments to prove that the phenomena did not correspond with the idea of a spinal origin of the disease: he argued that the persistent *clonic* character of the spasms, the partial control which the will has over them, the cessation of the movements during sleep, and the fact that emotional changes and attempts at volitional action induce or aggravate the spasms, are totally opposed to the spinal theory. Dr. Broadbent followed this up a few years ago, by adding to Reynolds's arguments the following:—Tickling of the hand or sole of the foot does not exaggerate choreic movements—indeed, there sometimes seems to be a lessened reflex irritability. And, again, the more or less unilateral character of the affection, in such a large number of choreic patients, is highly inconsistent with the spinal theory. The whole argument taken together seems to point rather strongly to the view originally put forward by Reynolds, that the pathological seat of chorea is the sensori-motor ganglia at the base of the brain, the corpora striata and optic thalami.

To this opinion as to the seat of the pathological change, Dr. J. Hughlings Jackson also agreed, but, as I shall have to mention presently, he adopted a theory as to a uniform pathological process in the encephalic sensori-motor ganglia which is not endorsed by Dr. Broadbent.

Not to dwell in detail upon the various doctrines that have been expressed, I may mention that M. Jaccoud holds, and very ably states the opinion, that a certain portion of the spinal cord is the true seat of chorea. He remarks that chorea is a perversion of muscular co-ordination; and he expresses the conviction that there is no *single* centre of co-ordination, but that the "elements" for that function extend the whole length of the spinal cord, as far as the pons Varolii. It is this apparatus which, according to Jaccoud, is morbidly affected at some point in its length in the majority of cases. The few examples of chorea in which he will admit the possibility of a primary lesson in the sensori-motor ganglia are to be explained on the theory that the encephalic lesion only takes an indirect part in the production of the chorea.

2. As regards the nature of the pathological process which produces chorea, there have been various opinions. The number of opportunities for post-mortem examinations of persons (either actually or within a short distance of time) suffering from chorea is very limited. In a considerable proportion of those which have been made, no lesion whatever of any importance or significance has been discovered; though, of course, in many of the old examples there is the possibility that the observer was incompetent to discover the lesion. Until comparatively recent years the principal lesions which had been observed were congestions of cerebral or spinal meninges; subarachnoid spinal effusion; and (occasionally) patches of softening in the brain. But for some few years past one theory has occupied the principal attention of the scientific world—viz., the embolic theory of Dr. Jackson. This author supposes that chorea is linked essentially with *embolic obstructions of* some of the bloodvessels supplying the corpus striatum; *the effect of this interference* with the supply of nutrition being to create an *instability of voluntary* movement which is a minor degree of that complete

interference which would be represented by paralysis. The embolic theory of chorea has attracted a remarkable degree of attention. It was elaborated in a most careful manner, and its author has connected it, in a very ingenious way, with similar explanations of other phenomena presumably dependent on lesions of the corpora striata and the neighbouring parts. Of late, however, it has been widely felt that this theory is incapable of universal application—that it can, at most, but explain a comparatively limited number of cases. There is one objection urged against it by Jaccoud, which, though stated perhaps in too absolute a manner, is highly important. Jaccoud says the chorea, when unilateral, is nearly always left-sided; whereas embolism of the cerebral arteries by far the most frequently occurs in the left corpus striatum or its neighbourhood, so that the resulting chorea should be *right-sided*, which is just what it is not, except in a minority of cases.—*Lancet, May* 10, 1873, *p.* 655.

DISEASES OF THE ORGANS OF RESPIRATION.

34.—ON THE TREATMENT OF ASTHMA.

By Dr. C. T. Williams, M.A., Physician to the Hospital for Consumption and Diseases of the Chest, Brompton.

The treatment of spasmodic asthma is by no means easy, and in a disease where the modes of causation are so varied it is difficult to lay down rigid rules. The grand principle should be: Avoid all exciting causes of the attack, and place the patient under conditions most likely to allay, and keep allayed, spasm. The treatment may be divided into *climatic* and *medicinal;* the former being, as a rule, vastly more important than the latter. We have to discover a climate in which the patient can *breathe*—no easy task, indeed, in a disease the course of which follows neither rhyme nor reason, and often seems to depend wholly on the idiosyncrasy of the individual.

The only safe guide to follow in obstinate cases is the doctrine of contrasts, and, whatever the climate of the locality be where the disease has been contracted, to boldly resort to its very opposite: should it be a damp one, to resort to a dry one; if a dry one, to a moist one; and if inland, to the sea, and so forth. High localities suit some asthmatics; sea air a few; cold places some; warm localities others; but the atmosphere in favour of which the strongest evidence exists is that of populous towns, and the more smoky and drier the town the better for the asthmatic. Trousseau and Sée give marked instances of asthmatics who were cured by quitting their country homes and residing in Paris. Salter enumerates many striking examples, and even concludes that the worse the air is for the general health, the better for the asthma. Thus he considers the worse parts of cities—*i.e.*, the least hygienic—the best for asthma; and, conversely, the best or most hygienic parts the least suitable for asthma; and in London he prefers the City to the West-end.

With reference to the advantages asthmatic patients derive by removing to the crowded portions of cities, a well-known physician of Nice furnished me with a good instance. An asthmatic English nobleman, who had resorted to the sunny south for sunshine and balmy air, took up his residence in a beautiful villa on the Promenade des Anglais, facing the sea. The situation was excellent, and the hygienic conditions as good as can be obtained at Nice; but he could not breathe, and spent night after night in great and breathless anguish. He had made up his mind to quit the place, and was one day wandering about the old town, a closely packed, ill-smelling neighbourhood, in search of a hair-cutter, when he espied a small and by no means tempting-looking shop of the sort. He entered, and in the somewhat ill-ventilated, low-ceilinged room found to his astonishment that he could breathe with comfort. He gave up his grand villa, took up his quarters in the barber's first floor, slept soundly, and ceased to wheeze.

Without going so far as Dr. Salter, I may say that I have found the air of London beneficial to by far the majority of my asthmatic patients, and I consider this the more remarkable because the localities they have quitted have been very dissimilar. Some come from high table-lands with little or no vegetation, others from deep valleys abounding in trees, some from damp river sides, some from the sea-coast, some from hot, some from cold climates. Even in town atmospheres asthmatic patients show themselves extremely sensitive to slight differences of situation; some can sleep in a back room and not in a front one of the same house, others in a garret better than in a first-floor, while a few feet of rise or fall in a city has been known to make great difference. All densely-populated smoky towns appear to exercise this good effect; for instance, Manchester, Leeds, Birmingham, Glasgow, have furnished instances of asthmatics improving in their atmospheres with little aid from medicine. It has sometimes surprised me to witness severe attacks, which have lasted in the country for days and weeks, subside in a few hours in the much-abused atmosphere of our metropolis.

Now, what do we know about the air of cities? How does it differ from the pure air of the country? First, it is drier than that of most country places; secondly, it contains, according to Dr. Angus Smith's careful analysis, more carbonic acid; thirdly, it contains less oxygen. Dr. Smith made numerous analyses of the air in various parts of the metropolis, in each postal district, near the river-side, in the parks, in the most densely-populated portions; and in the subjoined table I have given some of the results which appeared to me to bear most closely on our subject. I have compared them with those of Glasgow, and, what is more to the purpose, with Dr. Smith's admirable analysis of specimens of air taken from the summits of nine or ten of the principal mountains in Scotland (the wind not being strong at the time of observation), as well as with the air of the Scotch seaboard and of open heaths of no great elevation.

	Carbonic acid, per cent.	Oxygen, per cent.
Mountains of Scotland (average)	·0332	20·98
Seashore and open heaths “	—	20·99
Glasgow “	·0502	20·90
London “	·0439	20·88
“ near London Docks (maximum)	·0528	20·76
“ E. & E. C. Districts (average)	·0474	20·86
“ W. & W. C. Districts “	·0411	20·92

This table shows a decided increase in the percentage of carbonic acid, as well as a diminution in the oxygen per centage, present in the atmospheres of Glasgow and London. The differences in the air of the various postal districts are much what experience would have indicated, the air becoming purer as we advance to the west in consequence of the large extent of parks and open spaces. The differences in the amounts of oxygen and carbonic acid, when taken in relation to the whole per centage, are by no means great, and are only to be found in the first or second place of decimals. But when we remember the small per centage of impurity which suffices to render drinking-water injurious in spite of the disinfectant power of the gastric juice, and the usually small amount of water swallowed, can we wonder that the lungs, in the course of respirations 16 to 20 in the minute, and unprotected by any filtering apparatus, may introduce in the course of a whole day a sufficiently large quantity of a certain gas to have a decided influence for good or evil, even though the per centage of the said gas may be comparatively small? Dr. Angus Smith says: "We take into our lungs 1000 or 2000 gallons of air daily. The addition of $\frac{1}{2000}$ at each inspiration will give us fifteen grains in the day—an amount by no means to be overlooked." This excess of CO_2 in the London atmosphere, combined with the diminished amount of oxygen, may have a deadening effect on the pulmonary mucous membrane, and render it less sensitive to slight excitants. If it is not the excess of CO_2, or the smaller amount of O, it may be the unconsumed carbon, or some other component of town air which dilutes and renders it a less potent draught to the asthmatic lungs than the pure, fresh champagne-like air of the mountains or open country.

The late Sir James Simpson was in the habit of having a certain amount of carbonic acid mixed with the atmosphere inhaled by asthmatics, and the baths of St. Moritz and Ems and others are sometimes used on account of the carbonic acid which the waters give off.

The great difficulty with regard to introducing a certain amount of carbonic acid into the atmosphere is the regulation of the quantity introduced, for too great care cannot be taken on this point, as a very small excess of carbonic acid may prove fatal. Martius concludes, from experiments, that carbonic acid is the principal result of the burning of the popular remedy of nitre paper, and if this be the case this is one of the safest methods of administering this gas.

The *dietetic* treatment of asthma must be to a certain extent arranged to each patient's particular case, but, as a general rule, the earlier meals should be made the fullest, and but little solid food taken after two o'clock. Stimulants should be of the kind least likely to cause flatus in the stomach and intestines.

The *medicinal* treatment of spasmodic asthma must to some degree depend on its causation, and, in fact, on how far the cases are simply neurotic, or complicated with, and dependent on, inflammation, or, again, on a morbid state of the blood.

All the local causes, with the exception of inflammation, produce a form of asthma which is a simple neurosis resembling neuralgia ; but inflammation of the bronchiæ, as a rule, leaves behind it some organic lesion, against which it is necessary to direct our treatment if we hope for any success. It will often be found in these cases that the treatment of the catarrh or of the inflammatory symptoms accompanying it will considerably reduce, if not abolish, the spasmodic attacks, as the irritation of the vagus is dependent on the former. The combination of salines with small doses of such sedatives as stramonium, belladonna, and henbane, followed up by iodide of potassium, generally answers well, and these cases are perhaps the most manageable of any.

Again, where asthma is brought about by general causes acting through the blood we have not a pure neurosis to deal with, and our treatment must be directed to the particular morbid state of blood existing, whether it be gouty or syphilitic, or that producing the various skin eruptions. It is in this last form of humoral asthma, as it is called, that arsenic proves of such signal service, especially if the case be at all of a chronic order. It not rarely happens that both the asthma and the skin disease are cured by a steadily pursued course of the drug.

The great majority, however, of cases of spasmodic asthma are purely neurotic, and, like all neuroses, most difficult to treat, the results of drugs on different individuals varying greatly. On one belladonna acts like a charm ; on another, by causing dryness of the fauces, it only increases the spasm. Some patients swear by stramonium, others by lobelia ; a third class find relief in emetics. There is perhaps no department where empiricism is supposed to hold its sway more absolutely than in the treatment of neurotic asthma ; for what between the pills, the papers, the cigarettes, and the pipes, all of which are warranted certain cures, the humble inquirer about the action of drugs may well be perplexed. Though deprecating any attempt at an elaborate classification of the so-called antispasmodics, I would suggest that those of greatest use in the treatment of this kind of asthma may be arranged as follows :—

1. *Stimulant antispasmodics.*—A certain number of asthmatics gain relief by sulphuric ether, by strong coffee, and even alcohol. One of the most recently introduced of this class is nitrite of amyl, which, by acting as a violent cardiac stimulant, sometimes relieves the spasm.

2. *Sedative antispasmodics.*—This is an almost endless class ; and if the sufferer perseveres he generally gets relief from one or more members of the group, which includes datura, stramonium, tatula, belladonna, hyoscyamus, tobacco, lobelia, Indian hemp, and others. With regard to stramonium, belladonna, and henbane, their use was dictated, as is well known, by the experiments of Dr. C. J. B. Williams, who found that in animals poisoned by these drugs the bronchial tubes *were dilated and incapable* of being excited by any stimulus, and

presented a marked contrast to the results of opium-poisoning, where the bronchial tubes were found contracted after death. Both stramonium and belladonna seem to act most beneficially in the slighter cases; but where they cause dryness of the throat, or even delirium, they seem to have little effect on the bronchial muscle. I found far more satisfactory results follow the use of the extract of stramonium made from the seeds, in quarter-grain to half-grain doses, than in smoking the cigarettes of the leaves, which however have a high repute. Lobelia is a valuable agent when used in sufficiently large doses; but to do much good it must be administered, not in doses of from ten to thirty minims, as is often done, but a drachm at a time, and repeated every three or four hours until some effect is produced. Tobacco when smoked has a sedative influence, but when taken into the stomach is a very unsafe remedy, it being necessary to push it to the extent of producing vomiting before relief comes, and ofttimes alarming symptoms of failure of the circulation to follow. Indian hemp has been used, and with a certain amount of success, but it occasionally gives rise to curious head symptoms, and therefore requires careful watching.

Inhalations of various kinds, and smoking the different cigarettes often do good if the spasm be not very severe; but when this is the case we must expect them to fail, as they generally do, from the great difficulty of introducing them into the system. It is then that hypodermic injections of morphia come in useful, and, by inducing slumber, relax the spasm. The dose should be from one-sixth to one-fourth of a grain, repeated from time to time, and their use is at once contraindicated by any blueness of the face or signs of obstructed circulation.

In very obstinate cases you will find that sometimes all drugs fail, chiefly on account of the difficulty of introducing them into the system, and the fit gradually wears itself out, generally through the carbonic acid accumulating in the lungs and inducing anæsthesia of the mucous membrane, and thus relaxing the spasm.

I can testify highly to the benefits of chloroform inhalation in the worst cases, in many of which it has acted like a charm, and sent the sufferer into a calm slumber, from which he has awoke free from dyspnœa. In others the relief is temporary, and the spasm returns as severely as ever. The objections to the use of chloroform seem to me to be twofold. First, it sometimes in small quantities—*i.e.*, less than one drachm—causes intermittence of the pulse; second, it cannot with safety be entrusted to the patients themselves.

For these reasons I determined to follow Professor Biermer's example, and try the nearest and safest approach to chloroform—viz., chloral—and I selected for the purpose several cases where the fits were of long continuance, and marked by only short intermissions. The chloral was given in doses of from fifteen to twenty grains, in an ounce of peppermint water, every three or four hours. The effect in almost every instance was that the patients fell fast asleep after the first dose, and slept in a recumbent posture for a few hours, which they had not been able to do for days and weeks. On awakening there was a tendency of the spasm to return, which was generally obviated by two or three repetitions of the large doses. The breathing gradually became quite free, except when considerable emphysema existed, and here the asthmatic spasm was removed, and only the habitual dyspnœa remained. The doses of chloral were then reduced, and gradually discontinued.

Having succeeded with the most troublesome form of asthma, I next tried the drug in cases where the attacks, though severe, are far more periodic, appearing at a fixed hour every night, and completely disappearing the following day. Here the chloral seemed still more successful, as a large evening dose generally afforded a good night's rest, and steady perseverance with the night draughts seemed to break the habit of the attacks, and eventually to get rid of them altogether. After this I have tried chloral in most forms of spasmodic asthma, and it is not to be wondered at that it has become a favourite prescription in my wards.

In the twenty cases in which I have used it, speedy relief has been obtained in all. In two this has only been temporary; in the other eighteen permanent. *In three cases* where the scruple doses have been repeated several times, the *patients complained* of queer sensations in the head, and in one of these, a very obstinate case, where the chloral was continued for days, the patient complained of feeling muddled. Vomiting came on, which disappeared on the appearance of a purpuric eruption on the legs. The drug was of course discontinued, and in a few hours the wheezing, which had ceased, returned. This was the only instance of bad effects following.

I will conclude by strongly urging the claims of chloral to a fair trial at your hands in the treatment of neurotic asthma.

A word on *prognosis.* Patients do not die of asthma, although they sometimes seem on the verge of suffocation. The question of recovery is to a certain extent a question of (1) age; (2) of whether the attacks increase or not in frequency; and (3) what the state of the chest and breathing is in the intervals. If the patient be young—say under fifteen—the chest well formed, the attacks diminishing, and the chest free in the intervals, a most hopeful prognosis can be given. If, on the other hand, the patient be over forty, the attacks increasing in number and severity, and the breathing more or less short in the intervals, the prognosis is unfavourable.—*Lancet, Oct.* 11, 1873, *p.* 515.

35.—ON THE USE OF ERGOT OF RYE IN THE HEMOPTYSIS OF PHTHISIS.

By Dr. FRANCIS E. ANSTIE, Senior Assistant Physician to Westminster Hospital, &c.

I very early passed from the stage of implicit belief in styptics to a total distrust of them (as internally administered); but for some years past I have seen reason to think that their power is real, though far more limited than many persons have supposed it to be. That the action of the more commonly employed internal hemostatics is to the last degree uncertain (even though in a moderate number of cases they may apparently act with great promptitude and effect), I think no one of large experience will deny, unless he has entered upon the inquiry with the determination to see none but successful results. With regard to some remedies which (known to be *locally* more or less styptic) are continually given internally for hemorrhage, by the merest routine, he will quickly make up his mind. He will conclude that, for certain, the mineral acids, in any practicable internal doses, are not directly hemostatic at all, and are not in the least reliable when the momentary object is to stop the bleeding. And probably he will in the end conclude that, putting aside the case of digitalis, which is peculiar, there remain, among the ordinarily reputed remedies for hemoptysis, only (1) gallic acid, (2) acetate of lead, (3) either of these combined with opium, (4) alum, (5) turpentine, and (6) sesquichloride of iron, which yield anything like decisive results, while each of them in turn is very apt to fail. Certainly he would conclude, without the least hesitation, that we are ignorant of any but the vaguest clues to the discriminative use of internal styptics; yet there must be serious differences in the applicability of different forms of hemorrhage. To take a very striking example, what do we know of the reason for the difference between the magical styptic action of turpentine in hemophilia, and even in purely symptomatic purpura, and its capricious (though occasionally good) results in ordinary phthisical hemoptysis?

On the other hand, there is, in the case of ergot, good *à priori* ground for supposing that it would prove to be a more universally applicable remedy in hemoptysis than any other that has been named. Proofs of its general physiological action have accumulated on all sides of late years, and we know with certainty that, besides its familiar action on the uterine walls, it contracts the smaller arteries, and slows the heart. In these respects it would seem likely to check pulmonary hemorrhage in the same way as digitalis does, though probably with more vigour, in both these directions, than would be shown by any safe and practicable doses of digitalis. This leads me to those special remarks on digitalis as a *pulmonary hemostatic* which I had reserved till now. I think *that any person who has patiently* tried digitalis, both in ordinary hemoptysis and also in pneumonia according to the German system, will readily acknowledge its very decided power of diminishing the blood in the sputa in both diseases; and I believe that a careful observer would convince himself that this occurs in direct proportion to the degree in which the heart's action is slowed, *provided it continues regular*. But whatever may be the ultimate conclusion as to the working of digitalis in pneumonia, it certainly is an inconvenient agent to rely upon in the suppression of hemoptysis, for though very powerful *ad hoc* it produces in an inconveniently large number of cases a temporary fluctuation in the force and regularity of the heart's pulsations. This is very disquieting, and sometimes alarming to the patient, and is likely to more than neutralise any direct benefits from the drug. Now, with regard to ergot, I can affirm with great confidence that there is nothing to be apprehended. I commenced the use of this drug, for a variety of purposes, several years ago, and have probably given it as largely and continuously as anyone has done. Ever since 1868 (on the suggestion of Mr. Woakes) I have been in the habit of giving ergot in neuralgia and other nervous complaints, and in order to test the range of its powers have often administered it in large doses for many weeks together. Even the continuance of forty-minim doses thrice daily for six weeks or two months has never produced any untoward symptom beyond a general nervous depression, not greater than that which often follows a long course of large doses of bromide of potassium. I need not say that any employment of ergot for hemoptysis would involve only much briefer periods than this: for unless it pretty quickly produced a decided impression we should have no reason to expect good from its continuance; and after hemorrhage has ceased it is probably only necessary to continue the ergot, in diminishing doses, for a few days longer.

I think we have now established the facts (*a*) of the direct action of ergot in the cases which I have recorded; (*b*) of its superiority in several of these cases to other styptics that had been tried; (*c*) the probability, from physiological analogies, that ergot would act more universally as a checker of hemoptysis than the routine remedies with which we are familiar; (*d*) also that it is perfectly safe for the purpose in view, and in this respect is superior to digitalis, which otherwise resembles it a good deal. Some further illustrations of its special activity should, however, be given; and I quote, in the first place, from my own cases. Case II. in series *C* (advanced phthisis with rapid tissue destruction) was one of the most ugly and threatening cases of hemoptysis that I ever saw; in addition to the formidable symptoms already mentioned, there was a history of an epistaxis, some twelve years before, which proved alarmingly uncontrollable, and very nearly killed the patient; the fact that such an immediate impression was produced by the ergot in this case has a great significance. Case I. of series *B* is also very convincing; the alcoholic type of phthisis, when it follows the course described, is a disease in which hemorrhage, if it occurs at all, is often very violent and intractable, and here it was plain that the direct action of the ergot upon the heart and upon the small arteries was both prompt and efficacious.

One of the best proofs of the reality of a supposed action of any drug is afforded when the effects, which were comparatively slight or dubious after the administration of the rougher preparations, became more marked and apparent in proportion as fewer and more concentrated preparations come to be employed in practice. Now this is precisely the history of ergot as a remedy for hemorrhage. Used at first in the form of infusion or tincture, it was only moderately efficacious; and, indeed, so late as 1870, Nothnagel expressly states that which I have been endeavouring to show can no longer be considered true, viz., that ergot is not superior to acetate of lead, or sesquichloride of iron, in hemoptysis. But the liquid extract, given by the stomach, has given even better and more certain results, not only in my hands, but in those of several others. And the hypodermic injection of Bonjean's ergotin (which I have used for other purposes, but not for hemoptysis) has given still more excellent promise, more especially in the hands of Drasche, of Jamieson, and of Dr. Currie Ritchie, of Manchester; the latter gentleman published an excellent paper on the subject in the Practitioner for December 1871.

Meantime there have sprung up other collateral illustrations of the effectiveness of ergot in producing contraction of the arterioles and moderating the heart's action, of which the most remarkable is Langenbeck's discovery of the therapeutic use of ergotine injections in aneurism. It is true that this latter subject is as yet not at all completely developed; but enough has been proved to strengthen materially the general mass of evidence in favour of the belief that ergot acts precisely in the manner in which we should desire a pulmonary hæmostatic to act. I of course do not say that we shall not possibly discover a yet more useful remedy for hemoptysis, but at present we probably possess none equal to it. Over and above the important resource which ergot seems to afford when hemorrhage is actually dangerous in amount, and when such remedies as gallic acid, acetate of lead, or muriate of iron have failed, I think that ergot is likely to be the most appropriate of all hemostatics for the hemorrhage of early phthisis, where it may be hoped that the lung has as yet undergone very little structural change. The prompt suppression of all oozing into the air-cells which, in view of the danger of phthisis *ab hæmoptoe*, it is so important to effect, can probably be in no way so effectually accomplished as by the employment of ergot.

Nor do I prefer the gastric administration, although in the cases recorded I employed it. For getting the best results I can scarcely doubt that the hypodermic injection of ergotine is a decidedly superior method. But I was personally anxious to see whether the remedy would prove practically effective when given in a form which is much less troublesome, and consequently more adapted to every-day practice, than the hypodermic injection would be. I hope I have shown that it is effective in a very encouraging degree, and that the profession will soon come to recognise the preferability of a direct recourse to its use in hemoptysis, instead of the routine employment of a series of stringent remedies which are considerably less uniform in their action.—*Practitioner, May* 1873, *p.* 276.

36.—ON THE INFLUENCE OF DISTENSION OF THE ABDOMEN ON THE FUNCTIONS OF THE HEART AND LUNGS.

By Dr. Francis Sibson, F.R.S., Consulting Physician to St. Mary's Hospital.

[In treating this subject, Dr. Sibson passes rapidly in review the medicines most frequently serviceable, such as the warm and cordial tinctures, camphor, turpentine, assafœtida, and creasote. Charcoal is of little or no use in these cases. His object is rather to bring before the reader the non-medicinal means that may be employed for the relief of the distension.]

This gas, as I have said, is secreted, not by the structure of the stomach, but by the food. If you cut off the supply of food, sooner or later, when that already in the stomach has passed away, relief will be obtained. But we cannot cut off the supply of food; therefore we must select such kinds of food as give the greatest amount of nourishment, and tend to set free the least amount of gas; for, although every kind of food will produce gas when the powers of digestion are weak, yet some kinds will do it much more freely than others. My rule is simply this: take no bread, but eat biscuits instead. Take no solid vegetables, but use lemon-juice freely. Squeeze some of it over all your food. Shun soup, cucumbers, and fruit. Prefer meat, fish, fowl or game, tender, but perfectly fresh, lightly cooked and juicy—not hard and dry; or take it thoroughly soft and fragrant, as sent up by the hand of the artist. Take no rich sauces, or indeed, any other sauce, than wholesome fresh lemon-juice or mushroom ketchup; no twice cooked, or salted, or seasoned, or dried meat. Let everything be fresh and savoury. Under this simple rule of substituting biscuit for bread and fresh lemon-juice for vegetables, relief will often be obtained from distressing symptoms indicating danger. Let the food be agreeable to the eye, fragrant to the nostril, and savoury to the palate; and, invited by those *senses, the influences* of the nervous system will bring those *juices into the mouth and stomach* that are required for the digestion of the food—juices that are attracted if the food is attractive, repelled if the food is repulsive.

The rules, however, that I have given are but a portion of the proper treatment. Each case stands upon its own grounds, and must be treated according to its own demands. In treating each case, the laws of nature must be strictly enforced. Departure from those laws has brought on the illness; and in a renewed obedience to those laws, we shall usually find the best road to restoration of health. The right treatment of disease is neither expectant nor heroic, but is a treatment founded on knowledge, a watchful interest in the patient, moderation, and good sense.

One patient had been quite well until the previous April, when, one night, after a late supper, he woke up fighting for breath and life. He raised a thin and frothy phlegm, which was stained the next day with blood. He had suffered from no such attack for a month; but he could not lie down; he was short of breath, and felt, as he said, inflated—blown out—sometimes almost ready to burst. His heart was very large, its action being powerful. There was a doubling of the first sound over the ventricles, and feeble first sound, and loud second sound over the aorta. Effusion was present in the right side of the chest. There was a mere trace of albumen in the urine, but I found in it one large waxy cast. The kidneys were the true seat of the disease. Under the influence of the restricted diet I have described, and the general treatment, he lost the distension of the stomach; he could lie down on one pillow; he had no return of the sense of suffocation; and the fluid in the right side of the chest disappeared. The condition of the heart and that of the kidneys were not materially altered.

Here is another case of a patient (represented in this little diagram) who complained of "trouble" over the region of the heart. His abdomen was swollen; the stomach displaced the lower boundary of the heart upwards, so that it was situated in the fourth space. Subsequently, under a restricted diet, the upper boundary of the stomach and the lower boundary of the heart fell almost to the extent of an inch. The heart having gained an increase of space, lost its "trouble;" and, though he was ill in other respects, the distension of the stomach disappeared.

Another diagram that I have here shows a marked difference in the position of the heart during the second, as compared with the first, observation made eleven days previously. This patient complained that he had pulsation through the body, and that the heart stopped for two or three beats. This state was worse after dinner, and was accompanied by fulness in the head. The irregular action of the heart disappeared with the descent of that organ that followed upon the diminution of the size of the stomach.

The last case to which I shall refer gave me at first some anxiety from its obscurity. This patient was a fine-looking man, with a large but not overgrown frame. He was of good colour and complexion. He complained of great shortness of breath, and he had a dropsical swelling of the right lower limb. The only unhealthy condition that I could discover in his internal organs was a distension of the stomach from gas. I will allow, that I felt as if there must be some other cause, but I could find none. He took a dry diet; and although, a week later, the stomach was less distended, yet his right leg was still swollen, and his breathing short. He persevered, and gradually lost the shortness of breath and the swelling in the right leg, and he regained his usual good health.

Distension of the colon is illustrated in these diagrams. You will observe that, in these instances, the transverse colon is no longer transverse. In these they all form a loop from above downwards, the tip of the loop being midway between the navel and the pubes. You will also see that the ascending colon increases greatly in length, starting from its head in the crest of the ilium. You may remark, on looking at these two diagrams, that inasmuch as the top of the ascending colon immediately abuts upon the lower surface of the liver, the elongation of the ascending colon directly elevates the liver. So, also, with regard to this double flexure of the transverse colon. In one instance, you *see the loop is single*, and in another the loop is double. You will see that *the descending colon* is not in any of these cases, except perhaps in one, very greatly distended; yet there is in all of them great distension. In this diagram, representing a back view, the descending colon is distended quite as much as the ascending and transverse are in any of the others. In another instance, you see that the liver on the right side, and the stomach and spleen on the left, are both lifted up, as it were, upon the top of the lengthened colon. You may remark in these cases that the liver rises very high; in one instance, quite into the lower end of the fourth space; and in another, as high as the third space. And you may observe how much higher the upper boundary of the right wing of the diaphragm, which is lifted up by the liver, is than the upper boundary of the left wing of the diaphragm, which is lifted up by the stomach. The result is that, in these cases, we have the heart pushed over to the left side, owing to the elevation of the liver, which tends to displace the heart to the left; and while the right lung is compressed from below upwards, the left lung is encroached upon by the heart, which is pushed aside by the liver.

You will at once see, from the study of these diagrams, how seriously distension of the colon must affect the organs of the chest, not only the lungs, but also the heart, by displacing that organ to the left; whereas, in the case of the stomach, its displacement tended rather to the right.

The next set of cases that I shall bring before you are those in which the stomach, the colon, and the small intestines are all distended. In this diagram, distension of the stomach is not extreme, but there is extreme distension of the ascending, the transverse, and, I believe, the descending colon, and extreme distension of the whole of the small intestines, owing to peritonitis. You will observe that this diagram shows, better than any other, the oval shape that the distended abdomen assumes. Contrast it with this one, which shows the shape of health, like a reversed sugar-loaf rounded at the top, while the other is a true type of complete abdominal distension, being a perfect oval. In this other case, where the abdomen is perfectly flaccid, you have neither the reversed sugar cone shape of health, nor the oval of distension, but a double concavity. The centre of the abdomen is narrower than either the top or the lower part. We see here, in another diagram, the effect of immense distension upon the walls of the chest. See how these cartilages are pushed aside to the left, and those to the right, so that there is an addition of nearly one-third to the width from side to side. Again, you may perceive that the cage of the chest is lifted up by this great oval mass below pressing upon it from below, and this will account for the remarkable fact that, although we have the heart and lungs so elevated by the diaphragm thus pushed up, yet the lower boundary of the heart is very little above the lower boundary of the sternum; the lower boundary of the lungs is very little above the position in which we often find it in a perfectly healthy body. If you look at the aorta you will see how completely that artery has been driven over to the right side. If you compare the breadth of the heart from side to side, with the length of the heart and the great vessels from below upwards, you will see that the whole organ is, in point of fact, compressed upwards. In health, as you may see in several of these instances, the apex of the heart is lower than the lower boundary of the heart behind the lower end of the sternum. In this instance, the apex of the heart has been lifted upwards and is a third of an inch higher than the lower boundary of the heart at the end of the sternum.

We find in these cases—that the heart and the lungs are put to an immense disadvantage—that the lungs are compressed and the reserved air driven out of them—that the heart is compressed and the blood driven out of it—that the ventricles are diminished in size and are incapable of receiving so much blood as they did before—that there is an obstacle to their action, and that they are incapable of sending out the blood that they have already received with the ease that they did before. Again, if you look at the walls of the chest and the diaphragm, you see what an obstacle is put to the very limbs, by which, as it were, the lungs expand and the heart is moved. The cartilages and ribs are pushed aside and lifted upwards. The cage of the chest has reached the limit of its powers by the elevation given to it, and permanently given to it by the distension of the abdomen. Again, look at the diaphragm, lifted up by this immense distension. How can it act downwards? If the diaphragm is affected with peritonitis, the inflammation paralyses the inflamed muscle. The whole contest is then narrowed to the smallest bounds; and the whole work of respiration must be done by the few ribs remaining unassailed at the top of the chest. In cases of peritonitis, respiration is what is very well called high. The whole of the breathing is performed by the laboured lifting up of the upper cartilages, the upper ribs and the clavicles. All the muscles that act—the sterno-cleido, the scaleni, the levators of the ribs and the scapulæ, are hard at work, and, in spite of that, you see visibly in the face that the patient is undergoing chronic asphyxia, and must soon die unless his breathing is relieved.

Here is another case: a repetition of the other, but a little less extreme. It shows in what I would call a rather beautiful and orderly manner, a series of tiers, one above another, the top one made by the distended stomach, the second by the circular curve of the transverse colon, and the third by the small intestines. Here is also another case, showing tier above tier in the same way, the intestines below, the transverse colon in the middle, the stomach above, and above that the heart. Here is also another, showing the intestines below, the transverse colon at the middle and the stomach at the top, all pushing up the heart and lungs and interfering with their work. I could show to you other cases; but these are sufficient.

The tendency of this great distension of the stomach and intestines is to push the heart directly upwards, to flatten the heart from below, and to displace it somewhat to the right side. You see in this diagram that the aorta, instead of being shielded by the sternum, lies almost entirely to the right of that bone. The pulmonary artery, on the contrary, instead of lying entirely behind the spaces, is almost entirely behind the sternum. A similar condition obtains in this other case. You see that the apex has been raised. It is nearly half an inch higher than the lower boundary of the centre of the heart, although, naturally, it ought to be half an inch lower than that lower boundary. The position of the apex is the test of the extent to which the heart has been compressed upwards.

There are two means by which the treatment of great distension of the stomach may be met, independently of drugs; and I would say to you that when distension is extreme, drugs are worse than useless. They add, by their offensiveness, to the misery of the patient. These two modes are the insertion of the œsophageal tube into the stomach through the mouth, and the insertion into the stomach of the fine point for injecting morphia under the skin. I have the particulars of a case that occurred in this hospital—that of a man who had the most serious threatening of death in consequence of great distension of the stomach. The circumference of his chest was thirty-nine inches. We introduced the œsophageal tube, and a large quantity of gas escaped. The circumference of the chest was reduced from thirty-nine inches to thirty-seven, and for a time the man experienced perfect relief. Before this was done, he had great distress; after it, he was at ease. The relief lasted for some days. He had kidney-disease and various other serious affections; and after a time he died.

Another case, that I saw with the late Dr. Williams of Swansea, was more fortunate. The patient had acute rheumatism of a low type. The stomach was extremely distended, and respiration was interfered with in consequence. Dr. Williams introduced the tube; a large quantity of gas escaped; the powers of respiration were restored, and gradually the patient recovered completely, lived for many years afterwards, and is, I believe, now alive. With regard to the other method—the insertion into the stomach of the fine point for injecting morphia under the skin—I have never had the opportunity of employing it; but I have been always ready to use it if required; and I believe, though I cannot put my hand on a case, that it has already been practised with advantage and success.—*British Medical Journal, Aug.* 3, 1873, *p.* 105.

37.—ON ARTIFICIAL RESPIRATION AFTER THE OPERATION OF TRACHEOTOMY.

By Dr. Benjamin W. Richardson, F.R.S.

At the conclusion of a lecture published in the Medical Times and Gazette on Dec. 4, 1869, in which was described the double-acting elastic hand-bellows I have invented for artificial respiration, I took occasion to point out that in various cases of acute disease—such as cases of bronchial spasm or of failure of respiratory power,—when death is at hand, the practice of artificial respiration, by the bellows, might prove of service. The following clinical history, briefly told, illustrates forcibly the truth of this proposition:—

A child, aged 4 years and 4 months, was under the care of my friend and neighbour Dr. Neale. She had shown for ten days some obscure symptoms, varying in intensity, of irritation of the throat, with intermittent pyrexia. On Sunday, the 20th of last month (July), on his morning visit, Dr. Neale found the child breathing heavily, and again feverish. On examining the throat, he discovered a diphtheritic patch on each tonsil; he detected also a croup-like respiration. A little later in the day I was called to meet Dr. Neale in consultation. We agreed as to diagnosis and treatment, and all progressed well until Tuesday morning, the 22nd, at three o'clock. At that time the symptoms changed, food and medicine were refused, and in a few hours the respiration was embarrassed from obstruction in the larnyx. Severe paroxysms of spasmodic breathing also occurred at frequent intervals. An emetic gave great temporary relief; but the symptoms afterwards increased, and at 2 p.m. were so urgent that the question of the performance of tracheotomy came under consideration. At half-past six, at a consultation between Dr. Neale, Mr. Christopher Heath, and myself, the operation was determined upon as a necessity. I administered methylene bichloride, which acted promptly and effectively, and Mr. Heath operated.

Recovery from the anæsthesia was rapid, and the little patient soon began to breathe freely through the tracheal tube. Dr. Neale having to leave, I undertook to remain until his return. About an hour after the operation the child was suddenly seized with an attack of severe spasmodic breathing, followed by the escape of tenacious frothy mucus through the tracheal tube. I cleaned the tube freely, and drew up the mucus through the tube, but the paroxysm continued; the features became livid; there was a sharp convulsion; the natural respiration ceased; the pulse ceased; and, to all apppearance, there was death. At this moment, affixing an inner tracheal tube to the double-acting bellows, I thrust the inner into the outer tracheal tube, and commenced steadily to keep up artificial respiration. I confess I had no great expectation of useful results; but to my relief, after sustaining the artificial respiration for a period of three minutes, the patient recommenced to breathe independently, and by the time Dr. Neale, who had been sent for, returned—about ten minutes,—she was quite conscious, warm, and breathing freely. We watched alternately through the night, and the night was passed comfortably. Food was freely taken, sleep was natural, the pulse was firm and regular, never exceeding 120, while the respiration, though at times quick (36 per minute), was not laboured. At 8 a.m. on Wednesday, the 23rd, Mr. Lovell and Mr. J. E. Neale being now in attendance, a paroxysm of very severe dyspnœa and spasm recurred, but was quickly relieved by the use of the bellows, and on Dr. Neale's arrival, later in the day, the recovery was so complete that the child was playing at "catch ball" with a sponge. She progressed favourably until 8 p.m. of this day, the 23rd, when from a sleep of about twenty minutes she awoke with another violent paroxysm of the dyspnœa. *In extremis*, Dr. Neale again set up the artificial respiration, and in two minutes the natural respiration was restored. After this there was natural action of the kidneys, and milk was freely taken. At 9 p.m. another extreme attack of the dyspnœa supervened, relieved in the same manner after the use of artificial respiration for five minutes.

At 9.30 I *rejoined Dr. Neale*, and we found now, for the first time, moist *crepitation from exudation or* condensation of fluid in the upper surfaces of both lungs. Relief in removal of some of this fluid was afforded by a device of Dr. Neale's of making a Higgins syringe act as an aspirator, and there followed a period of gentle and hopeful breathing. But at 10.30, on the occurrence of another of the extreme paroxysms of dyspnœa, the respiration ceased altogether, the pulse ceased, the countenance assumed the lividity of death, and death, indeed, once more appeared complete. The tracheal tube was removed, the tube of the bellows was inserted into the tracheal opening, artificial respiration was established, and after an interval, as far as we could measure, of five to six minutes, the natural respiration was restored.

From this time, unhappily, the pulmonary lesion increased; the obstruction to the breathing passed from a local to a general condition; the bronchial passages became closed from accumulation of fluid; and absolute death, without return of asphyxia and without a struggle, occurred at 1.30 of the 24th.

To have to record the ultimate fact of death in this case, after such restoration from apparent death, is indeed a painful duty. Yet the one particular process for restoration—artificial respiration—is thereby, in no degree, lessened of its value. It gave a renewal of thirty hours of life, during which if we had known more respecting the removal of those fatal conditions which so commonly commence in the extreme or peripheral parts of the pulmonary organs, we might have made the temporary safety complete and permanent. Step by step and with that agony of experience which ever attends the highest efforts of the human mind, we shall attain one day even to this perfection. We shall acquire a still more perfect method of maintaining artificial respiration; we shall learn to introduce, with good effect, other gases and vapours with the common air; we shall regulate pressure on the pulmonary surface; we shall sustain, by some simple means, the muscular force; and we shall supply to muscle the stimulus it has lost from the exhausted nerve. In a word, we shall hold mastery of conditions in the presence of which the sick person cannot die. This, the *summum bonum* of medical science and art, may be afar off; but he is no physician who does not live for it, and the history I have written above demonstrates that one of the most important steps towards it is laid for our advancement.—*Medical Times and Gazette, August* 2, 1873, *p.* 117.

DISEASES OF THE ORGANS OF DIGESTION.

38.—ASCITES IN INDIAN PRACTICE.—USE OF THE DRAINAGE-TUBE.

By Dr. JAMES DONALDSON, late of H.M. Madras Medical Staff.

[The habit of body of the native Hindoo is quite different from that of the inflammatory European. The former easily succumbs to sudden accessions of violent diseased action, but is all enduring as to chronic forms of illness, and tolerates an amount of handling of vital organs and interference with functional processes which would scarcely be believed by any one unacquainted with the peculiarly phlegmatic temperament of the natives of India. The following case is related by Dr. Donaldson in illustration.]

Disease, Ascites.—Ponamah, female, age 35. Caste or country—Hindoo, native of Madras. Admitted into the Government hospital at Vepery, second district of the presidency town of Madras, on 30th April, 1864, in a distressing state of helplessness, breathlessness, and weakness, enormously swollen, abdomen distended apparently to fullest limit, skin tense. As it is not intended, and would serve no useful purpose in these succinct histories, to give *minute* details of symptoms and treatment here, it may be merely noted that the object primarily looked to in this instance was, bringing the patient into the best state of preparation for the operation of paracentesis abdominis, with the intention of performing it at the earliest date. Mild aperient medicine, warm baths, and saline diuretics were the remedies indicated. There was no history detailed to account for this abnormal collection, no apparent disease of liver, heart, kidneys, or other organ; the urine not noted as being much different from the ordinary standard unless small in quantity, which was to be expected, and which would naturally be more prominent from the high temperature prevailing at that time, viz., 95° to 100° Fahr., in the large, well-appointed hospital, daily. On May 6, there appearing to be no reason why the operation should be delayed, and the patient being exceedingly anxious for its performance, "tapping" was carried out in the usual way, the patient lying well over on her side, and twenty-two (22) pints of fluid drawn off. (Patient would weigh, in health, about 90 to 95 lbs., but this could not be very exactly ascertained.) The relief afforded by the removal from the abdominal cavity of about 400 ounces of fluid was very grateful, and a bandage being applied with comfortable pressure, the patient expressed herself as feeling "restored to health." On careful examination, now that the different organs were more within the reach of proper observation, no chronic disease of liver, heart, or kidneys was discovered. The heart's action was feeble, nothing more. The patient, with good nourishment, and very moderate and temporary use of stimulants, soon felt strong; but the process of re-filling commenced but too soon, and with sufficient quickness. As diuretics, the acetate, nitrate, and bicarbonate of potass were given, with iodide of potassium; also carbonate of ammonia and squill; the old "compound iodine ointment" was rubbed assiduously into the abdomen, and careful bandaging kept up; but in the end of May the distress and dyspnœa were again such as to cause the poor woman to beg for relief. On 2nd of June tapping was again had recourse to, nearly eleven pints of fluid on this occasion being removed. Now, as this probable often repeated paracentesis seemed to promise only very temporary relief, with the almost certain prospect of rapid prostration of patient's strength, the idea was entertained of not only withdrawing as much fluid as possible, but farther, of allowing it to drain away as it formed, with the hope that by aid of steady, carefully-applied pressure kept up, even if a moderate amount of inflammation should be excited in the cavity, a radical cure might be expected. This view of the case, and this proposition being placed before the patient, she readily assented, preferring any risk (as natives of India usually do) to the prospect of repeated accumulations and operations. Accordingly, after as much of the fluid as possible had been drained off, the bandage being carefully adjusted, the cannula was left in, and tied in such a way as to act like a drainage-tube—a small cork having been first fitted to it, so that, instead of the drain being constant, the cork could be taken out at short intervals, and any little collection of fluid allowed to run away. On the 3rd June this is the report,—"Little or no fluid has passed by the cannula since the operation; there is a little pain just where the cannula is inserted; no general pain, no threatening of peritonitis, no fever, no bad symptom, but the patient, as on the former occasion after operation, is weak, and the pulse is feeble. Bowels and kidneys acting well." On the 6th, little fluid appeared to have been secreted, and the report is,—"Abdomen getting smaller, instead of immediately commencing to increase in bulk as it did last time, and the flannel binder was tightened yesterday; no pain or uneasiness of any kind; patient feels well and is sitting up." On the 16th, the cannula had got dislodged. It was not thought advisable to try to replace it, and report is,—"Water threatening to collect again, but not much; the compound iodine ointment to be rubbed in, and the diuretic medicines given as before." 21st June,—"Feels easy and well, little change of any kind; to continue idem." On the 15th July, patient was well in all respects (except that the fluid in abdomen had increased somewhat) and afraid that it would augment farther, and bring on the old, uncomfortable symptoms, she wished the cannula replaced, and the drain re-established; but this was not considered advisable or necessary. After this date there was occurrence of fresh symptoms—viz., congestion in several organs (liver, lungs, and kidneys), with pain in the right hypochondrium, some oppression of breathing, and albuminous urine; there was no retarded circulation from re-accumulation of water to account for this, and probably the patient had got a chill from exposure to the strong hot land wind, or the stronger chilling sea breeze which daily succeeds it, after the free perspiration caused by the excessive heat. Besides warm baths, and re-induction of the skin's action, all that was thought necessary was the application of a large blister over the abdomen, just below, and over the lower portion of the liver. These means were effectual. On the 21st July, "patient was easier, breathing comfortable, lungs relieved, pain gone, urine copious, and free or nearly so of albumen;" the medicines ordered to be continued, the blister dressed with mild ointment. On 29th, "collection of fluid apparently stationary, not increasing." From this date the patient's condition, from being "stationary," began to improve; the fluid in abdomen, instead of increasing, decreased; and the report is,—"From this date the poor woman slowly but steadily improved in health, the various organs reassumed their natural state, and satisfactorily performed their functions. The remaining fluid in the abdomen was absorbed, the belly regained its proper shape, and she was discharged from hospital, after being kept sufficiently long under observation, apparently 'quite well' in the beginning of January following."

There were, I think, several points of interest in this patient's illness—*e.g.*, the large accumulation of fluid in this small woman without apparent organic disease; without apparent noteworthy obstruction to the return of blood by the portal vessels to the heart; without any previous history of active inflammation of the peritoneum, or obvious chronic inflammation of the same, &c. Sir Thomas Watson records a case almost identically the same in its more obvious aspects, referring to it as "a condition he believes he had met with." Again, the rapid re-filling of the sac without any manifest exciting cause after the first operation of withdrawal, under nearly the same existing conditions; the circulation being so much more free by the abstraction of the heavy mass of fluid before weighing on the abdominal organs, and oppressing, almost paralyzing, the action of the diaphragm. But the main point was the establishing a drain from the peritoneal sac with the strong risk of almost inviting, as it were inflammation within the cavity. I cannot call to remembrance seeing such procedure anywhere recommended, and I would not have proposed such in an European, nor ventured upon it in private practice, but "the end justified the means," I hope. I had only one other very similar case in India, the details of which need not be given, except just stating the leading facts. The patient was also a female, native of the Madras Presidency, admitted into the Garrison Hospital at Vizianagram, in 1860:—had been "tapped" (by my predecessor in office), the sac had gradually refilled; there was no discovered organic disease, and no "history." I operated once, and drew off a large quantity of fluid, then (on the second occasion) left the cannula tied in, and established a drain. There was no bad result; on the contrary, the process of refilling was checked, the drain was free, and I was indulging hopes of a complete cure when the woman, tired of hospital, or thinking she was well enough, went away without "gratia," seu "vale," as is very common with the natives of India when the spirit moves them, and I saw her no more; could not even hear what had become of her during my short remaining term of duty in the station.—*Glasgow Medical Journal, May* 1873, *p.* 306.

DISEASES OF THE URINARY ORGANS.

39.—ATROPHY AND SUPPURATIVE INFLAMMATION OF THE KIDNEY FROM RETENTION OF URINE.

By Dr. George Johnson, F.R.S., Physician to King's College Hospital, London.

The effect upon the kidney of retention of urine varies according to the nature and seat of the impediment. It differs, too, according as the obstruction occurs gradually or suddenly. One of the most frequent causes of renal disease consequent upon retention of urine is stricture of the urethra. The urinary organs behind the stricture undergo changes of structure in proportion to the degree and the duration of the obstruction. The canal of the urethra on the vesical side of the stricture becomes dilated; its mucous membrane is frequently inflamed, and secretes pus. The muscular coats of the bladder become thickened by hypertrophy, and its mucous membrane often inflamed and sacculated. The obstruction then affects the ureters, one or both of which may have their canals dilated and their walls thickened; and at length the natural cavities of the kidney—the pelvis, infundibula, and calyces—undergo the same process of dilatation. The medullary cones become flattened out by the pressure of the retained urine. The cortical substance of the gland is expanded, and presents lobed bulgings on its surface, which correspond with the original lobes of the embryo kidney. The glandular tissue is squeezed between the distended interior cavity and the fibrous investing capsule; and the intertubular capillaries are compressed by the dilated tubes. Thus the circulation is impeded, and the result is atrophy of the gland, which may by degrees be converted into a membranous cyst, all traces of glandular structure being lost.

It is but seldom that the kidney undergoes much dilatation without the occurrence of other structural changes. The mucous membrane of the dilated pelvis often presents irregular inflamed patches, and secretes a purulent liquid; and the apices of the medullary cones are frequently ulcerated. Then, as the mischief extends, inflammatory deposits occur in the substance of the kidney, and numerous small abscesses are scattered through the cortex. One or more of the abscesses on the surface may burst through the capsule, and then the kidney may be found imbedded in pus.

When the retention of urine is the result of stricture, or of enlarged prostate, or of calculus with thickening of the walls of the bladder, or of atony of the muscular coats, both kidneys are usually affected simultaneously, but in different degrees; but when one ureter is obstructed by a calculus, or by a cancerous growth, the structural changes are limited to the corresponding kidney.

The explanation of these changes is not difficult. The secreted urine is unable to escape in consequence of the obstruction in front; there is, therefore, an accumulation, first in the ureter and pelvis of the kidney, and later within the uriniferous tubes. The tubes become distended by the retention of their own secretion, just as some tubes in the small red granular kidney, having lost their lining of gland-cells, but continuing to secrete a serous liquid, become distended and dilated into cysts. The epithelial lining of the straight tubes is disintegrated and destroyed by the pressure of the retained urine; and at length some of the tubes in the cortex, whose basement-membrane is more delicate than that of the cones, give way, and allow their contents to become infiltrated amongst the intertubular capillaries. The infiltration of acid urine may cause the immediate formation of coagula within the capillaries, and, as a consequence of localised capillary and venous obstruction, irregular atrophic puckerings of the gland occur, somewhat similar to those which result from capillary embolism, to which I have before referred. But the escape of urine through the broken walls of the tubes may excite suppurative inflammation and abscess. A rapid cell-formation takes place between the tubes; and soon the glandular structure is disintegrated, and replaced by inflammatory products. The changes within and between the uriniferous tubes are a miniature representation of what happens on a larger scale when a distended urethra gives way behind a stricture, and a perineal abscess results from the infiltration of urine into the submucous tissues. Bear in mind that the changes within the substance of the kidney are due to the retention and accumulation of the newly secreted acid urine within the tubes, and not to the regurgitation of fœtid ammoniacal urine, as has recently been suggested. Without doubt, the urine in the bladder in cases of old stricture, vesical calculus, and cystitis, is often fœtid from decomposition; but the regurgitation of such urine into the uriniferous tubes is a physical impossibility. If you have ever attempted to inject the tubes from the pelvis of the kidney, you will have found the task a very difficult one, in consequence of the resistance offered by the liquid and solid contents of the closed tubes. Obviously, then, during life, while streams of secreted urine are perpetually flowing through the tubes, it is impossible that urine from without can *regurgitate into them.* The tubes are dilated, and some of them *ultimately ruptured, by the* retention and accumulation of their contents, and not by the regurgitation of urine from the pelvis of the kidney. Then intertubular coagula and suppuration result, from the infiltration of urine escaping from the torn tubes amongst the intertubular capillaries and veins.

The *Symptoms* of renal disease consequent upon an impeded escape of urine are usually more or less marked by the diseased condition of other parts of the urinary organs. The mucous membrane of the bladder in cases of stricture, vesical calculus, or enlarged prostate, usually secretes pus; and there are no means by which this can be distinguished from matter derived from a suppurating kidney. For the suppurative process in the kidney rapidly destroys the tubular structure of the organ; the pus, therefore, is not moulded within the tubes, and there is no microscopic evidence of the renal origin of the pus. Chemistry, again, affords no more assistance than the microscope. The urine, which contains pus, is always albuminous. The coagulability of the urine by heat and acid is, therefore, no indication that the kidneys are implicated, except when the degree of coagulability is out of proportion to the amount of pus, and it may be blood, mingled with the urine. Chemical analysis affords little practical aid in estimating the efficiency of the kidney. The urine is usually fœtid and alkaline, and much of the urea is decomposed into carbonate of ammonia while the urine is still in the bladder. A low specific gravity of the urine with a scanty secretion would be a suspicious condition, and especially so when associated with indications of uræmia, such as drowsiness, headache, vomiting, and a brown dry tongue, with an excess of urea in the blood. Pain and tenderness in the region of one or both kidneys may be severe when, with sudden retention of urine, there is great distension of the cavity of the kidney; but, in cases of long continued and slowly increasing obstruction, these symptoms bear no proportion to the amount of structural change in the kidneys; and it sometimes happens that the first indication of serious renal disease is afforded by the occurrence of alarming symptoms of uræmic poisoning, quickly passing on to fatal typhoid collapse and coma, with a low temperature. The cases are few in which the kidneys are sufficiently enlarged by distension and expansion to form a palpable tumour in the lumbar region.—*British Medical Journal, May* 31, 1873, *p.* 605.

40.—ON ACUTE CYSTITIS SIMULATING ACUTE BRIGHT'S DISEASE.

By Dr. George Johnson, F.R.S., Physician to King's College Hospital.

It has happened to me to meet with a considerable number of cases of acute inflammation of the mucous membrane of the bladder unconnected with stone, stricture, or gonorrhœa, which in consequence of the urine being blood-tinged and albuminous, have been mistaken for cases of acute Bright's disease. Therefore, before I proceed to describe the treatment of Bright's disease, I think it well to point out to you the distinctive features of acute cystitis. Remember that I exclude from our present consideration such obvious and common cases as cystitis from stone, stricture, retention of urine, and gonorrhœa; and I refer to cystitis not excited by any obvious mechanical cause. In a large proportion of cases—in six out of twelve of which I have notes—the disease directly followed, and was probably caused by, a chill. In four cases, dyspepsia with rheumatic or gouty symptoms had preceded the cystitis. In one, the disease came on after feasting with excess of wine. In one case—that of a physician—the symptoms commenced within a few hours after he had been impressed by a peculiar odour from the throat of a boy whom he was attending with a low form of scarlet fever. It is probable that in all these patients the immediate cause of the cystitis was some irritating material in the urine, as acute inflammation of the urethra is excited by the contact of the gonorrhœal poison. In the cases last mentioned, some poisonous product may have entered the circulation, and passed out through the kidneys without exciting disease in them, but setting up inflammation in the bladder. It is difficult to explain or to understand how it happens that exposure or cold should in one person excite acute cystitis, and in another acute desquamative nephritis. Of the twelve cases of acute cystitis which I have noted and tabulated, eight were males, and four females. The ages ranged from seventeen in a female to sixty-nine in a male. In some cases there has been more or less of vesical irritation for a few days before the acute attack, but in most instances the onset has been sudden and severe. The chief symptoms are frequent micturition, with more or less of uneasiness or pain in the region of the bladder. The calls to pass urine may occur every half-hour, and even oftener; and micturition is usually attended with an increase of pain and a sense of scalding in the neck of the bladder. The vesical irritation is increased by exercise, by exposure to cold, and by alcoholic liquors. The urine quickly becomes turbid with puriform mucus, and often blood-tinged. In one case there was a puriform discharge from the urethra as well as from the bladder, though the disease was certainly not the result of gonorrhœa. Usually the urine has an acid reaction; but, if there be much admixture of blood, the acidity is lessened by the alkali of the blood. It contains an abundance of albumen, partly derived from the puriform secretion, partly from the blood. On a microscopic examination, pus-cells and blood corpuscles are seen in abundance, but no tube-casts. Although the local symptoms are distressing, there is little or no constitutional disturbance. The nights are disturbed by frequent calls to micturate, and the broken rest is attended with a sense of fatigue and nervous exhaustion; but there is little fever, and no vomiting. If, within a few days from the onset of the symptoms, the patient be subjected to appropriate treatment, the disease usually subsides as rapidly as it came on. If, on the other hand, the symptoms be negligently or erroneously treated, the disease may become chronic, and cause prolonged and severe suffering. The urine becomes alkaline, ammoniacal, and fœtid; there are perpetual pain and annoyance; and ultimately the disease may extend backwards through the ureters to the kidneys, and so set up a fatal pyelo-nephritis.

Diagnosis.—The distinction between acute cystitis and acute Bright's disease is sufficiently obvious if you bear in mind that the local symptoms are all referable to the bladder, while dropsy, vomiting, and other renal symptoms, are absent. The urine is usually secreted in normal quantity, and of normal specific gravity. It is albuminous only in direct proportion to the amount of blood and pus which it contains, and the most careful microscopic examination discovers no tube-casts.

Treatment of Acute Cystitis.—In the treatment of this form of cystitis, you have to bear in mind that the inflammation of the lining membrane of the bladder is kept up and increased by contact of the acid and irritating urine. The main object of treatment is to diminish as much as possible the irritating qualities of the urine; with this object in view, the patient must be confined to bed or to a sofa in a warm room, and be kept on liquid food without stimulants. If there be no special reason in the peculiarity of the patient's stomach to forbid it, milk, cold or tepid, may serve as meat and drink—as a rule, milk is easily digested, and, at the same time, it acts as a diluent. If milk disagree, soup, beef-tea, mutton or chicken-broth may be given, with the addition of some farinaceous pudding, and any simple diluent drink. Pure cold water is as efficacious, though not so pleasant, as aërated or Seltzer water. In addition, the urine is to be kept neutralised by citrate of potash, given every six hours. A warm hip-bath should be given night and morning; a dose of morphia at bedtime, to allay irritation and procure sleep; and an occasional seidlitz powder as a laxative if necessary. Under this plan of treatment, the acute symptoms usually subside with great rapidity; the pain and irritation pass away, and the urine regains its normal characters. After a few days, the citrate of potash may be discontinued, and the tincture of perchloride of iron given in twenty-minim doses two or three times a day after food. So long as any mucus appears in the urine, even though the local uneasiness may have passed away, the patient should be kept under observation and treatment. If a chronic catarrhal condition of bladder remain after the acute symptoms have subsided, copaiba balsam often effects a rapid and complete cure. One capsule may be given an hour or two after food three times a day; and, if the stomach will bear it, the dose may be increased until six and even nine capsules are taken in three doses in the twenty-four hours. In one case which had been of a year's duration, the urine being turbid with blood and pus, and smaller doses of copaiba having failed to cure, at the suggestion of Sir William Fergusson, who saw the patient with me, the dose was increased to three capsules three times a day, and the result was a complete and permanent cure within six weeks; the urine at the end of the treatment being of natural sherry colour, transparent, and without a trace of mucus. Quite recently I have seen a young lady in whom acute cystitis from cold had left a vesical catarrh after a period of six weeks. I prescribed one capsule three times a day, and, in less than a week, the urine was entirely free from mucus, and the cure was complete. I have seen equally good results in other cases. It is probable that the remedy, being excreted by the kidneys, has a local curative action on the mucous membrane of the bladder. The copaiba sometimes brings out a transient erythematous rash.—*British Medical Journal, May* 31, 1873, *p.* 605.

41.—ON THE PATHOLOGY, DIAGNOSIS, AND TREATMENT OF BRIGHT'S DISEASE.

By Dr. George Johnson, F.R.S., Physician to King's College Hospital.

[This paper treats more especially on the treatment of acute and chronic Bright's disease. The origin of the disease appears to be that the kidney becomes damaged by the discharge of its duty, possibly owing to having to excrete abnormal products.]

It follows that a leading principle of treatment is to lessen as much as possible the excretory work of the kidney by instructing the patient to avoid the exciting causes of his malady, by a carefully regulated diet, and by such remedial agencies as experience has proved to be beneficial.

In all cases of *acute Bright's disease*, whatever may have been the exciting cause, rest in bed and in a room of moderate uniform temperature, well ventilated, but without chilling drafts of cold air, is an essential part of the treatment. In a large proportion of cases, rest in bed, with a scanty diet and a liberal use of diluent drinks, will suffice for the cure. A convincing proof and illustration of the effect of exercise, food, and cold, upon the amount of albumen in the urine, is afforded by the fact that, in most cases of albuminuria, the urine passed after rest in bed and before breakfast contains much less albumen than that which is secreted after exercise in the open air and after an ordinary meal.

The diet may consist of milk alone, if milk do not disagree, as it does with some patients. Milk is especially suitable for children; and it serves both for meat and drink, so that no other food or liquid need be taken. It may be taken cold or tepid, from half a pint to a pint at a time. An adult will take sometimes as much as a gallon in the twenty-four hours. Children will take less, in proportion to their ages. If the cream disagree, causing heartburn, diarrhœa, headache, or other symptoms of dyspepsia, the milk may be given skimmed. One reason, amongst others, for giving the milk as a rule unskimmed—that is, with the cream—is, that constipation, which is one of the most troublesome results of an exclusively milk diet, is to some extent obviated by the cream in the unskimmed milk. The advantage of milk as a main article of diet is that, as a rule, it is easy of digestion; and that, taken freely, it supplies an abundance of liquid, which, by its diluent action, has a diuretic influence, and so favours the removal of the dropsy. There are some patients with whom, unfortunately, milk in any form, even in small quantities, so decidedly disagrees, that we have to find a substitute in beef-tea, chicken, veal, or mutton-broth, with an egg or two, and some farinaceous addition, such as barley-water, arrowroot, rice, or sago, or a small quantity of bread. Under this regimen, adopted and rigidly carried out at the very commencement of acute Bright's disease, the urine soon becomes copious, while the albumen diminishes and gradually disappears, and the dropsy quickly passes away.

In my second lecture, I gave you the physiological explanation of the copious *flow of urine which* usually occurs during convalescence from acute Bright's disease, *and especially* when there has been a copious dropsical effusion. This abundant flow of urine usually occurs without aid from diuretic drugs, or, indeed, from drugs of any kind. Stimulating diuretics, such as squills, cantharides, or turpentine, would be injurious by increasing congestion of the kidney. The best diuretics in cases of acute Bright's disease are those means which tend to lessen congestion of the kidney; such as dry cupping or hot poultices or fomentations over the loins, warm baths, and a free use of diluent drinks, one of the pleasantest and most efficacious being the "imperial drink," made with cream of tartar and lemon.

Warm baths are particularly useful in the early stage of an acute attack, and more especially when exposure to cold has been the exciting cause of the renal disease. A warm water bath, at a temperature of 98 or 100 deg., may be given every night during the first few days of an acute attack; or a hot-air lamp-bath; or, what I believe in most cases to be still more efficacious, a wet sheet and blanket bath. A sheet is wrung out of warm water; and the patient, either naked or covered only by his shirt, is enveloped in the wet sheet up to the neck. Then three or four dry blankets are closely folded over the wet sheet. He may remain thus packed from two to four or six hours, or even longer. Recently, a boy in the hospital with acute renal disease and almost complete suppression of urine, consequent on scarlet fever, was kept packed incessantly for four days without serious discomfort, and with great relief from very distressing and alarming symptoms. When he left the hospital, all traces of his malady had disappeared. If the packing be long continued, the sheet has to be re-wetted as soon as it becomes dry. The evaporation and consequent drying of the sheet will be slow in proportion to the closeness of the blanket packing. If the outer blanket be covered by a mackintosh cloth, the sheet remains wet for a much longer time than when no waterproof covering is used; but patients often complain of a feeling of oppression when surrounded by the impervious mackintosh. The advantage of the blanket-bath over a warm water or a hot air bath, is, that it requires no special apparatus, that the diaphoretic action may be more prolonged, and that in most cases it is more agreeable to the patient. The hot air bath not unfrequently causes an unpleasant throbbing in the head, or a feeling of exhaustion and even faintness. When the wet pack is removed, the patient should be quickly rubbed dry, and enveloped in dry blankets.

The diaphoretic action of any form of warm bath is assisted by copious libations of simple diluent drinks, and it may also be aided by the internal administration of the solution of acetate of ammonia. It has been objected on theoretical grounds, that to promote perspiration in these cases is injurious by diverting to the skin the water which is required to wash out the uriniferous tubes. To meet this objection, you have only to bear in mind that the dropsical patient is oppressed by an excess of water, which has been poured into the areolar tissue and the serous cavities, in consequence of defective urinary secretion resulting from the inflammatory engorgement and obstruction of the kidneys. If, therefore, by the relaxing effect of external warmth, you divert a large amount of blood to the surface, you thereby lessen the congestion of the kidney, increase the freedom of the renal circulation, and so favour the occurrence of that copious secretion of urine which is one of the surest signs of satisfactory progress, and by which the uriniferous tubes may be effectually flushed and cleansed. Whatever fluid is lost by perspiration, may be quickly restored by the liberal use of diluent drinks, which again assist the secretory activity of both the skin and the kidneys. In fact, one of the main objects of treatment is to increase the freedom of the circulation, more especially through the kidneys; and thus to get rid of the excess of stagnant water which has accumulated in consequence of defective action of the skin and kidneys.

Purgatives may be usefully combined with other means for lessening dropsical effusion. In ordinary cases of acute Bright's disease, I do not, as a rule, advise the frequent employment of drastic purgatives. I reserve this method of treatment for cases in which there is an excessive and increasing dropsical effusion which does not yield to other means of cure, but more especially for cases in which cerebral symptoms, the result of uræmia, are either present or apparently impending. Dr. Abercrombie and others, who wrote on brain-disease before Dr. Bright's *discovery* had led on to our present knowledge of *uræmic nervous symptoms*, published cases of cerebral disease which they took to be of an inflammatory or an apoplectic character, in which the most strikingly beneficial results were obtained by free purging. Dr. Abercrombie, in discussing the treatment of inflammatory affections of the brain, states that, according to his own experience, "more recoveries from head-affections of the most alarming aspect take place under the use of very strong purgatives, than under any other mode of treatment." Our more recent experience is entirely confirmatory of Dr. Abercrombie's statement; but our improved pathology enables us to add a very important qualification—namely, that most of the cases in which formidable cerebral symptoms have been removed by the action of strong purgatives, have been neither inflammatory nor apoplectic in their nature, but cases in which brain-symptoms have resulted from blood-poisoning; and in the majority of instances the poisoning has been uræmic. Amongst the cases recorded by Dr. Abercrombie as examples of inflammation of the brain successfully treated, there are two in which the brain-symptoms were associated with anasarca, which had followed an attack of scarlet fever. These were unquestionably cases of acute renal disease, with cerebral symptoms of uræmic origin. I offer for your practical guidance this rule of treatment: when such symptoms as headache, delirium, convulsions, or coma, are the result of uræmia, give purgatives freely; and, if the renal disease be acute, and therefore probably curable, your treatment will often be completely successful. On the other hand, when you have reason to believe that the like brain-symptoms are consequent on cerebral hemorrhage, or embolism, or thrombosis, be very cautious in the use of purgatives, which may greatly increase the patient's distress and exhaustion, while they can do little to improve his condition. In inflammatory affections of the brain and its membranes, purgatives are often useful, but less frequently and strikingly so than when cerebral symptoms are the result of uræmia. As to the form of purgative in uræmic cases, croton oil is the most convenient when there is coma and consequent difficulty in swallowing a more bulky dose. When there is no such difficulty, two pills, composed of three grains of calomel with seven grains of compound colocynth pill, may be followed in four hours by an ounce of the compound senna mixture; or the following powder, which I think an improvement on the compound jalap powder, may be given: ℞. Scammoniæ resinæ gr. v to viii; potassæ tartratis acidæ ℈i; pulveris zingiberis g. iii. Misce. The dose to be repeated once or oftener, according to circumstances.

When, in a case of acute Bright's disease, the renal congestion is excessive, as shown by the scanty secretion of highly albuminous urine, with vomiting, headache, and other threatening nervous symptoms, local bleeding by leeches or cupping on the loins is often extremely useful, and is quickly followed by an increased secretion of urine. If, by the abstraction of a few ounces of blood from the loins, we relieve the renal congestion, we thereby lessen the destruction of blood-constituents which results from contamination of the blood by urinary excreta. Moderate and timely local bleeding, therefore, tends to economise blood, and to prevent its waste.

The more I have seen of the disease, the more hopeful I have become as to the ultimate result, when the history and the symptoms, and, above all, the chemical and microscopical characters of the urine, do not indicate extensive and irremediable degeneration of the kidney. In all the cases of recovery from long continued albuminuria, the preparations of iron have entered largely into the medicinal treatment of the disease, and have apparently contributed much to the favourable result. There are two preparations which I have found especially useful; these are the tincture of the perchloride and the syrup of the phosphate—the former in doses of from fifteen minims to half a drachm, and the latter in drachm doses twice or thrice daily. The preparations of iron are best taken soon after food. I have frequently combined with each dose of the perchloride of iron ten grains of hydrochlorate of ammonia; and I believe that this ammonio-chloride of iron is an useful combination.—*British Medical Journal, June* 21, 1873, *p.* 696.

SURGERY.

FRACTURES, DISLOCATIONS, AMPUTATIONS, AND DISEASES OF BONES, JOINTS, ETC.

42.—ON NERVOUS MIMICRY (NEUROMIMESIS) OF DISEASES OF JOINTS.

By Sir James Paget, Bart., F.R.S., Consulting Surgeon to St. Bartholomew's Hospital.

[Sir James Paget has been giving us some most interesting lectures on the nervous mimicry of organic diseases, showing that often the pains in the nerves of a part will give rise to many of the symptoms of real disease when in fact there is very little the matter with the case. In this lecture he gives us some capital hints on joints.]

Among all the joints, the hip and the knee, which are the most frequent seats of real disease, are equally so of the mimicry: a fact not easy to account for. It may be due to mental association, perhaps unconsciously; or to a mingled inheritance—for instance, to an inheritance of nervous constitution and of relative weakness in the joint or joints most weak in progenitors. After the hip and knee follow, in order of frequency, the tarsal joints and carpal, or the elbow and shoulder; but, in these, mimicries are too rare for counting.

Another fact, adding to the difficulty of diagnosis, is that the most frequent exciting causes are the same for the real and for the imitated affections of the joints. Injury, or some great fatigue, is commonly referred to as the source of the mischief. The injury may seem inadequate for such trouble as followed; but you cannot rely on this. Many of the worst instances of scrofulous arthritis follow injuries that seemed very trivial. It may suggest a suspicion of neuromimesis if pain set in with full severity directly after an injury that was not severe; but the suspicion must be lightly held. One of the most acute inflammations of the hip-joint I have seen set in severely, with an almost agonising pain, directly after a wrench of the joint in quickly turning round.

Thus, then, you can get little help for diagnosis from either the seat or the apparent cause of the malady; they are for the most part the same for the real and for the mimic affection. Your reliance must be on the examination of all the features of each case, and on a right estimate of the weight to be attached to each. Let me then take in turn each of the signs of inflammatory affections of joints, and see how far they may be imitated, whether separately or together. For although it may be said generally that an inflammation of a joint should be marked by many signs, and that in a well-marked case you may study them all, yet in practice you cannot treat lightly any case which has even one clear sign of diseased joint; for this may be only the first sign, which others will follow, or the last, which has survived the rest; or it may be one which is so exaggerated as to conceal the others. What is wanted in every case of suspected disease is, that you should be able to say positively yes or no, and this you cannot do without knowing the weight in evidence of each usual sign.

First, as to pain. Alone, it is not to be at all relied on for a sign of inflammation of a joint, especially if it be severe. If a patient be ready to scream when the accused joint is touched, and yet the joint is not over warm and the patient not feverish, you may be nearly sure of neuromimesis; and more nearly still if the pain be rather in the parts outside or about the joint than in the joint itself, so that a light touch is said to hurt as much as a hard one, or a pinching of the skin as much as pressure on the joint itself. You must not even rely on what are *supposed to be characteristic* pains, such as those felt at the *knee for disease of the hip*, or about the middle of the arm for disease of the shoulder, or even those grinding and burning pains at night which some regard as characteristic of ulceration of cartilages. All these may be mimicked.

I had a boy, aged about 15, in the hospital, who had these night pains in a most marked degree in one knee, and the joint was a little swollen; and he, being tuberculous, was wasting, hectic, and very ill. I did not doubt that he had destructive articular disease, and the use of the actual cautery cured his night-pain; another reason, some would have said, for being sure that his articular cartilages were ulcerating. Soon after this he had similar pains in or near the tibio-fibular joint, and these also were cured with the cautery. But he went on and died with pulmonary tuberculosis, and I examined his knee-joint, and found it almost healthy. A very small piece of one edge of the cartilage on the femur appeared eroded, but it was a trivial change, and quite inadequate to account for the severe pain of which the lad had long complained.

The case had been one of nerve-pain at the joint, and the imitation of organic disease had been made closer by the signs of the coincident disease of the lungs. But for this, it might have illustrated what you may take as a general rule, that acute pain in a joint, if it has existed for even a few days without either local or general increase of temperature, is not a sign of acute inflammation of the joint. It may be rheumatic disease or rheumatic gout, or some slight inflammation after injury, in a neuralgic person, but it is not acute inflammation.

There is more difficulty in judging of the meaning of pain in a joint when it is not severe, but dull, aching, "wearing," as patients describe it. You must be cautious in these cases. Pain alone is not enough to prove organic disease; yet the lower degrees of pain seldom exist constantly and long without some organic mischief. I have indeed known several such cases, especially of pain at the hip, the conclusion of which made me believe that the pain was only nervous, for the patients got at last suddenly or very quickly well, without stiffness or other apparent change at the joint, or with a shifting of pain to some other part. But in all such cases you must be cautious and watch for other signs of disease adding themselves to the pain, such as local heat, swelling at the joint, wasting of parts about it, and others that are least dependent on the sensitive nervous system.

The pain I have been speaking of is that which may be felt in the joint even while it is at rest. Different from this is the pain which prevents the free movement of the joint. It is observed alike in the real and the mimic disease; and you may often judge the pain to be mimicry by its inconsistent severity. If with scarcely any other sign of disease a joint will not permit the slightest movement because of pain, you may greatly suspect the reality of disease; but be very careful not to overlook the signs of increased pain on movement in cases of slight real disease, especially in the diseases of the hip in children. You may often find this the only sign making it clear that a child has real hip-disease. A suspected joint may allow free and smooth and painless movement till, for instance, in extreme abduction of the thighs one adductor becomes much tenser than the other, or in extension the loin is quickly raised, or by some other movement, it becomes evident that the joint will not allow extreme movement without pain, although within all but its extreme range its movements may be free and painless.

In these, among other cases, chloroform or ether may help in diagnosis. In the real disease, as in the mimic, while the patient is utterly insensible, the joint may be moved as widely as in health, unless indeed there be such changes in its structure as might alone have proved its disease; but commonly you will observe that, in the real disease, the muscles become alert, and restrain the movement of the joint before the patient has regained consciousness; while in the mimic disease there is no restraint till consciousness is completely regained. The test is a delicate one, but I think I may be sure that it is a true one, and fit to be relied on, whenever the chief sign of disease of a joint is a restraint of movement on account of pain and the guarding action of muscles.

Closely allied with this pain on movement of an inflamed joint is its stiffness, with contraction or other set posture, dependent on muscular action; for this *posture, whether* it be due to choice or to reflex movement, is the posture of *greatest ease, or of* safest guard against weight or shock or other causes of pain. *The absence, therefore,* of the fixed, or nearly fixed, posture usually observed in a diseased joint, may always suggest the suspicion of mimicry. It would be rather strange to see a hip or a knee extended after many weeks of such pain as would be felt in an acute arthritis, unless, indeed, they were rheumatic or gouty, with exaggerated pain, or had been carefully maintained in good position. It would require the presence of many other good signs of real disease to counterbalance the absence of this sign; for diseased joints, left to themselves, will be habitually, or always, in the position easiest to the patient.

But the reverse of this is not true. Very commonly a joint mimicking disease assumes the posture of disease—assumes and maintains it stiffly in even an extreme degree. This may be seen even when there is no objective pain in the joint; but much more, when the joint is a little really painful, as after a blow or strain, the nervous condition of a patient may either make this pain so intense as to demand the position of greatest ease, or may bring about this position for the relief of even a little pain. Especially the posture of hip disease is apt to be imitated by the drawing up of one side of the pelvis and rotating it, so that the limb looks shortened.

The conjunction of pain and stiffness in a joint always looks like real disease; but you may generally detect the mimicry by observing that, while these things would indicate disease of much severity, everything else is as if there were no disease, or at most only a very mild arthritis. The inconsistency of the several parts of the case exposes its true nature. Lately I had to see a young lady, with a reported healthy nervous system, whom I found lying in bed with extreme contraction of the thigh towards the pelvis, pain at the hip and knee, increased pain on any touching near the hip-joint, and especially great pain and tenderness at and about the tuberosity of the ischium. She could not bear the least attempt to straighten the joint, and the contraction was said to be persistent during sleep. She had had vomiting, hated food, and looked ill and distressed; and all these troubles were ascribed to slight injury or over-fatigue a few days previously. The case looked very like real disease of the hip; but if it had been so at all it must have been a very acute disease, swift and severe, such as should have had attendant fever. Yet the pulse and temperature were natural, and there was certainly nothing in the case which might not be explained by mere nerve-disturbance. And the event proved that there was no organic disease, for after a few days' rest, with careful food, and some wine, and mental quietude, the pain ceased, and then very slowly the limb resumed its natural power and posture, and the young lady married.

Let me here tell you of two or three notable groups of cases of pain and stiffness of joints without real disease. None are more frequent than those of boys from ten to fifteen, who complain of pain and declare themselves unable to walk after injuries of the ankle or knee, or who disuse their arms for pain after injuries of the elbow or other part. They describe the pain as horrible, and hold their limbs in some unnatural position, and limp and cry when you try to make them walk; yet you can find nothing wrong in the shape or size, or temperature of the joint, or in the general health. It is very hard to say whether they are shamming or neuralgic; but the utter inconsistency of their cases proves that they are the one or the other, and you must make them use their limbs. They will often do it better if you have first moved the joint severely for them, and "loosened" them.

Something like these, yet different in having little or no pain, are the children who mimic a disease of a joint through fear. After a hurt, the immense care bestowed on a joint, and the earnest injunctions not to move it, seem to impress some children's minds so deeply that, long after all is well, they hold up their joints scrupulously and timidly, and dare not try to use them. You may amuse yourselves with the astonishment with which both child and parents find, on a positive assurance, that walking, or any other use of the lame limb, is quite easy and painless.

Another group allied with these is, that in which you find young people with joints contracted by involuntary and quiet muscular power after injury. The joints are painless, unless when great force is used at them; and you can easily feel that their stiffness is not through inflammation or adhesion, but muscular resistance like that which sometimes produces wry-neck directly or very soon after a blow. You feel a kind of elastic recoil at efforts to move them, as if the effort was resisted by a tough elastic substance. Ether or chloroform decides at once the diagnosis. As soon as the patient is insensible, the joint becomes moveable as widely and as smoothly as an uninjured healthy one, and only slowly, if ever, stiffens again.

Just like these are the ordinary cases of painless stiff joints in hysterical girls,—the muscles hold them fixed, and that is all; the joints are healthy to the touch and the sight, and even to the patients are painless unless violently used.

In all these cases the ground of diagnosis is essentially the same. You have one or two or three signs of the disease of a joint present in an extremely marked degree, or at least well marked; a pain or a stiffness, or both, and limping or other consequent disability, such as would exist in a joint severely or long diseased. But with a joint really thus diseased there should be—unless it hid some chronic, rheumatic, or gouty trouble—swelling, or heat, or spoiled shape, or general wasting of the limb, or all these together; and with them usually some disturbance of the general health. The absence of these is weightier evidence than the presence of the other symptoms.

All that I have just been saying relates to the distortion of joints produced by muscular action, not to deformity due to displacement of one or more of the bones forming the joint, such deformity as you see, for instance, in the knee when the tibia falls backwards and outwards from the femur, or is rotated by the weight of the foot, on which also the bedclothes may have been allowed to rest. When this, or any similar deformity exists, it is a nearly sure sign of real disease, past or present, for it can scarcely happen except through textural changes, through softening of the ligaments and other textures about the joints permitting one bone to move away from the other. Now, such softening as this can hardly take place except in inflammation. I will not say that it is impossible, but I know that it is very rare. Posture alone, though very long continued, will not produce deformity of joints with displacement of bones. I saw a gentleman who, in a half-lunatic condition, sat for five years in the same posture, and was credibly said to have never moved. At the end of this time his knee-joints were contracted to a right angle, and felt as if absolutely fixed; yet they were not deformed. Their bones had their right relations; and, after some weeks of extension with instruments, the knees were straightened, and power over them was completely gained.—*Lancet, Nov.* 1, 1873, *p.* 619.

3.—ON THE TREATMENT OF FRACTURES OF THE FEMUR BY A NEW METHOD AND APPARATUS.

By Dr. Zach. Johnson, A.M., Surgeon to the Kilkenny County Infirmary.

The management of a fractured thigh is beset with difficulties known to every experienced and practical surgeon, and it cannot be said that any of the apparatus heretofore devised completely or satisfactorily fulfil the intentions of the inventors or the requirements of the practitioner. It will not, then, be considered presumptuous to bring under notice, in addition to the many in use, one which has been subjected to the test of hospital experience for many years, and has been found to possess advantages over its rivals, which the following description of its principle and construction will explain.

It is not proposed to enter here into a complete dissertation on the treatment of fractured thigh, and the various plans from time to time proposed and recommended by eminent surgical authorities, nor to examine into the respective merits of the inclined and extended positions. With the received opinions on these points it is presumed the readers of this article are already acquainted. The arguments of the late lamented Professor Houston, so lucidly propounded in his admirable little *brochure* on the advantages of the extended position in treatment of fractures of the lower extremity, leave nothing to be said on this point. As a rule, then, the extended position is that now accepted as the best, when practicable. The cases are unusual which do not admit of it, though it may here be incidentally observed that there are two in which it is not available, and though these are rare, as they occasionally present themselves in actual practice, they should not be overlooked. The one is where the fracture occurs in the upper third of the femur, in which case the upper fragment may be tilted up, almost at a right angle, by the spasmodic action of the psoas and iliacus muscles. Here there is no resource but the inclined plane for some days, until the irritability of the muscles has been overcome, when the limb may be gradually brought down to the horizontal position, and the extended plan of treatment ultimately put in practice. The other where, with a knee anchylosed at an angle, the femur is broken; and here, obviously, the inclined plane alone is available throughout.

It is, however, with the extended position we have more immediately to do. It is for the more effective management of the limb in that position the apparatus about to be described was specially devised and subsequently tested, although it will be seen by-and-by, it is capable, on an emergency, of being adapted for use in the inclined position also.

The objections to Desault's long splint, with its oblique inguinal and ankle straps, its defective extensile powers, and tendency to excoriate the perineum and groin, are too well known to require further comment. The paramount objection, obviously, is the loss of power arising from the obliquity of the extending and counter-extending forces. The minor objections are several, such as the danger of excoriation of the groin and perineum above, and the ankle and instep below, interference with the use of the bed-pan, &c. &c. These objections all hold good, more or less, in the case of Liston's and Bulley's splints, which, although they present some advantages, retain many of the most serious defects of Desault's apparatus.

Our primary object in the management of a fractured thigh is, that the extending and counter-extending forces should antagonize each other in a *direct* line, which line should be parallel to the long axis of the limb. On this principle depends the efficiency of all our expedients to counteract the inevitable tendency to shortening, caused by the contractions of the powerful femoral muscles. Here it is that Desault's plan fails, the obliquity of the antagonizing forces necessarily entailing great loss of power. Next in importance is the condition that the power should be constant and persistent, such as to weary and overcome the rebellious muscles, while our aim should be to accomplish this in such a way as shall be thoroughly efficient and at the same time in the least possible degree distressing or inconvenient to the patient. With a view to devise means by which to keep up a direct extending and counter-extending power, various expedients have been proposed. For this purpose the weight and pulley was long since adopted and has had very gratifying success in the hands of many practical surgeons. It has been frequently used with satisfaction in the Kilkenny County Infirmary by my colleague and myself. About the year 1852, I published, in the Dublin Medical Press, the details of a most remarkable case where a femur, shortened after a first fracture, was brought to its normal length by this treatment, when accidentally broken a second time, about a year or more afterwards; and Dr. Smith, of Donoughmore, has drawn attention to the fact that the weight and pulley was used to effect extension of the fractured thigh, so far back as 1829, by Dr. Daniell, of Georgia, in America, and by Sir P. Crampton, in the Meath Hospital (Medical Press and Circular, 23rd June, 1869.)

But the weight and pulley, however effective and admirable in its action, has its objections, as every practical surgeon must have found. The difficulty of effecting counter-extension, the tendency to drag the patient to the foot of the bed, intolerance on the part of the patient, generally speaking (if the weight be sufficient to overcome the contractile power of the muscles), all present difficulties which stand much in the way of its universal or even general adoption. Besides it is awkward, unseemly, and inconvenient.

It will be admitted that any apparatus which could embody all the advantages of the several systems now in use, at the same time that it got rid of their several disadvantages, would be a great boon to the practical surgeon, as well as to the sufferer. With such intentions the apparatus about to be described has been constructed. It has been tested amply in actual practice, and the result has been to induce me to submit it to the experience and judgment of the profession.

Among the advantages claimed for the apparatus now proposed, is its simplicity; although a modification of Desault's, Liston's, and Bulley's splints combining the good points of each, with the addition of several innovations unknown to them, it is simple, easy of application, and, best of all, thoroughly effective. It does not excoriate, it can be so used as not even to incommode the patient, while it confers on the surgeon power, which no apparatus hitherto in use has realized.

The apparatus consists of a single long splint and foot-piece, each of which requires separate description.

The long splint resembles Desault's inside splint, but differs from it in some very essential points. The most important of these is the method of effecting the counter-extension. Instead of the perineal strap, which is open to so many objections, the upper or perineal extremity of the splint, which is slightly concave, is applied directly to the perineum. It is softly padded on the extremity with caoutchouc, which again may be covered with chamois, silk, lint or any other movable and non-irritating material. The ramus of the pubis is here made the fulcrum, and it will be seen that, with the foot-piece used, the extending and counter-extending powers act "in directum." The pressure of the splint against the perineum is borne, so far as past experience has gone, without complaint, and has great and obvious advantages over the perplexing and ineffective oblique inguinal strap of Desault's splint. (See Fig. 1.)

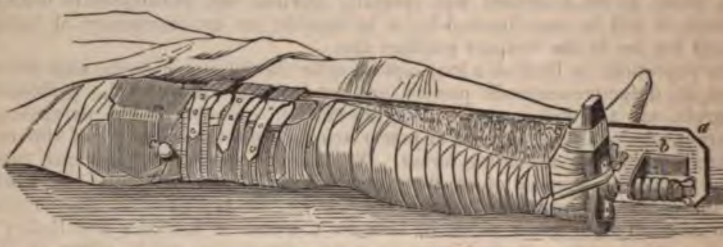

Fig. 1.—Splint applied on inner side of limb. Ramus of pubis the fulcrum of counter-extension. Foot-piece parallel to splint. *a.* Splint. *b.* The standard or fulcrum extension. *c.* The button by which counter-extension is effected, when applied on outer side of limb.

But, should any objection arise to using this fulcrum, a second one has been provided, the idea of which has been borrowed from Dr. Sayer's ingenious method of treating affections of the hip-joint and knee.

A short splint of strong sole leather or flexible material, about some twelve or fourteen inches long, is provided with a stud or gig-apron button, which is firmly rivetted; this splint is to be attached to the outer side of the thigh and pelvis. This can be effectively done by means of oblique adhesive straps, of sufficient length and breadth, laid on in the manner proposed by Dr. Sayer in attaching his apparatus for extension of the hip and knee. The surface of the splint

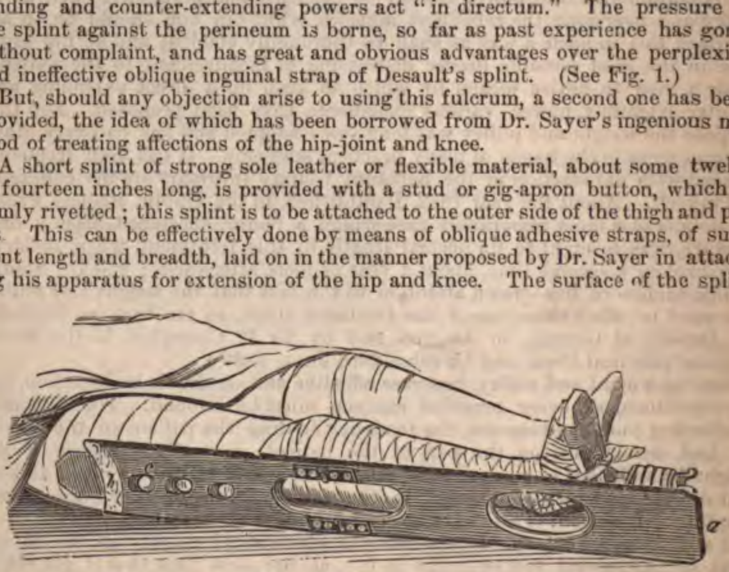

Fig. 2.—Splint transferred to outer side of limb. Fulcrum of counter-extension now the button *c.* acting through whichever button-hole best suits the length of the limb. *b.* The padded end, which rests against ramus of pubis, when splint is applied on inner side. In this diagram the bed has been tilted up, so as to bring more into view.

itself may be made adherent by a coating of starch, or soap plaister, and by these means a degree of fixity can be obtained more than sufficient for any practical purpose. (See Fig. 1 *c*.)

In the upper end of the long splint may be seen a series of button-holes, capable of fitting on the stud in the short splint. By transferring the long splint to the outer side of the thigh, the surgeon can now, if he wishes, make this stud the point of counter-extension, an arrangement which has been found perfectly effective, and which contributes immensely to the comfort of the patient. (See Fig. 2.)

The fulcrum of counter-extension having been decided on, extension is to be effected by means of the foot-piece attached to the other end of the long splint, to the peculiar construction and advantages of which attention is now directed.

By reference to Fig. 3, it may be seen that, by means of a ball and socket joint (*b*) at the back of the foot-piece, it is capable of an infinite variety of adaptations, while, by the use of a pinching screw (*c*) provided for the purpose, it can be firmly and immovably fixed in any required position. The ball is rivetted on the end of a bar (*d*), which moves horizontally, backwards or forwards, through a mortice hole in the standard, or support (*a*), which is affixed to the long splint. On the other extremity of the horizontal bar a winged nut (*e*) plays, drawing the foot-piece towards the standard, or support (*a*), with great power when required. Between the winged nut and the standard may be observed a series of caoutchouc rings or flanges (*f*), the use of which will be explained presently. (See Fig. 3).

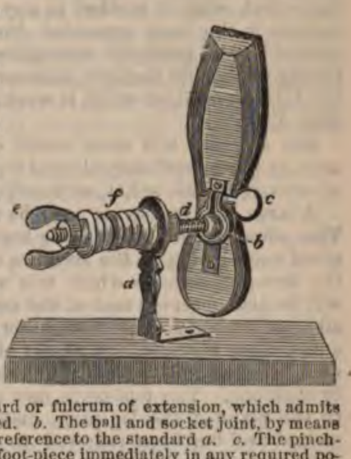

Fig. 3.—Separate view of foot-piece :—*a*. The standard or fulcrum of extension, which admits of being fixed either perpendicular or parallel to the bed. *b*. The ball and socket joint, by means of which the foot-piece can observe any position with reference to the standard *a*. *c*. The pinching screw of the ball and socket joint, which fixes the foot-piece immediately in any required position. *d*. The traversing bar, which runs backwards and forwards through the mortice hole in *a* *e*. The winged nut, which plays on the thread cut on the corners of the square bar *d*. *f*. The caoutchouc flanges, which, when compressed, draw the foot-piece towards the fulcrum *a*.

The advantages of this foot-piece are numerous and obvious. By attaching the foot to it any requisite position may be obtained, and then maintained by a turn of its pinching screw. This is of immense value in injuries of the leg and ankle. The motions of the ball and socket joint allow of any adaptation, and the pinching screw secures it. In fracture of the thigh it obviates eversion of the foot. In fracture of the fibula it can evert the lower fragment.

If we suppose the extending lacs to be attached to the limb, in whatever manner the surgeon may prefer, and the foot-piece brought into apposition with the sole of the foot, by sliding the horizontal bar through its standard(fig. 3, *a*), the lacs are to be tied firmly at the back of the foot-piece, and now extension will be effected by screwing up the winged nut, which draws the foot-piece towards itself with irresistible force.

Thus two important objects are obtained—a steady, gradual force of great power, and one acting in a direct line with the axis of the limb.

It will be observed, that by a simple revolution of the foot-piece on the ball and socket joint the long splint may be applied indifferently to either the outer or inner side of the leg.

Having obtained a powerful extending force, acting "in directum," it remained to render this force persistent and self-adapting, so as to combine, if possible, the action of the weight and pulley with the advantages already gained.

This has been effected in the following manner :—

When tension is applied by so screwing the winged nut so as to draw downwards the *foot-piece, the caoutchouc flanges*, which lie between it and the standard *(and to which attention has been* already drawn), become compressed, and by their elasticity exercise a constant strain on the extending lacs, which can be modified by increasing or relaxing the pressure. Thus the antagonizing action of the muscles may be wearied out. The more they contract the greater the strain on the lacs, while the tension continues, in modified degree, in proportion as they yield and relax, and thus the action of the weight and pulley is simulated; and if the attachments of either extending or counter-extending agents should yield or slip, the resiliency of the caoutchouc will compensate for the derangement, and still keep up an equable tension.

Such are the paramount and essential features of the apparatus. Some points of detail remain to be noticed and explained.

The reason that the horizontal bar is square, is to ensure steadiness when it is desired to fix the foot in any peculiar attitude, by tightening the pinching screw of the ball and socket joint; were it round, the foot-piece could revolve, when it was desirable to have it fixed and steady. The thread of the screw which the winged nut traverses is traced only on its corners, but will be found as unerring and effective as if the circle were complete.

The persistent tension of the weight and pulley might be simulated by elastic lacs, secured to the foot-piece, but with this great disadvantage, that when the lacs yielded the foot would be drawn away from the foot-piece, and would be free to take wrong positions. It is of manifest importance to keep the foot in close apposition with the foot-piece by a slipper, bandage, or adhesive straps, so that when any peculiar position has been attained, deviation from it may be obviated. In order to ensure this it was necessary so to arrange the elastic force that, while it yielded to any extraordinary tension, the foot should not become in the least separated from the foot-piece, and its requisite attitude should be still perfectly maintained. This has been accomplished by interposing the elastic medium between the extending agent (the winged nut) and the fulcrum against which it works (the standard attached to the long splint). (Fig. 1, *b*.)

But as cases will occur which cannot be treated in the extended position, such as those previously alluded to, arrangements have been made so as to allow of converting this splint, on emergency, into a double-inclined plane.

A large oval fenestrum may be noticed about the middle of the long splint. This allows of the splint being kept in close apposition with the thigh, so as to avoid encroaching on the scrotum or vulva, without pressing against or hurting the knee, opposite to which this vacancy has been purposely constructed. It will be also observed that at the centre of this oval the splint is hinged so as to allow of its being doubled together on the outer side. This not only makes the entire more portable, but allows of its being converted, at any moment, into a double-inclined plane, the inclination of which can be easily regulated by pillows or cushions. The foot-piece now revolves to a suitable position, perpendicular to the plane of the leg-piece, instead of being parallel to it as before, while a second oval has been removed, to secure the heel from any contact or pressure. Thus, in case of emergency, a double-inclined plane can be readily extemporised.

When it is desired to keep the long splint straight, for the purposes of extension, the yielding of the hinge can be effectually prevented by inserting a pad of adequate thickness between the knee and the site of the hinge, which will prevent the bending of the splint, whether it be on the inner or outer side of the thigh.

The revolving foot-piece will be found of great advantage in other cases besides those where an extending power is required. In accidents and injuries of the leg and disease of the ankle, where it may be of importance to secure some special position of the foot, it will be found most useful. Here it is to be kept in apposition with the sole of the foot by means of a second pinching screw, whose office is to prevent the square bar from sliding in the standard, and so to keep the foot-piece permanently in contact with the foot, which now derives support from it. If it be desired to effect any peculiar position of the foot, the ball and socket joint enables the foot-piece to be adjusted at any fraction of an angle, and the foot having been previously secured suitably by adhesive strapping, the entire may be fixed in the required posture by a turn of the pinching screw (fig. 3, *c*) which controls the ball and socket joint.

This foot-piece is equally useful for support or extension. It is applicable alike to long splint or inclined plane. It was exhibited by me at the Surgical Society some twenty years ago, since which time it has been amply tested in hospital and private practice. It has been found to possess great advantages in a variety of cases. Where some special position of the foot is required it can exercise either extension or support, as may be most desirable. In the case of fractured fibula it can fulfil the purposes of Dupuytren's splint. For the purpose of direct extension, it has proved more effective than any means hitherto proposed.

With respect to the best method of applying extension in the case of fractured thigh, it may not be amiss to say a few words. It can be applied, as is so frequently done, above and close to the ankle, by means of the familiar clovehitch tie. Here it is apt to excoriate and cause pain by its pressure on the malleoli. A great advantage will be gained by attaching the extending agent near the knee; this may be accomplished by broad adhesive straps applied after the method followed by Dr. Sayer in his treatment of hip disease. By this means double points of attachment may be effected by which a double advantage will accrue. Thus one attachment may be arranged above, the other just below the knee, and by using these alternately the patient obtains great relief. It will be found that the irritability of the femoral muscles will be lessened by applying the extending power high up. When practicable, it is well to attach it above the knee, and relieve the ligaments of the knee from strain, but in any case it can be attached, with advantage, above the calf of the leg.

A decided advantage will also be obtained from the use of short secondary splints, embracing the femur alone. Of these, one in front and one on either side will be sufficient. They may be secured by broad straps and buckles, and their equable pressure tends to soothe the spasmodic irritability of the femoral muscles. Their use was much insisted on by the late Sir Philip Crampton, at the Meath Hospital, and they have been justly recommended by other eminent authorities.

When counter-extension is made against the ramus of the pubes, and the long splint applied to the inner side of the leg, it serves as the inner splint, while the leather splint, with stud affixed, is available as the outer one. The straps and buckles may now embrace all, and by including the long splint will keep it close to the thigh, and prevent any encroachment on the scrotum or vulva.

When counter-extension is effected by the button on the outer side of the thigh, it will not be so convenient to include the long splint in the straps which confine the short ones. The comfort enjoyed by the patient when this method of counter-extension is used is very great. It can be best appreciated by applying extension on the inner side first, and then changing to the outer side. It is fully as effective as any other method, and presents no interference whatever with the use of the bed-pan; thereby contributing materially to the convenience and comfort of the patient.

It would not be right to conclude these observations without acknowledging my obligations to the ingenuity of Dr. Sayre, of the Bellevue Hospital, New York, from whose method of affixing his apparatus for hip and knee disease I took the idea of attaching a fulcrum (fig. 1, *c*) on the outer side of the thigh, a measure which conduces vastly to the comfort of the patient. The mode of applying counter-extension by broad adhesive straps, applied above and laterally, to the calf of the leg, and to which the name of "Pancoast's Stirrup" has since been given, was adopted by me previous to 1850, and referred to in my publication "On the Weight and Pulley," about that time.

I now submit this mode of treating disease and injury of the lower extremity with all deference to the judgment of an impartial profession.—*Dublin Journal of Medical Science, Aug.* 1873, *p.* 101.

44.—SUCCESSFUL CASE OF EXCISION OF THE SCAPULA.

By JAMES SPENCE, Esq., F.R.S.E., Surgeon to the Queen in Scotland, Professor of Surgery in the University of Edinburgh.

[The patient, aged sixty-eight, was admitted to the Royal Infirmary on the 25th of December, 1871, on account of a tumour in the scapular region. The swelling had existed upwards of twenty-three years, but had given him no pain or inconvenience until about six months before his admission, when it began to increase rapidly in bulk and also became painful.]

On examination I found that whilst the part of the tumour in the infraspinous fossa of the scapula was still pretty movable and not much altered in size or form, there was a very marked enlargement towards the axilla, where it pressed forwards, surrounding the neck of the scapula and the shoulder-joint, and nearly filled the axillary space. At the neck of the scapula the growth seemed fixed. I therefore gave it as my opinion that excision of the scapula would probably be required. As the patient was desirous that the operation should be performed as soon as possible, I proceeded to operate on February 21st.

Operation.—As the tumour had originally been of slow growth, and had remained seemingly movable up to a recent period, I thought it advisable, before proceeding to excise the whole scapula, to make my incisions so as to ascertain whether it was firmly attached to the bone or merely bound down by muscles and fascia. I accordingly began by making an incision from the posterior part of the acromion process obliquely downwards and backwards over the tumour to beyond the inferior angle of the scapula, dividing freely the fascia and fibres of the infraspinatus muscle. This exposed the scapular aspect of the tumour, smooth, defined, and so movable that it seemed as if it would have been easily turned out; but at the neck of the scapula and around the glenoid cavity the growth was evidently from the bone or periosteum, and was soft and cerebriform in substance, so that I proceeded to excise the whole scapula. With this view I entered the point of my bistoury over the clavicle internal to the attachments of the coraco-clavicular ligaments, and carried the knife backwards over the acromion into my first incision, so as to complete a long incision from over the outer part of the clavicle to the inferior angle of the scapula. I next made an incision from the superior angle of the scapula obliquely downwards and forwards over the tumour towards the neck of the scapula. The flaps of skin were then dissected and reflected, and the attachments of the trapezius and deltoid muscles to the spine of the scapula divided. I then cleared the clavicle by separating the attachments of the trapezius, deltoid, and great pectoral muscles, and divided the bone, dissecting through the coraco-clavicular fascia, so as to enable the axillary artery to be completely commanded. I now hooked my left forefinger under the coracoid process, and divided the attachments of the pectoralis minor, short head of biceps and coraco-brachialis muscles. The disarticulation of the humerus was next accomplished, and this required care, as the projection of the tumour around the joint made it necessary to be very cautious. The long head of the triceps was then divided, the scapula being carried backwards and drawn from the body, so as to allow me to divide the rhomboid and other muscular attachments with a single sweep of the knife. The loss of blood was very small, for in dissecting off the cutaneous flaps, and in separating the deltoid and trapezius from the scapula, all bleeding points were secured before proceeding farther. Subsequently, from the manner in which the axillary artery was commanded, no bleeding occurred, the subscapular and other vessels being secured before compression was relaxed. I expected to have had bleeding from the artery of the base of the scapula, but could scarcely find a bleeding point in its usual position.

Although there was so little loss of blood, the effect of shock was very obvious when the scapula was removed, as evinced by feeble pulse and sunken features. But this state speedily disappeared, and before the wound was dressed the pulse had recovered after the administration of stimulants.

After all bleeding had been arrested, I examined the wound carefully, and noticed a series of elongated bodies of a prismatic form and a milk-white colour in the position of the axillary chain of glands, and these I dissected out. (They proved on examination to be glands, but contained no proliferous cells.) The whole surface was then washed with tepid carbolized water, and the flaps of skin having been brought together by sutures, a long flat pad of lint enclosing "marine tow" was placed in the axilla and along the inside of the arm, which was then secured to the side by a broad flannel roller. The lines of incision were dressed with strips of dextrine oiled silk, wetted in carbolic lotion, and secured by gutta percha tissue glued down at the edges by touching them with chloroform.

The following extracts from the report in the Hospital Journal will best show the subsequent progress of the case :—

"February 22nd. The patient has passed a tolerably good night; pulse 120; little discharge from the wound. He is getting milk and white of egg for diet, and occasional small portions of brandy and water and ice to allay thirst.

"24th. Patient much as at last report, but troubled with severe bronchitic cough. Two stitches were removed to-day. The wound is looking very well, and there is only very slight discharge.

"25th. Patient has had less cough, but complains of dryness of his mouth. The tongue is moist and not much furred. Pulse has fallen to 108, and is of fair strength. Diet, milk with white of egg, beef-tea, and brandy. Quinine in doses of 1 gr. with dilute sulphuric acid in water to be given thrice a day.

"27. The cough has again been severe, otherwise the condition of the patient is satisfactory. The discharge is very small in quantity, and has no odour. Some stitches were removed to-day, and the greater part of the incisions corresponding to the back of scapula seem united by first intention. There is a little irritation and discharge at the wound over the section of the clavicle, and the head of the humerus has a tendency to press against the cicatrix at the upper part, causing it to break up.

"March 1st. Patient going on favourably. Cough less. The tongue is slightly furred; pulse 108, regular, and good strength. Ordered a cathartic, as his bowels have been confined.

"4th. Patient complains of feeling weak and is very irritable, but his general condition is good. Pulse 104; temperature of skin natural. Diet, steak or chop, with sherry 4 oz., and 2 oz. of brandy.

"11th. Patient is more cheerful; is still troubled with his cough, but takes his food well. Pulse 100. All stitches have now been removed some days since; all the large wound has closed, with the exception of the upper part, where the head of the humerus still tends to project, no matter how the arm is placed, and even when he is sitting up in bed with the elbow unsupported.

"13th. Pulse 90; little or no discharge.

"23rd. Progressing favourably.

"26th. Patient has had a fresh attack of bronchitis, which has rather exhausted and dispirited him. The wound is healed, except at upper part, which is now contracting.

"April 13th. Patient allowed to get up. Wound nearly healed over head of humerus. His cough is greatly better.

"May 7th. Dismissed cured."

Remarks.—The cases in which excision of the entire scapula has been successfully performed are as yet few, and such instances as that just recorded are worthy of publication as encouragement to operate. The foregoing case has, moreover, some interest in other respects as regards the history and character of the tumour, and as drawing attention to the operative procedure which should be adopted to facilitate the after treatment of the wound.

The tumour for which excision was performed presented some peculiarities when we contrast its vital manifestations with the structural appearance and invasion of other tissues. It had originally, and, indeed, up to a recent period, presented all the vital characters of a simple growth—very slow progress during twenty years, no pain, mobility and definition, and absence of any constitutional cachexy or disturbance; but its structure when cut into presented at one part all the appearance to the naked eye of medullary growth, and it had not only involved part of the capsule of the shoulder-joint, but had also infiltrated the neighbouring muscles and axillary glands which were removed.

These conditions were at variance with all my past observation of tumour growth. A very considerable experience had led me to lay down as an axiom that when a tumour was defined, of slow growth and possessed of the other manifestations of simple tumours, we might safely reckon on its maintaining that benign character, however much it might alter locally—that is to say, such a tumour, though it might ulcerate and adhere to other textures in consequence of local irritation, would not invade other tissues as malignant growths do; nor would it affect the constitution with true cachexy, and might be safely removed without risk of reproduction. In this case, however, the section of the tumour, the involvement of other structures, and more especially the infiltration of the long head of the triceps and other muscles, and the neighbouring chain of glands, so impressed myself and others who saw it that, on exhibiting the tumour to the Medico-Chirurgical Society, I expressed myself as rather shaken as to the views I had long held by what seemed so marked an exception to what I had hitherto observed. Subsequent microscopic investigations, however, would seem rather to confirm the axiom I have long held, for neither in the medullary-looking part of the tumour nor in the altered axillary glands was there any appearance of cancer cells.

The trouble experienced in the after treatment of the wound in this case has led me to reconsider the method of operating, and whether that might not be modified with advantage. The troublesome part of the treatment was the difficulty of obviating the constant tendency of the head of the humerus to be protruded at the upper part of the wound. Whilst the large posterior lines of incision corresponding to the body of the scapula healed almost entirely by the first intention, that part of the wound which corresponded to the outer end of the clavicle and acromion process was prevented from healing, and the cure rendered tedious by the pressure of the head of the humerus, and also to some extent by the irritation caused by the edges of the section of the clavicle, and when this part of the wound did heal, the head of the humerus was adherent to the cicatrix.

In the special memoir of a successful case of excision of the scapula published by the late Professor Syme no such difficulty is mentioned, but the reason is obvious. In Mr. Syme's case the head of the humerus had been excised some time previously on account of disease, and hence both the operation and after treatment of the wound would be simplified.

At first sight it may seem that there would be little difficulty in obv ating the protusion of the head of the humerus. That by merely attending to position, keeping the arm at a right angle with the trunk, or elevating it so as to depress the head of the humerus, the object in view would be attained. Such was my own opinion at first, but when I tried to put these plans into practice I found that they did not answer my expectations. When the arch formed by the acromion and clavicle is removed and the scapula excised, the cavity of the axilla ceases to exist, and the head of the humerus, however placed, is close to the surface, and has a tendency to be drawn or pushed upwards by the action of the pectoralis major. Moreover, it was found impossible to retain the arm in the positions indicated for any length of time. The only method that had any effect, and that very slight, was fixing the arm by bandaging it to a broad roller fastened round the pelvis.

Resection of the head of the humerus in such circumstances is a very obvious method of getting rid of the difficulty, and Mr. Syme's case is an instance in point; but unless the part was implicated by the disease, I think it would be unadvisable to add to the risks of the operation by such complication if we can attain our object otherwise.

The method I would adopt now would be to disarticulate the clavicle from the scapula, instead of sawing it through. By this means we would avoid a source of irritation produced by the section of the clavicle, and at the same time by leaving the broad flat acromial extremity of that bone we would have a *point d'appui* with which the head of the humerus could be kept in contact, and so prevented from protruding, and there a false or new joint might ultimately be formed.

Such a procedure might render the operation in some respects more difficult, because by sawing through the clavicle, as in the case narrated, the operator can most readily expose the great vessels so as to enable the assistant to command them thoroughly, and he can almost easily expose and deal with the muscles attached to the coracoid process; whereas in disarticulating the clavicle from the scapula he requires to divide both the acromio and coraco-clavicular ligaments from above and behind after separating the attachments of the trapezius, and to avoid division of the integuments below the clavicle. Practically, however, I believe the difficulty could not be great, for I have on one occasion excised the scapula by this method in a case in which, after removing a large medullary cystic tumour from the axilla, I found there was a medullary growth from the subscapular fossa. In that case, owing to the exhausted state of the patient, I had to proceed very rapidly, and on the spur of the moment I divided the muscles attached to the base of the scapula and the trapezius, threw forward the scapula and arm, divided the coraco-clavicular ligament, separated the clavicle from the acromion, and removed the scapula after disarticulating the shoulder-joint. In Dow's case I adopted what I considered the more methodical plan of operating to secure against hemorrhage, as already mentioned; but I believe that important object could be equally attained by direct compression above the clavicle through the incision necessary to separate the trapezius muscle, whilst the extent of the wound would be diminished, its after treatment simplified, its healing expedited, and a more useful arm left.—*Dublin Journal of Medical Science, June* 1873, *p.* 508.

45.—ON EXCISION OF THE EXTREMITY OF THE HUMERUS AS A REMEDIAL MEASURE IN CASES OF ANCHYLOSIS OF THE ELBOW-JOINT RESULTING FROM INJURY.

By Dr. PATRICK HERON WATSON, F.R.S., Senior Surgeon, Royal Infirmary, Edinburgh.

In cases of injury to the elbow-joint in which anchylosis, partial or complete, has resulted, excision of the elbow-joint has long been recognised as a legitimate procedure, the movements of the limb resulting from the excision of this joint being so very much more perfect than those effected when either complete or partial anchylosis impairs the flexibility of the elbow.

The resulting limb, however, in cases of excision for anchylosis, has not, in my experience, been so satisfactory as in those cases where disease has been the occasion of operative interference. On the one hand, too great a degree of mobility in every direction has been the result; on the other, the union between the divided ends of the bones has been more complete than could be desired, and the movements have been commensurately imperfect. In one case, when the latter condition resulted, I practised resection no less than three times, cutting away the osseous textures freely, but on each occasion ossific growth rendered the movements of the limb very unsatisfactory. In the former class of cases (where too great mobility has been the fault), a want of muscular development has apparently formed the chief obstacle to the usefulness of the limb, and the want of controlling power of the *triceps* in extension has appeared to be what was mainly deficient.

These observations had forced themselves upon my attention for a considerable time, during which I had continued content with the simple operation of resection of the articular surfaces of the humerus, radius, and ulna. The first modification I attempted with a view to saving an insertion of the *triceps*, consisted in performing, in the summer of 1869, in a case of fracture of the end of the humerus, a subperiosteal resection of the elbow-joint. This was effected by a straight or linear incision through the *triceps* and over the olecranon; the insertion of the *triceps* and the periosteal covering of the upper end of the ulna *were then turned* aside, and the olecranon cut off obliquely, so as *to divide the coronoid process* as well, and thus detach both the greater and lesser sigmoid cavities of the ulna. The end of the humerus was next cut off by the saw, an excision director having been carried round its anterior aspect, in the groove of which the key-hole saw had free play. These portions of bone having been extracted through the wound by means of sequestrum forceps, the head of the radius was clipt off by the bone-pliers. The result of this case was not satisfactory, extensive osseous deposit rendering the movements less perfect than was anticipated. I repeated this method, however, upon two occasions, but in both there was too little movement to satisfy every requirement.

In the summer of 1871, I operated upon a boy by a new method, which I conceived would fulfil every indication, so far as the preserving muscular attachments was concerned, and at the same time enable me to effect the removal of as much of the osseous textures as might appear to be necessary. The speculative reasonings which led to my adoption of this method were these:—

It was quite obvious that, as in most cases of fracture into the elbow-joint, the humerus was the bone alone affected; no changes in the osseous structures of the radius and ulna necessarily resulted from any injury the humerus had sustained; nor even should the radius and ulna be involved in the injury, did the resulting efforts at repair constitute a condition which implied any need for their removal by operation. It was also obvious that the removal of the upper extremity of the ulna necessarily impaired the perfection of the muscular attachments, viz., of the *triceps* and the *brachialis anticus*, and indirectly the power of the *biceps* in flexing the forearm. It was clearly, therefore, very desirable that neither the radius nor ulna should be interfered with, if removal of the extremity of the humerus alone would suffice to remedy the anchylosis.

The operation I devised for carrying out these theoretical requirements consisted in the following steps:—(1.) A linear incision to be made over the ulnar nerve to the inner side of the olecranon process rather longer than that usually employed in the ordinary excision of the elbow by linear excision. (2.) The ulnar nerve to be turned over the inner condyle by careful dissection. (3.) A probe-pointed bistoury to be introduced into the elbow-joint in front of the humerus, and then behind that bone, and carried upwards, so as to divide the upper capsular attachments in front and behind. (4.) A pair of bone-forceps to be next employed to cut off the entire inner condyle and trochlea of the humerus, and then introduced in the opposite diagonal direction, so as to detach the external condyle and capitulum of the humerus from the shaft. (5.) The truncated and angular end of the humerus to be cleared, turned out through the incision, and smoothed across at right angles to the line of the shaft by means of the saw, whereby (6.) room might be afforded, so that partly by twisting, partly by dissection, the external condyle and capitulum are removed without any division of the cutaneous tissues on the outer side of the arm.

This operation, it will be observed, by the single linear incision upon the inner side of the arm, enables the operator to drain efficiently the entire area of operation, and through an incision of very moderate limits to remove the entire expanded extremity of the humerus, without interfering with any muscular structures, except those of the forearm, which take origin from the osseous tissues actually excised. The result in this instance was perfectly satisfactory, the movements of the forearm being restored so as to maintain a degree of muscular power not usually observed in cases of ordinary excision of the elbow.

Shortly after this, in August 1871, I operated in precisely the same manner upon a young gentleman suffering from anchylosis of the elbow-joint, consequent upon a fracture of this joint. Here not only had the humerus suffered, but in the course of the operation the coronoid process of the ulna was seen to have been broken off, leaving as a result a line of fibrous union traversing the greater and lesser sigmoid cavities of the head of the ulna.

All went on well in this case, till the wound had almost healed, when *chronic osteomyelitis* of the humerus set in, and a rigidity began again to *return, which resisted* diligently-maintained passive motion effected under the influence of chloroform. In these circumstances I again opened up the line of incision, and found that a long spike of new bone, developed from the granulation tissue, which projected from the cancelli of the centre of the divided surface of the humerus, had become united to the surface of the ulna. This projection, along with half an inch more of the shaft of humerus, I then cut off.

After this the wound healed, and the movements of the forearm became steadily restored.

In the same week I operated upon a lad sent to my care from Caithness-shire, who had met a severe injury to the elbow-joint by falling from a roof. I practised the same operation I have described, and had the satisfaction of obtaining a most perfect result.

I have since then operated upon two female patients and a child in precisely the same manner and with equally satisfactory results.

I have thus practised this mode of operation in six cases in all. With a single exception, a satisfactory result accrued immediately from the operation. In this single case, when an attack of osteomyelitis supervened upon the operation, and osseous union was threatened between the humerus and bones of the forearm, the secondary removal of a further slice of the humerus afforded an ultimately satisfactory issue.

The merits of this operation, which, so far as my observation and reading go, is an original one, consist—(1.) In leaving the attachments of the *triceps* and *brachialis* undisturbed, affording therefore a degree of leverage in the movements of the forearm which cannot be attained when the olecranon, or any portion of the upper end of the ulna, are interfered with or removed. (2.) In limiting the area of operation almost exclusively to within the capsular ligament of the elbow-joint, which seems to secure more speedy healing of the wound than would otherwise occur. (3.) In securing, by the line of incision being internal and posterior, less ultimate surface deformity, a more direct drain for discharge, and a more ready access to the ulnar nerve than by any other method.

One objection only can be taken to this mode of procedure, viz., that it does not afford a ready access to the external lateral ligament of the elbow-joint; this, however, is of trivial importance, if the plan of procedure I have laid down be rigorously carried out in the division and removal of the end of the humerus, viz., 1. The oblique division of the condyles of the humerus from above downwards, so as to cut through the articular surface by means of bone-pliers between the trochlea and capitulum of the humerus. 2. To cut off the capitulum and external condyle obliquely from the shaft by means of pliers applied from below upwards. 3. To turn out the end of the shaft and cut off as much of its truncated and conical extremity as may be deemed requisite; and, lastly, to dissect and twist away the capitulum and external condyle from their remaining ligamentous and other attachments.

It may be urged, that while this may be easy enough when there is only partial rigidity of the elbow-joint, it is impossible to effect it in cases of complete and absolute anchylosis of the elbow-joint. But such an objector must not fail to recollect, that absolute anchylosis of the osseous kind is not a common result of fracture into the articulation, especially when passive motion has been attempted to be kept up after the accident; that in most of these cases it is rather due to the altered form of the osseous surfaces resulting from the fracture and displacement, and that at most the anchylosis is usually fibrous in its character.

Again, even were it present, forcible flexion and extension under chloroform will, in the great majority of cases, effect such a degree of solution of continuity as will enable the operation to be carried out in the manner already described without any real difficulty. Should any case occur of very dense osseous union of the articular surfaces, rendering the risk of fracture of the olecranon or of the shaft of the humerus a reasonable danger possibly involved in such strenuous effort, then a transverse section of the humerus with bone-pliers through the condyles, excision of a portion of bone above this level, and piecemeal excision *of the anchylosed condyles* themselves, by means of the forceps *and gouge, would afford an* alternative means calculated to remove any ordinary difficulties; while the conversion of the operation into a complete excision of the elbow-joint may always be had recourse to should insuperable obstacles be found to prevent the execution of the more limited resection.—*Edin. Medical Journal, May* 1873, *p.*985.

46.—ON THE TREATMENT OF LUXATIONS BY ROBERT'S MODIFICATION OF JARVIS' ADJUSTER.

By William Stokes, Esq., Professor of Surgery, Royal College of Surgeons, Surgeon to Richmond Hospital, Dublin.

[Mr. Stokes first witnessed the use of Jarvis' adjuster in the clinique of Professor Nélaton, and from the results he saw, formed a high opinion of it. A short time subsequently, a modification, and in many respects an improvement on the original adjuster, was devised by M. Robert, of Paris, and it is to this appliance that attention is especially called in the following paper.]

In overcoming the muscular resistances which oppose the reduction of dislocations, the surgeon has, as a rule, hitherto employed one or other of two kinds of force, viz., either physical or mechanical. The former has often succeeded when the latter has failed, but it frequently fails from the surgeon and his assistants being unable by it to use a sufficient amount of force. The mechanical, on the other hand, I am persuaded, often fails from its being applied between two points which, during the attempt at reduction, remain fixed. It appears to me that the great value of the instrument, to which I wish to draw attention, arises from the fact that it combines the advantages of both the physical and mechanical methods of reduction, for while the direction of extension and counter-extension can be altered at will, it furnishes an extending force which is unlimited in principle. The indications, as mentioned by Jarvis, which this instrument is intended to fulfil are—

1. To establish a line of extension and counter-extension between any two described points, and to be enabled to change the direction of the force without any previous relaxation of it.
2. To furnish an extending force which is unlimited in principle, yet easily calculated, and which is perfectly subject to the will of the operator.
3. To be enabled to estimate with mathematical accuracy the amount of force used.
4. To enable the operator to apply the force, whether direct or transverse, either rapidly or slowly, and to retain it on the limb during his pleasure.

That a violation of mechanical principles in the application of force in the treatment of dislocations frequently occurs, and that this leads often to failure and sometimes to grave accidents, which seriously complicate the case, there can be no question, and one prime reason of the frequent occurrence of these failures and accidents has been to a great extent, as Jarvis has rightly shown, owing to the employment of forces which operate from fixed points over which the surgeon has no control. "Thus," he remarks, "if the line of extension happens to be the correct one, he (the surgeon) does not desire it to be changed, but if it happens to be incorrect the force must first be relaxed before he can change it." This difficulty, which the surgeon has often to contend with in attempting the reduction of dislocations, is chiefly evident in cases where the luxated extremity of the bone is driven into muscular tissues which tightly envelope or grasp it. Many illustrations of this might be adduced. I may mention one instance by Jarvis. He observes:—"Suppose the case where the head of the bone is driven into the soft tissues and the neck is closely embraced by them, as, for instance, in some cases of the head of the os humeri, we must first disengage the head before we can even hope to succeed in reduction; suppose, also, we apply the pulleys; force is steadily and long continued, but owing to the firmness with which the neck is embraced, and the strength with which the muscles contract, it is not relieved from its confinement. Is it not plain, therefore, that should the extending point now be subject to the will of the surgeon to move at pleasure, while the force is yet constant and steady, to give free motion to the limb would greatly aid him in reduction and that without increasing the power?"

Many other illustrations I might also mention, as, for example, in certain luxations of the femur where the head and neck are forced obliquely into the muscular tissues, which, tightly embracing the neck of the bone, must, most probably, have to be torn through, and, in truth, often are in attempting to replace the bone in its normal position.

The apparatus which I think goes far in getting rid of these difficulties and accidents, consists of a steel case of the dimensions of an ordinary gun-barrel, open at the upper surface and containing a rack-bar. At one extremity of the steel case the pinion wheel is fixed. The rack-bar constitutes the extending bar. The counter-extending-bar is fixed, not at the extremity of the rack-bar case, as in Jarvis' adjuster, but to the thigh fork, the shoulder collar, or the elbow fork. The shaft of the pinion wheel terminates in a square heel which fits into a corresponding square opening at the extremity of the lever; and which passes through the centre of a ratchet wheel which is fixed to one extremity of the lever. The rack-bar case is so adapted as to fit into the end of the counter-extending-bar, and the two bars, the extending and the counter-extending-bar, are united by the dynamometer which is fixed by two hooks and which are close to the extremities of the two bars. The effect of turning the pinion wheel by the lever, is to increase the distance between the extending or rack-bar, and the counter-extending-bar. This latter is attached, for luxations of the upper extremity of the humerus, to the shoulder-collar; for those of the elbow, to the elbow-fork; and for luxations of the femur, to the thigh-fork. These appliances can also be adapted to luxations occurring at the knee and other articulations.—*Dublin Journal of Medical Science, August* 1, 1873, *p.* 89.

47.—DISLOCATION OF HUMERUS AT SHOULDER-JOINT.

By Dr. Charles Kidd.

In dislocation of the shoulder, chloroform is of vast service: thus cases of forward and inward dislocation, old cases especially, will fail with pulleys; but by elevating the elbow to a level with the shoulder, rotating the arm, and then by the little manœuvre of drawing it back, so well known now under chloroform, the bone goes in at once. A late house-surgeon at University College Hospital relates nine such cases. Parker, of Liverpool, cites several cases also (1872), the glenoid fossa being, he thinks, the support for the neck of the humerus, but the joint is almost always set right under the "manipulation" plan.

The old methods of reducing old dislocations appear to us now very mischievous and erroneous, if not cruel. Practically, there are perhaps but two dislocations of the humerus at the shoulder that get mixed up with our chloroform exercises: the one, forwards and inwards under the pectoralis; and the more familiar and frequent, accident, downwards into the axilla, the head of the humerus here pressing against the inferior costa of the scapula; between these (always uninviting structures in chloroform administration) large tendons, viz., those of triceps and subscapularis; for we hold injury of tendons a more frequent cause of death by chloroform than heart disease.

Need we remind the reader of these large tendons or the torn capsule; the glenoid cavity, so shallow; the stretched muscles; the difficulty, especially in old cases, of stirring the fixed head of the bone. The student will be pleasantly told these dislocations of humerus form at least half of all dislocations put together; and of these, half again are sub-glenoid, the rest subcoracoid or forwards, and about 5 per cent. sub-spinous. We pass over the immediate or diagnostic marks of the several lesions, confining present remarks to the manipulations that now so often *go together* with chloroform; nor need we stop to *specify the three dislocation symptoms*: want of roundness in the shoulder, hollow under acromion, and acromial projection, that every industrious student in the out patients' department is aware of.

Some no doubt will think us over tedious as to symptoms, or injured tendons; the severity of the symptoms, as once explained in a college examination, being like measles in proportion to the attention bestowed on them; but it is not so.

Chloroform has systematised what Mr. Abernethy used to style "stratagem," or taking the patient, or rather his muscles, unawares, as in that way the muscles were relaxed. Now the surgeon places the dislocation patient deeply under chloroform (it must not be nitrous oxide, under which the muscles are not relaxed), then with one hand grasping the shoulder, the flexed elbow with the other (when the dislocation is sub-glenoid or subcoracoid), the thumb is next placed over the head of the errant bone, and fingers over the spine of the scapula, the thumb acting as a fulcrum; so much for one hand. Then, with the other hand, the painfully flexed elbow should be drawn from the side, extension made; some slight rotatory movement outwards also employed. The surgeon's anatomical knowledge and *tactus eruditus* will here help him much; when extension has been thus carried to its full extent, the elbow is then raised, the arm made to describe a sort of semicircle towards the sternum; then suddenly brought down to the side, the head of the bone rotated inwards, the thumb giving the right direction to the latter, and all comes right.

Without this accuracy of symptoms and anatomical knowledge and attention in this good rule of thumb way, the surgeon will not succeed, but with it, the cure is little short of miraculous; there are modifications, such as pressing the knee into the axilla; a jack towel with clove-hitch knot on the patient's arm, two or three assistants making steady extension horizontally outwards; but we doubt if any plan is so perfect as this new method, almost the reverse of the older one. The plan of White, of drawing the arm vertically upwards is another. It has succeeded where the manipulation has seemed to fail, but both are worthy of a trial in the more puzzling cases.

In these old dislocations of humerus as well as in hip cases, there was formerly, what with pulleys, torn capsules, bandages round the arm to secure a good pull; what with clove-hitch knots, jack towels round the chest, a sort of daring or engaging barbarism that much excited the earlier students; all is now changed, ropes and pulleys and pull altogether are gone; "a comic reading these as the ceraseur chain to a breast tumour," quietly observed Sir W. Lawrence to us once, "these triple folds and brass round the chest.

Aës triplex,
Circa pectus erat, qui fragilem truci, &c.

A comic reading of the hazard to joints and capsules, and hardiness, and shipwreck made of some cases; recalling the ship in Horace, and the first mariner with his pulleys, and sea sickness."

Pulleys, indeed, amongst all right-thinking surgeons, have given way to anæsthetics; in recent dislocation, perhaps, neither is required; but in old ones, adhesions should be broken through or made to yield by forcible rotation and flexion, rather than torn and dragged by pulleys; or these triple girths or bandages, or dragging the arm like a pump-handle vertically upwards. We are told now of such cases, a year old, quite cured by chloroform.

There are many cases of old unreduced dislocation, such as that of the astragalus, patella, &c., which also readily yield under chloroform.

Who shall say, then, that not only in reducing the agony and shock of such operations, but facilitating the cure, chloroform is not a blessing?—*Students' Journal and Hospital Gazette, Oct.* 25, 1873, *p.* 324.

48.—TWO CASES OF DISLOCATION OF THE HIP TREATED BY THE AMERICAN METHOD OF MANIPULATION.

By Dr. Hector C. Cameron, Glasgow.

In a former series of this Journal (July 1866), I related a case, in which I *reduced a recent dislocation* of the hip into the sciatic notch by means of the

American method of manipulation. I have since met with the two following cases:—

1. *Dislocation on to dorsum ilii of seventeen days' standing. Reduction at first attempt.*—William Lynn, æt. 16, miner, was driving a horse harnessed to some "hutches," in a coal pit, on the 5th September, 1867. The horse ran off, and he, holding on to the reins, was drawn along by it. Being at last brought on to his knees, the hutches came up behind and struck him on the lower part of the back. When he rose he was unable to stand, experienced pain in the left hip-joint, and his "legs clung together the same as if they had been tied with a rope." About two hours after the accident he was seen by a medical man, who recognized a dislocation, and, having produced extension by pulling on the leg, while he kept one of his feet pressed against the perineum, he stated, according to the patient's account, that he believed he had succeeded in effecting reduction; explaining, that the shortening and inversion, which continued to be a source of alarm to the patient and his friends, would in all probability pass off. Thirteen or fourteen days after the accident, the patient still suffering great pain, the doctor returned with a friend. Having administered chloroform, and got the assistance of "two labourers who were working on the road," they produced violent and long-continued extension on the limb, while steady counter-extension was maintained on the axillæ. They left after many attempts, and without succeeding in their object. After a day or two of great pain, he was admitted into the Royal Infirmary. I saw him after the house-surgeon in charge had made an unsuccessful attempt at reduction under chloroform. The head of the femur was on the dorsum ilii, and all the symptoms occurring under such circumstances were clearly marked. Chloroform having been fully administered, I flexed the leg on the thigh, and the thigh on the abdomen, abducted the limb fully, and rotated outwards. During this last movement, I felt the tearing through of adhesions, and the head of the bone at once returned to the acetabulum. On extending the limb, it was seen to be in good position. The patient made a good recovery and went to the Convalescent Home three weeks afterwards.

2. *Case of recent dislocation of the hip into the thyroid foramen. Reduction.*—Thomas Yuill, æt. 42, quarryman, was admitted on the 25th April last into a ward of the Royal Infirmary, of which I was in temporary charge. He had been struck a short time previously by the "jib of a crane," but could not give a very accurate account of the accident. He complained of great pain in his right hip and thigh, and a very slight examination served to show that the head of the femur was in the obturator foramen. There was slight lengthening and marked abduction of the limb; while the knee was slightly flexed, and the prominence of the trochanter was conspicuous by its absence. There was a distinct fulness in the obturator region, although, as the patient was a very muscular subject, the head of the bone could not be defined by the hand. Reduction was effected under chloroform in the following manner:—I flexed the limb as thoroughly as possible, slightly abducted, and then rotated inwards firmly, at the same time adducting the limb and bringing it towards the floor. This resulted in transferring the head of the bone from the thyroid foramen—not to the acetabulum, but to the sacro-sciatic notch. Flexion, abduction and rotation outwards (which so readily transfers the head of the bone usually to the acetabulum in the case of dislocations upwards and backwards), sent it back to the thyroid foramen. A careful repetition of the first set of movements put it again in the sciatic notch; and again as before, in trying to reduce it from that position, I sent it back to the thyroid foramen. Having again removed it to the sciatic notch, I put my foot in the perineum, and made extension aid my movements, when it slipped at once into the acetabulum. The time occupied was very short, for the head of the bone was changed from the one position to the other with provoking facility. The patient made a good recovery.

The first of these cases illustrates the ease with which a dislocation of the hip may be reduced when extension, fairly and intelligently tried both with and without chloroform, has failed to attain the desired result.

The second case is interesting, chiefly as pointing out a mishap which seems apt to occur in manipulating the thyroid dislocation; namely, the removal of the head of the bone *not into the acetabulum*, but upwards and backwards into the *great ischiatic notch or on to the dorsum ilii*. In a paper by Dr. Thos. M. Markoe on "Reduction of dislocated femur by manipulation," in the New York Journal of Medicine for January 1855, this is well shown in several of the cases related. In Case 4, the original dislocation was into the sciatic notch, and Dr. Markoe says: "The effect of the first attempt was to throw the head on to the obturator foramen, making the limb longer than the other, and producing the deformity characteristic of that dislocation. From this point, by a slight alteration of the movement, the head could be made to slip back to its original position. Between these two points it could be made to play backwards and forwards, but would not enter its socket." Much the same thing is described in several other cases. In all the cases of thyroid dislocation, it would seem that extreme flexion was employed and rotation was found to succeed, whether made outwards or inwards. It seems as if too great flexion is the cause of the bone slipping from the one position to the other without entering the acetabulum. At all events, this is the opinion expressed by Bigelow and Hamilton, and in the case related above I flexed the limb to the fullest possible extent.

Bigelow writes in regard to Dr. Markoe's cases of thyroid dislocation: "These cases are instructive as showing that the head of the bone is directed towards the socket when the Y ligament is wound upon the shaft by rotation, *whether inward or outward*, and they correspond to the results of my own experience, made before I had read them. . . . . It may be superfluous to add, that in Markoe's case, inward rotation would probably have reduced the bone, had the thigh been less flexed." Hamilton says: "In one example mentioned by Markoe, it is pretty evident that the head of the femur was thrown into the ischiatic notch, by having flexed the thigh too much, so that 'the knee touched the thorax.' Indeed, it is questionable whether it will be best ever to bring the thigh much, if at all, above a right angle with the body, since any further flexion can only throw the head below the acetabulum, when, in fact, it is already too low." In backward dislocations, on the other hand, the flexion should be firm and complete.—*Glasgow Medical Journal, August* 1873, *p.* 444.

49.—ON PROFESSOR ESMARCH'S MODE OF PERFORMING BLOODLESS OPERATIONS.

By Wm. Maccormac, Esq., Surgeon to St. Thomas's Hospital.

The importance of the simple and efficient method briefly described by Professor Esmarch at the second Surgical Congress in Berlin, for preventing loss of blood during operations on the limbs, appears to me so great, that I take the earliest oportunity of communicating my experience on the subject, brief though it be.

A little girl, five years old, struck the left tibia, twelve months ago, against a stone; necrosis followed, and when admitted to Hospital a year afterwards a sequestrum could be felt in the tibia enclosed by a considerable thickness of new bone. Whilst the patient was being chloroformed, I applied, pretty tightly, an ordinary elastic bandage from the toes to the middle of the thigh. The bandage was two inches wide and five yards in length, and, thus applied, the bandage forced all, or nearly all, the blood from the limb into the body. When the patient was fully narcotised, I wound a half-inch indiarubber rope around the thigh immediately at the upper border of the bandage, and sufficiently tightly to obstruct all the afferent vessels. Hooks previously attached to the extremities of the rope furnished a ready means of fastening it, as well as of removing it at pleasure. The bandage first applied was now unrolled, when the limb presented a blanched appearance. The operation was then commenced; some new bone removed, so as to get at and take away a considerable-sized sequestrum. During the entire time not a single drop of blood appeared in the wound; a sponge was not once required, and the facility with which the operation was conducted and finished requires to be seen to be realised. The tissues were divided, so far as bleeding was concerned, just as they might have been on the dead body. This operation was performed in St. Thomas's Hospital on August 16 in the present year, Esmarch's method for producing local anæmia being then practised, I believe, for the first time in Britain. Since the operation the little patient has progressed very favourably, and, although carefully watched, no peculiarity which might be attributed to the use of the apparatus has been observed either in the wound or in the limb.

Since then other operations for necrosis have been performed, and an excision of the knee lasting thirty-five minutes, also an amputation of the thigh, and in no instance has one single drop of blood been lost. The advantages of such a plan are so palpable as not to need much insisting upon. The generality of hospital patients can ill spare a serious loss of blood, and such a loss often proves inevitable during operations for extensive necrosis of bone. In amputations the greater part of the blood of the lost extremity is preserved, to the advantage of the patient. The duration of operations will be be much shortened, as there is neither blood nor the constant dabbing of sponges into the wound to remove it, to interfere with the surgeon's sight. No accident or ill consequence at all appears to follow the use of the apparatus. In cases where amputations require to be performed for gangrene, or where there is a deposit of septic material in the limb about to be operated upon, there might be a risk of the elastic bandage forcing some portion of the septic material into the circulation. In the further use of the apparatus this possibility must be kept in view. Anyone will be surprised, in trying it upon his own arm, to find what a small amount of pressure of the india-rubber rope will stop the pulsation of the radial artery, and the femoral can also be stopped with no great exercise of force. Doubtless the history of surgery abounds with many attempts to empty limbs of blood previous to amputation, and to arrest hemorrhage during their performance. Stromeyer, in 1853, as he relates in his "Maxims," adopted a plan precisely similar in principle, in an operation on a brachial aneurism. He bandaged the limb to a point just above the aneurism, and then applied a tourniquet. The loss of blood was very small during the operation. Billroth mentions that when he was assistant to Von Langenbeck in 1853 and 1854 a somewhat similar plan was tried in the clinique in Berlin. Vanzetti, of Padua, relates in the Italian Medical Gazette that Dr. Silvestri, in Vicenza, has employed bandaging and the india-rubber rope compression above it in amputations; but, this notwithstanding, to Professor Esmarch must be attributed the credit of devising and making known a most simple, practicable, and efficient plan for wholly preventing loss of blood during operations, of whatever kind, when performed upon the extremities of the body. —*Medical Times and Gazette, Sept.* 20, 1873, *p.* 320.

50.—MODIFICATION OF ESMARCH'S ELASTIC BANDAGE.

By W. H. Cripps, Esq., House-Surgeon to St. Bartholomew's Hospital.

Esmarch's admirable suggestion of using an elastic bandage to exclude the blood before operating on limbs, and the complete success attending it, are now probably well known. The following is a simple modification of his arrangement, by which many yards of elastic bandage may be dispensed with, and it can be easily and quickly applied.

A short india-rubber tube is used, not only to prevent the blood from returning to the limb, but also for the purpose of removing it in the first place. The two ends of an india-rubber tube, twenty-one inches in length and about three-eighths of an inch thick, are bound together with a piece of twine, the whole forming an elastic ring seven inches in diameter. A grooved wheel revolving between a double handle completes the necessary apparatus.

To apply this to the arm, three or four complete turns of the elastic ring are wound tightly round the hand in such a manner as to include the fingers and thumb, care being taken that the turns lie even and do not cross one another. *The reel is then put under* the free portion of the ring connecting the *upper and lower coil.* The reel is passed round and round the limb in an upward direction: thus each coil is unwound from below as another is added above. In this way four tight coils of india-rubber are carried up the limb to any distance required. The degree of tightness can be regulated with the greatest nicety by the distance the reel is drawn from the limb by the bandager.

This method of driving blood from the limb answers perfectly in the arm and in the lower part of the leg; but in carrying the bandage over the popliteal space the flexor tendons prevent the artery being effectually compressed. A firm pad in the space would probably answer the purpose.

To remove the bandage, it may either be unrolled by reversing the action of the reel, or the twine connecting its two ends may be cut with scissors.

A very neat little reel in a holder may be obtained at Ferguson's, in Giltspur Street.—*Lancet, Oct.* 11, 1873, *p.* 520.

51.—ON THE ANTISEPTIC SYSTEM AS SEEN IN PROFESSOR LISTER'S WARDS AT EDINBURGH.

By Rickman J. Godlee, Esq., Surgical Registrar at University College Hospital.

Although the vital importance in surgery of the antiseptic system is now very generally recognised, it is difficult for those who have had little experience of the advantages to be derived from the treatment to form a just appreciation of its merits, even though fully convinced of the truth of the great principles upon which it is based. To such, therefore, an account of the comparatively recent improvements in practical details may not be without interest, illustrated by notes of a few cases, showing the character of the results which are now obtained in Mr. Lister's wards at the Royal Infirmary of Edinburgh; and as I have during three months of last session enjoyed unusual opportunities for observing for myself the efficiency of the treatment in his hands, I have asked and obtained his consent to the publication of the following facts, some of which have not hitherto been laid before the profession.

Mr. Lister principally relies at the present time on three antiseptic agents: Carbolic acid, boracic acid, and chloride of zinc, each of which is possessed of peculiar properties, which render their employment advisable under different circumstances. The first differs from the other two in being volatile, and is therefore employed by preference in every case in which a cavity exists, into which regurgitation of air is liable to occur, both during and after the changing of the dressing; any such air is deprived of its septic influence by the vapour of carbolic acid which exists between the meshes of the gauze or amongst the drops of the spray; but it would be quite unaltered after passing through a substance containing a non-volatile antiseptic salt, such as boracic acid, the action of the latter being confined to the objects with which it comes into actual contact. The proportion of the ingredients with which the gauze is saturated is as follows: by weight—carbolic acid, 1; resin, 5; paraffin 7. The resin is employed on account of the tenacity with which it holds the carbolic acid, the paraffin (which gives it up with great readiness) being added to give the mixture a suitable consistence. In making a dressing a sufficiently large piece is folded in eight layers to overlap the wound in all directions, to a greater or less extent, in proportion to the amount of discharge that is anticipated. Between the two outer layers a piece of thin mackintosh cloth (stained red for distinction) is interposed, by which the discharge is made to traverse the whole of the seven inner layers before it becomes exposed to the causes of putrefaction by reaching the surface. Without such an arrangement the continual flow of fluid would soon exhaust the carbolic acid from the part of the gauze immediately over the wound, and this would then cease to form a barrier to the spread of decomposition inwards.

For use as a lotion, carbolic acid is dissolved in water in the proportions of 1 part to 20, or 1 to 40; the weaker solutions being now abandoned, as they did not appear to be perfectly trustworthy. The 1 to 20 lotion is employed for destroying already existing organisms, as,—*e. g.*, in the injection of a recent compound fracture, for purifying the epidermis of a part on which operation is to be performed, or for the cleansing of an ulcer on which putrefaction has been allowed to take place. The solution of 1 in 40 of water is used for the spray, and during the changing of an ordinary dressing, and for most purposes it will be found a lotion of very convenient strength. Before an operation, some of the instruments, such as the saw and cutting pliers, are purified by smearing them with a solution of one part of the acid in ten of olive or other sweet oil, and the same is used as a temporary dressing after operation on some putrid cases to be afterwards described (*vide* the case of W. McK.); but for cleansing knives, and most other instruments, for which the oil has no special virtues, either in facilitating their working or in penetrating intricacies of their surface, dipping in 1 to 40 watery solution is quite sufficient. It is of great importance that catheters, &c., should be freed from septic germs before use, as there can be no doubt that in very many cases decomposition of urine has followed the introduction of instruments into the bladder. For this purpose a 1 to 50 of oil is found to be sufficiently powerful, and it does not, as do stronger solutions, prove in the least irritating to the urethra.

The antiseptic qualities of boracic acid were discovered a short time ago in Sweden; but though used for preserving articles of food, it had not proved of any value as a surgical application until its slight solubility in water was ingeniously turned to account by Mr. Lister, for the purpose of storing up a supply of the acid in the so-called boric lint. As boracic acid is much more soluble in hot than in cold water, the effect of soaking lint in a saturated boiling solution is a copious deposit of the crystals throughout its substance on cooling and drying; and as these will be but slowly acted upon by the discharges from the wound, which are at or below the temperature of the body, the lint remains for a considerable time efficacious as an antiseptic application. A cold saturated solution of the acid is also employed as a lotion, with the assistance of which a moist dressing may, if required, be made by wetting with it the boric lint, over which is placed a piece of ordinary oiled silk or gutta-percha tissue. But, as a general rule, in cases free from putrefaction it is used as a dry dressing, overlapping a piece of the protective moistened with the lotion, and it is often found very convenient to fix the lint by means of collodion, its edges having been previously frayed out with a pin for the purpose. The special advantage of boracic acid is its extreme blandness; it is in substance almost tasteless, and as the crystals themselves are very soft and almost greasy, resembling closely in physical properties those of the crystallisable fats, and it acts but very slightly either as a mechanical or chemical irritant; but its non-volatility, for the reasons before mentioned, limits its application to superficial sores, in the treatment of which it will be found invaluable.

Chloride of zinc, though long known as a surgical remedy, owes its reputation largely to its employment by Mr. Campbell de Morgan, of the Middlesex Hospital. It is remarkable, as well for the potency of its action as for the duration of its effects; thus, besides being a most efficient corrector of putrefaction, it has the advantage of preserving or pickling the part to which it is applied for about three days. But as, like boracic acid, it is non-volatile, and is moreover a powerful caustic to the tissues, its employment is confined in Mr. Lister's practice to a limited number of cases. He makes use of a solution of 40 grains to the ounce of water for application to the surfaces of the wound, after operating on a part where decomposition is already present, or on one which communicates directly with one of the natural cavities of the body (as, for example, after removing piles or cutting a fistula): and in this way, even if putrefaction be not completely eradicated, its noxious products cannot influence the general cavity of the wound during the three days prior to granulation, when the tissues are specially prone to irritation and absorption. For the method of employment in the former class of cases, I may refer to that of J. W. given below, and to that of W. McK., who, besides other injuries had sustained a severe compound fracture of the ring-finger of the right hand, which was not submitted to Mr. Lister's inspection till several days after the accident, when it was found that the wound had putrefied and was much inflamed, having assumed a sloughy appearance, while the proximal phalanx was necrosed. Mr. Lister amputated the finger, removing the necrosed phalanx with dressing forceps, and fashioning two lateral flaps from the inflamed tissues. After removing, as far as possible, the adherent sloughs, the raw surfaces were freely treated with the solution of chloride of zinc, and a piece of lint soaked in carbolised oil (1 to 10) was inserted between the lips of the wound, which were not brought together at all, and the whole hand was loosely enveloped in the antiseptic gauze. The oiled lint was changed by the nurse every three hours during the first day, and afterwards the little and middle fingers were bandaged together and the ordinary gauze dressing applied, the case being from this time treated as though it had been "aseptic" from the first. The wound rapidly assumed a healthy appearance; while the discharge, which had previously been copiously and foully offensive, became serous, and rapidly diminished in amount, remaining free from putrefactive odour to the last. Very soon after the operation symptoms of tetanus appeared, which eventually proved fatal to the patient; but it must be added that this is only the second case of tetanus that has occurred in Mr. Lister's practice during the last six years, and as in both of these decomposition had been allowed to take place, it is only fair to infer that in them, at least, it had something to do with the disease. It is also worthy of remark that in Mr. Lister's wards all arrangements as regards bedding, utensils for urine, &c., are so planned as to avoid, as much as possible, the vitiation of the atmosphere by putrid emanations, and this circumstance no doubt aids the antiseptic management of the cases in bringing about the perfect immunity from pyæmia and hospital gangrene, which, considering how crowded the beds are, constitutes one of the most remarkable as well as beneficent features of the treatment.

After operations about the face or mouth, the action of chloride of zinc is found to be exceedingly beneficial. Thus, in the case of S. F., half of whose lower jaw was removed by Mr. Lister, on January 15th, 1873, on account of a very large and rapidly-growing sarcoma, the chloride was applied freely to the surface of the wound, while over the incision in the skin a boric dressing was placed, under which union by the first intention went on without interruption. There was not the slightest offensive smell about the patient's breath on the second and third days after the operation, and he was thus saved from the irritation, to say nothing of the risk which is so often connected with this period in such cases.

All those who have applied a lotion containing carbolic acid to a raw surface must have noticed, as a result, that the serous discharge of the first few hours is much greater in amount than that which occurs in similar cases where simple water is used; and this evil—for an evil it is—is probably a necessary one in connexion with the antiseptic treatment, as it can hardly be supposed that an agent powerful enough to destroy the living causes of putrefaction should be absolutely unirritating to the higher animal tissues. It involves, however, the necessity for providing a free drainage at first; but as, if decomposition do not occur, no subsequent inflammation is anticipated, and as in most cases early union of the cutaneous surfaces is of very great importance, the simple method of attaining this result by leaving a wide interval between the stitches is seldom advisable. Accordingly, Mr. Lister makes use in almost all his cases of the drainage-tube, his modification of which is as follows:—A piece of india-rubber tubing, provided with suitable holes, is cut obliquely at the inner end, so that it may not be closed by pressing against the flat surface of the interior of the wound, while through opposite sides of the tubes, close to its outer extremity, two pieces of silk are passed with a fine needle, and the ends of each are cut at a distance of about two inches, and fastened together by a knot. It should be of such a length that, when inserted as far as possible into the wound, its outer end, previously cut obliquely or transversely, according to the direction of the sinus, may be exactly level with the surface of the skin, a position which the pieces of silk will be found to maintain. With such an arrangement the discharge is shed with great perfection, and the surgeon may place the stitches as close together as he pleases.

Another way of accomplishing the same thing is by means of the sponge, the use of which is indicated in those cases where a cavity remains, the walls of which it is desired to keep in apposition by the uniform pressure which this method provides. The sponge, previously soaked in a strong watery solution of carbolic acid, and thoroughly squeezed, is applied over the wound between the protective and the folded gauze, the whole dressing being firmly secured by a bandage. Unless combined with a drainage tube, a pretty wide interval must be left between the stitches. As an example of its employment, I may refer to the case of E. F., a young lady four years of age, from whom Mr. Lister removed an enchondroma of the size of a hazel-nut, growing from the posterior border of the scapula. At the operation, which took place on March 5th, 1873, the arm was held well back by an assistant, so as to render the tumour prominent under the skin, and an incision 1½ inch to 2 inches long, was made down on to this parallel to the edge of the bone; while another, 1 inch long, was carried outwards at a right angle, from the centre of the first. After cutting through the superficial fat and the overlying muscle, an oblique notch was made in the scapula on each side of the tumour, and the piece enclosed between the two was then removed with cutting pliers. When the arm resumed its natural position, the edge of the bone was at some distance from the wound. No stitches were employed; but protective was placed on the incision, and over this the sponge, and then the gauze dressing, and the whole was well secured by a bandage. The sponge was removed on March 6th; and neither then, nor at any subsequent period, could anything be pressed out of the wound, nor was a trace of pus seen from first to last. On March 21st, a sound cicatrix, evidently some days old, was disclosed on finally removing the dressing.—*Lancet, May* 17, 1873, *p.* 694.

52.—THE APPLICATION OF EARTH IN SURGERY.

By Dr. Joseph Groves, B.A., Venice.

It was my good fortune during the winter to be residing in the same hotels, both at Nice and Rome, with the eminent American surgeon, Dr. Hewson, of Philadelphia, who brought to my notice the advantages attending the use of earth as a dressing in surgical cases. He told me he attributed his great success at the Pennsylvanian General Hospital—a success which has been remarkable since the American war, especially in connexion with cases of amputation—entirely to the use of this agent. The results of its use which have come more or less directly under my own observation have been so successful that I venture to send you brief outlines of the cases.

1. A case of epithelioma of the cheek, of twelve years' standing. In December there was an ulcer on the right cheek larger than a half-crown piece, which was being treated with dry-earth applications. In March I saw the case dressed in Rome, as did also Dr. Pantaleoni and other physicians. There were then, instead of the extensive ulcer of three months before, superficial points of ulceration which, had they been collected together, a fourpenny-piece would have covered. The patient, a gentleman of sixty-two years of age, was not very sanguine, having had the unfavourable opinions of several distinguished surgeons, but he expressed himself most grateful for the relief from pain and from the horribly fetid odour, which made life a burden to him before the earth was applied.

2. A physician at Mentone, under Dr. Hughes Bennett's care, I believe, sent for Dr. Hewson from Nice to resect his knee-joint, or to amputate his leg. Dr. Hewson found an abscess, thought to be metastatic, in the head of the fibula. He begged to be allowed to try earth dressing before proceeding to an operation. The case did perfectly well, and the joint is apparently as good as ever.

3. The American chaplain in Rome was thrown from his horse, and struck his leg against a rail. The trowsers were not cut, but the tibia, the spine of which was laid bare for several inches, sustained a comminuted fracture, and the *tibialis anticus* was partly stripped from its attachment. Dr. Hewson put

in two or three stitches, and applied earth dressing. The patient had scarcely any pain; there was but little discharge, and the leg rapidly healed. The almost total absence of pain was very remarkable.

4. At Naples a poor woman accosted me in the street, and asked me to cure a large chronic ulcer on the right leg, which extended from the external malleolus to midway between the ankle and knee. I offered her a few soldi, and told her to go to the hospital. She said she was not begging for money, but to be cured, and as I was an English doctor, I could cure her if I would. Her grandfather had been in the service of an English nobleman, it seemed. I managed to find some light-yellow clay, allowed it to dry, pounded it and sifted it through fine muslin, and applied it to the sore, covering it with ordinary grocer's blue paper, the edges of which I wetted in order to make it adhere to the skin. I told the poor creature to remain at home and rest the ulcerated leg on a chair even when she sat spinning at her door. The next morning I found her waiting for me at the gate of the hotel. She said she was quite free from pain; she had slept all night, a thing she had not done for months before; and with tears in her eyes she thanked me for the relief I had given her. In three weeks the ulcer was almost well. It is right to say I took means to improve the general health.

5. An American gentleman in Rome asked me to prescribe for him, as he had gonorrhœa. The gonorrhœa was very acute, with chordee and severe pain during micturition. As he bound me to secrecy, I could not borrow a small double-tubed catheter which Dr. Hewson uses in these cases, and was obliged to inject a large quantity of muddy water with an ordinary syringe. The patient was greatly relieved by the first application, which was repeated on the two succeeding days, though the discharge and pain had ceased on the second day. The patient had a dose of chloral and an aperient each night.

6. A young Roman, a friend of the previous patient, came to me complaining of rheumatism. He had had a gleet eight months. I injected the urethra with muddy water every other morning for ten days, when the discharge ceased. The rheumatic pains ceased also, supporting, therefore, my friend Dr. Bond's theory of the cause of gonorrhœal rheumatism. This patient took iron and arsenic in small doses, as he was somewhat anæmic.

The freedom from pain, the destruction of all odour from the wound, the diminution of the discharges, and the rapid healing after the application of the earth, were the features common to all these cases which impressed me most.

Dr. Hewson, who will shortly visit England, believes the action of the earth mainly a chemical one, and he covers his dressing with blue paper in order that the chemical rays of light may be admitted to the wound.—*Lancet, June* 21, 1873, *p.* 894.

53.—ON THE USE OF RESIN-CLOTH, IN THE PLACE OF CERE-CLOTH, IN THE TREATMENT OF WOUNDS.

By Edward Lund, Esq., Surgeon to the Manchester Royal Infirmary.

To those who practise the system of dressing wounds advocated by Professor Lister, the use of the carbolised gauze which he employs, as far merely as expense is concerned, is a matter for consideration. This perhaps is the case more in hospital than in private practice, because in the former, at the present time, the gauze is largely used to test the value of the antiseptic method, and economy should be observed as far as possible. Moreover, beyond the cost, there is a difficulty in the preparation of the gauze, for the mixture of paraffin and acid with which the fabric is saturated must be applied to it in a heated liquid state, and, unless a certain temperature be maintained during the whole process, the gauze will be of unequal thickness and imperfectly porous. I have tried to make the gauze myself according to Professor Lister's recipe, but I have often failed to do so in these particulars. In the paper in which he first described this dressing (in the British Medical Journal of January 14, 1871, p. 30), Professor Lister refers to a very curious property possessed by carbolic acid—namely, that it can be combined with resin, and resinous matters generally, with great facility, and when so blended nearly all its irritating acrid properties are neutralised, while the compound so formed retains the power of evolving an antiseptic vapour at the temperature of the body. It is well known that to touch the mucous membrane or even the skin of the lips with a fragment of pure carbolic acid occasions pain and excoriation, in some subjects more than in others. But when we experiment in the same way on a mixture of the acid with five parts of resin, we find all the acridity is destroyed; the acid is still present, but it is stored up and rendered harmless by this new combination.

Having verified this fact by numerous observations, I came to the conclusion that we have here a method of forming easily, without the aid of heat, an application for antiseptic purposes, in surgical practice, of great value. All that is required to carry out the idea is to saturate very thin calico gauze with such a mixture of resin and acid dissolved in methylated spirit, press it powerfully, spread it out to dry quickly so that it may become quite porous and absorbent, and it is ready for use. Yet the resin and acid thus left upon the threads of the calico after the complete evaporation of the spirit would be found too brittle and adhesive to the skin for a wound-cover and air-filter if something else were not added to impart flexibility. The substance which I employ for this purpose is castor oil, because it is the only fixed oil easily accessible which is quite miscible with spirits of wine. All samples of castor oil, by reason of adulteration, do not show this property of perfect solubility in alcohol, as stated by Pereira ("Materia Medica and Therapeutics," 1865, p. 251), and therefore I am content to use the oil if it will unite perfectly with twice its bulk of rectified spirit. The exact composition of the solution is as follows:—Carbolic acid crystals, melted, two fluid ounces; castor oil, two fluid ounces; purified resin, sixteen ounces by weight; methylated spirit, forty fluid ounces,—mix.

To dissolve these ingredients easily we must add them together in a certain order. To the resin, liquefied by heat and removed from the fire, add gradually one third part of the spirit; when these are well mixed, put in another third of the spirit, in which the oil has been previously dissolved; and lastly, the acid in the remaining portion of the spirit must be slowly added to complete the mixture. The whole must be agitated until all the constituents are thoroughly incorporated, and afterwards passed through a muslin filter to get rid of any extraneous matters. Unless this plan be adopted, the resin will concrete into a mass at the bottom of the vessel, and it will be extremely difficult afterwards to get it perfectly mixed. When thus prepared, the solution is of a dark colour, clean and free from any deposit, and can be kept unchanged in a well-corked bottle for a long time. It is, in fact, a sort of thick varnish. To make the resin-cloth—as I term it, for the sake of distinguishing it from the cere-cloth dressing for wounds which I described in a paper read at the Leeds meeting of the British Medical Association in 1869,—it is needful to select a very thin, cheap, porous calico, or calico muslin, known in the trade as "mull," which costs at wholesale price about 4s. per piece of twenty yards. This divided into strips, each about nine inches wide and six yards long, is reduced to a convenient form for general use. The calico should be unbleached and free from stiffening; and each of the strips should be carefully folded up, so as just to lie flatly in the press, as I am about to explain. An ordinary square tincture-press may be used to press the cloth, or such a press as is sold for copying letters, to which a tin box has been adapted, so that the plates of the press can work in it; and in this box the folded calico is placed, the solution being poured over each successive layer so as to wet perfectly every part of it. There should be an aperture at the bottom of the box, with a tap by which the superfluous fluid can be removed, collected, and used a second time. The press being brought into action, the pile of calico should be squeezed as dry as possible, all the fluid drained off, the resin-cloth taken out, laid over a few lines of string in a warm room with good ventilation, and, in an *hour or two, when all traces of* smell of the methylated spirit have been dissipated, the cloth may be rolled up and kept in tin cases ready for use.

It is difficult for me to state the exact cost of resin-cloth made by this process, for I have not yet bought the materials for its preparation at such wholesale prices as could be paid if it were to be manufactured in large quantities; but, allowing for some slight reduction where six or eight twenty-yard pieces of calico are bought at a time, and the solution made by the gallon, I find it comes to a fraction less than 3d. per yard of average width of forty-four inches. In using it as a dressing for wounds, I deal with it precisely as I would do with Professor Lister's antiseptic gauze, for which it must be taken as a cheap and ready substitute. I generally apply ten folds of it over the face of a wound (as in an amputation), and perhaps six folds higher up the limb for some distance, and I cover it with the mackintosh hat-lining, so as to distribute the serous discharges through the breadth of the resin-cloth thus covered. I have never found it to irritate the skin in any degree beyond what the oil-silk protective, liberally used, could control, except once, when, in the hurry of preparing the resin-cloth, I had neglected to dry it thoroughly, and it was applied still moist with the methylated spirit; the naphtha in it seeming to be the chief cause of the skin irritation. But if this precaution be observed, I believe this resin-cloth will be found a very useful addition to our means of treating wounds and abscesses on antiseptic principles.—*Medical Times and Gazette, July* 19, 1873, *p.* 62.

ORGANS OF CIRCULATION.

54.—ON ANEURISM OF THE CAROTID ARTERY.

By Timothy Holmes, Esq., M.A., Professor of Surgery and Pathology to the Royal College of Surgeons.

[There are three modes of treating carotid aneurism, each suitable to different cases; they are,—proximal pressure, distal pressure, and the Hunterian operation of ligature of the artery. The pressure may be digital or instrumental.]

The more of healthy artery there is accessible below the tumour, the smaller the latter is, the slower its growth is, and the more clot it contains, obviously the better adapted must it be for the compression treatment. But the histories of the cases hitherto known will prove that this treatment is not limited in its application only to these simpler forms of the disease. This proposition is proved by the cases recorded of successful pressure under the care of Sheppard, of Worcester, and Kerr, of Canton, as well as those of Rouge and Humphry, referred to in the last lecture.

Another case, which excited much interest here at the time of its occurrence, is the following, for the notes of which I am indebted to Mr. Gay, who was originally consulted in it, and to Mr. Coles, who superintended the application of the pressure. The patient was a city merchant, who had been in a railway carriage which ran up an embankment. He and two other occupants of the carriage scrambled very hastily out of the window, head foremost. He did not perceive that he had sustained any injury beyond a shaking. About a fortnight afterwards, while shaving, he perceived a pulsating tumour at the root of the neck on the left side. It was about as large as a walnut, and was accompanied with aneurismal pulsation and an unusual bruit described as being of a whistling kind. It was situated on the carotid artery, a little above the bifurcation of the innominate; but there was room to push the thumb or the pad of an instrument down behind the sterno-clavicular joint and stop the pulsation, nor did this give him any peculiar inconvenience. The diagnosis was made by Mr. Coulson and Mr. Gay, who first saw the case, and was subsequently confirmed by Mr. Buxton Shillitoe, Sir J. Paget, Sir W. Ferguson, and Mr. Prescott Hewett, who were called in consultation. There can therefore be no reasonable doubt of the aneurismal nature of the tumour. Pressure was decided on, and was at first applied by means of a frame, to which was attached an arm movable in every direction by rack and pinion joints, and carrying a small pad. This was tried for four days and nights; and the proper application of the pad stopped the pulsation; but it was thought that the instrument was liable to become displaced by the movements of the patient's body, and accordingly, at a consultation between Mr. Gay and Sir J. Paget, it was decided to resort to digital pressure. In this the services of my friend Mr. Coles were employed, and the treatment was commenced on June 4th, 1867, rather more than a month after the first discovery of the aneurism. Relays of students were obtained to keep up pressure night and day; the patient was placed on an Earle's bedstead, and digital pressure was commenced about 6 p.m. He bore it with much fortitude, and it was kept up pretty continuously till the evening of June 7th—*i.e.*, for four days and three nights—the patient sleeping at intervals. The excoriation of the skin and its extreme tenderness now compelled the discontinuance of the pressure. It was resumed at 10 a.m. on June 10th, but after six hours the tenderness of the skin again compelled its discontinuance. In the interval the acetate of lead had been given in two-grain doses three times a day. The effect of the pressure had been to abolish the bruit in the tumour; but this returned on June 14th. The skin, however, being still too tender to bear pressure, the acetate of lead was continued until June 24th, when digital pressure was resumed. In the interval a consultation had been held, at which the tumour had been somewhat roughly manipulated, but without any methodical attempt to effect the dislodgement of any clot, and with no symptoms of embolism following. On the day above named (June 24th) digital pressure was resumed, as before, at 10 a.m., and was kept up till June 27th—*i.e.*, a little over three days, when it was discontinued finally. It was not, however, until a month from this time,—viz., July 27th,—that the pulsation in the sac disappeared, and the reduction in the size of the tumour enabled him to button his shirt collar. From this time the tumour gradually diminished, and in about two months had quite disappeared. He was examined by Mr. Gay last March, and was found in perfect health, with no trace of aneurism whatever, and the carotid conveyed blood and appeared to be of the same calibre as the artery of the opposite side.

Two views may, I think, be taken of this case,—namely, (1) that it was a small sacculated aneurism, and that the application of the pressure produced a certain amount of deposit of soft clot in the sac, which afterwards went on to complete obliteration under the influence of rest and appropriate regimen, aided, very possibly, by the administration of the acetate of lead; or (2) that the artery was merely dilated, as we so often see in the right carotid near its root, and that the pressure, by temporarily relieving the dilated coats of the artery from the force of the circulation, enabled them to contract again. It would be rash, and even absurd, to dogmatise about a case which one did not see; and the latter view appears probable to Mr. Gay, who was in attendance on it. But I may perhaps be allowed to observe that such aneurismal dilatations are not usually accompanied by anything like a distinct bruit, nor are they known to be susceptible of cure by any method; while the gradual disappearance of pulsation after a vigorous application of total compression, and the subsidence of the tumour in a longer or shorter space of time, is exactly what has been noticed in other cases of cure of aneurism by rapid pressure, as in Dr. Murray's and Mr. Durham's cases of abdominal aneurism. In the latter it was more than a month after the discontinuance of pressure before the pulsation finally ceased. In those cases there could be little doubt that the effect of pressure had been to allow the sac to contract on its contents, and to produce a deposit of soft clot in the sac, and that this gradually hardened and increased until it filled the sac, or that by some lucky accidental displacement of a portion of it, the mouth of the sac became obstructed, and the cure was thus produced. Whatever view we take of the case just related, it must possess very great value as showing the possibility of curing by milder means the very class of cases for which Wardrop performed and recommended Brasdor's operation. It would have been very interesting in this gentleman's case to have noted the effects of distal pressure, but, as far as I have been able to ascertain, this was not done.

I have met with no case on record, nor heard of any in private, where the rapid method of pressure has been used in carotid aneurism; yet I can easily imagine circumstances in which it might be advisable; where the patient cannot tolerate the pressure without chloroform, yet the aneurism is of no large size, and appears to contain some clot. The rapid cure of many cases of small aneurisms under pressure when clot has previously been deposited appears to prove that, in our Hunterian preparation (1685) a cure might have been attained in a very few hours.—*Lancet, June* 21, 1873, *p.* 867.

55.—ON ANEURISM OF THE VERTEBRAL ARTERY.

By Timothy Holmes, Esq., M.A., Professor of Surgery and Pathology to the Royal College of Surgeons.

The occurrence of spontaneous aneurism of the vertebral artery in the neck has not as yet been noticed, so far as I am aware; but traumatic aneurisms of this are not so very infrequent, and we possess already records of eleven cases in which the carotid artery has been tied—in four for wound and in seven for traumatic aneurism of the vertebral.

It is therefore worth while, in discussing the practical surgery of cervical aneurism, to see what were the leading features of these cases, and how such an error could in future be avoided. For there can be no question that the ligature of the carotid in a case of vertebral aneurism must be fraught with double mischief. In the first place, by throwing the strain of the anastomosing circulation on the vertebral, it tends to aggravate the disease it was meant to cure; and in the second, if the circulation in the wounded vertebral artery is interrupted before the operation (which to some extent it almost certainly must be), the stoppage of the supply from the carotid artery is rendered doubly perilous to the nutrition of the brain.

Of these cases, the one which is most fully reported, and which best illustrates the diagnosis and treatment of vertebral aneurism, is that recorded by Lücke in the eighth volume of Langenbeck's Archiv, in which a traumatic aneurism formed at the nape of the neck after a stab. As compression on the carotid checked the aneurismal pulsation, the common carotid was tied. This produced no effect. Repeated injection of perchloride of iron into the tumour produced suppuration and the rupture of the sac. The latter was then laid open, and the open mouth of the vertebral artery was plugged near the foramen magnum. All went on well as far as the aneurism was concerned. There was no more bleeding, but the patient died from hemiplegia and softening of the brain, the result apparently of the operation on the carotid.

In commenting on this interesting case, Lücke remarks on the error into which he was led by the fact that compressions on the carotid checked the aneurismal pulsation. He explains this by supposing that the compression of the carotid artery acted simultaneously on the vertebral, and points out that this may easily happen if the vertebral (as is occasionally the case) enters a foramen in a higher vertebra than usual. It is a little singular that he seems to have omitted to ascertain whether this was so in his patient when he had the opportunity at the post-mortem examination. But other writers have noticed the possibility of checking the pulsation of vertebral aneurism by pressure applied in the same way as for aneurism of the carotid, even in the normal relations of the vertebral.

On the subject of digital compression of the vertebral artery Chassaignac speaks as follows:—As the vertebral near its origin lies immediately behind *the common carotid*, between it and the spinal column, the simultaneous closure *of both arteries* is inevitable [*i.e.* when pressure is made on the carotid low down], and it is an error to think that very powerful compression is required for this purpose. In order to convince oneself of this it is only necessary to repeat the following experiment on the dead subject. After having removed the skull-cap and the contents of the cranium, taking care to cut the vertebral arteries below their junction in the basilar, tie the axillary arteries and the upper part of the abdominal aorta, then inject water through the arch of the aorta. The liquid will be shot out through the two internal carotids and the two vertebrals at the base of the skull. But as soon as even light pressure with the fingers is put upon the course of the common carotid in the space extending between two and three inches above the clavicle, between the trachea and the internal border of the sterno-mastoid, the jet will no longer flow from either the internal carotid or vertebral on the side compressed, but will recommence as soon as the compression is removed. If both sides are compressed at once, all the arteries cease to spirt. If pressure is made with the same force on the common carotid above the 'carotid tubercle' (*i.e.*, the anterior tubercle of the transverse process of the sixth cervical vertebra), the space below that tubercle being left free, the jet from the vertebral artery will continue, while that from the internal carotid wholly ceases." He proceeds to show that the tubercle in question is much higher than is generally supposed, being always from two to three inches above the upper border of the clavicle. The bearing of this fact on the diagnosis between carotid and vertebral aneurism is obvious. In a case like Lücke's or Kluyskeus', the surgeon, knowing now the true position of the tubercle on the transverse process of the sixth cervical vertebra, and knowing that pressure applied along the course of the carotid anywhere below this—*i.e.*, for two inches at least above the clavicle—will most probably stop the pulsation in the vertebral also, will not conclude that the aneurism affects one of the carotids, or some branch of the external carotid, until he has seen that pressure also stops the pulsation when applied at a higher level, or when applied to the carotid in such a way as to isolate it from other structures, as M. Rouge did. A traumatic aneurism whose pulsations are unaffected by compression so applied must be regarded and treated as vertebral.

There are several cases which are classed by surgical authors as "vertebral aneurisms," in which the fact has not been absolutely ascertained by post-mortem examination, but where the progress of the case left no doubt as to its nature. This was so in Kluyskeus' case, reported by Chassaignac from the Annals of the Medical Society of Ghent. A man had been stabbed an inch below the mastoid process. The wound healed, and a traumatic aneurism formed, which grew rapidly to the size of the fist. All symptoms ceased at once on compressing the carotid, but at what level was not noted. The result of this compression, however, left no doubt on the minds of the surgeons that the aneurism affected some artery of the carotid system, and, accordingly, the trunk of the common carotid was exposed and tied. The operator did not notice whether compression on the denuded artery would stop the pulsation; at least, there is no note to that effect. But he noticed with great surprise, after tying the ligature, that it produced no effect on the tumour. The man recovered from the operation, but the pulsation of the tumour continued as before, except that it diminished in force during a severe attack of dysentery which occurred to him in the hospital, and much reduced his strength. Finally, after he had left the hospital, and his general health had improved, the aneurism burst, and he died of the hemorrhage some four months after the wound.

In this case there was no post-mortem examination, but the symptoms are identical with some in which the vertebral has been found to be the artery injured, such as the following, which is also quoted by Chassaignac (Chiari's case).

A man had been stabbed beneath the left parotid; the wound healed, but a traumatic aneurism formed, and grew to the size of an egg in about two months. It was taken for an aneurism of one of the posterior branches of the external carotid, and the common trunk was tied. The pulsation continued. The patient died from the operation, the cause of death being apparently pyæmia. The autopsy proved that the tumour was situated on the vertebral artery, at the precise point where it makes its curve between the first two vertebræ.

Cattolica, of Naples, has related in an Italian journal (translated in the Gazette Médicale, 1836, page 425) two cases of aneurism of the vertebral artery—one treated by ligature of the carotid, and proving fatal by rupture of the aneurism. The precise level at which the artery was wounded is not stated, but it was high up in the neck. Compression of the carotid had not been tried. In the second case the wound was immediately below the angle of the jaw. The ligature of the carotid was commenced, but when the artery was exposed it was found that pinching it between the fingers did not stop the pulsation of the tumour. The operation was abandoned. Compression and styptics were applied to the tumour, but the skin covering it was sloughing, and it would soon have burst, when the patient died of gastric fever.

In commenting on these cases the French writer points out the propriety of commencing the treatment by direct compression, which having failed, he recommends the plan ultimately adopted by Lücke—viz., to lay the tumour open, and command the bleeding mouth by compression with charpie steeped in some astringent.

After the relation of his case Lücke also comments at considerable length on the treatment which should be pursued in similar circumstances, allowing that the diagnosis of vertebral aneurism has been established. The ligature of the vertebral artery at the root of the neck is a difficult and formidable operation, nor is there any probability that it would check the circulation through the upper portion of the vessel so near its inosculation with its fellow. He argues therefore, I think, conclusively against any such attempt, and still more is the attempt to tie the subclavian above the origin of the vertebræ to be reprobated. Yet I cannot say that the course which he followed—viz., the repeated injection of perchloride of iron—seems at all to be recommended. In order that this mode of cure should have any fair chance of success, it appears to me necessary that the surgeon should be able to command the affected artery both above and below the tumour. And if the injection does not succeed, or at any rate produce some marked effect, at the first or second sitting, I doubt whether it should be persevered in. To practise repeated injections into a tumour like this will almost certainly cause suppuration and rupture of the sac, as it did in this instance.

The only case of recovery ofter vertebral aneurism which I have found is reported by Möbus in the fourteenth volume of Graefe and Walther's Journal. It bears much resemblance to Lücke's case, being the result of a stab just below the ear. The tumour was expected every day for some time to burst. The surgeon, believing that some branch of the external carotid was wounded, exposed that artery in order to tie it, but found that pressure on the denuded artery had no effect on the pulsation, so abandoned the operation, and urged the laying open of the tumour. The patient would not consent, and ultimately the tumour ceased to beat and disappeared. No treatment was adopted except the application of cold. The cessation of pulsation was preceded by very severe pain.

Bearing this case in mind, I think we should hesitate before proceeding to any extreme measures in a case of traumatic aneurism of the vertebral artery. I do not see what operation can be practised on such a tumour with any hope of success, except that modification of the method of Antyllus proposed by Mr. Syme—viz., to make a small opening in the sac, through which the finger is introduced, plugging the hole and preventing hemorrhage, while the surgeon feels for the point at which the blood-stream enters the sac, and where the pressure of his finger checks the pulsation. Having this under command, the sac may be freely laid open, so as to expose the mouth of the vessel, and a ligature may possibly be cast upon it. Even if the vessel is divided too near the bone to be tied, it may be possible to plug it, as Lücke did. The account of his case and of its post-mortem examination, points very strongly to the conclusion that his patient might have recovered but for the ligature of the carotid, even in spite of the damage done by the injection of the perchloride of iron; while abstinence from all active treatment might very possibly have led to the same fortunate result as in Möbus's patient.

The effect of compression applied over the lower part of the carotid ought to be carefuly noted in any case of supposed vertebral aneurism; and if this stops or greatly reduces the pulsation of the tumour, I see no reason why it should not produce a cure if the patient can tolerate it.

The conclusions, then, to which present experience points on the subject of vertebral aneurism may be thus summed up :—

1. A traumatic aneurism may be taken to be vertebral when it is situated in the course of that vessel, and when its pulsations are not commanded by compression of the lower part of the common carotid.

2. When a traumatic aneurism is situated as above, and its pulsations are commanded, however completely, by pressure on the common carotid low in the neck, it ought not to be treated as being carotid, or as affecting a branch of the carotid, unless it is clearly proved that its pulsations are stopped by pressure applied above the level at which the vertebral ceases to be compressible—*i.e.*, above Chassaignac's "carotid tubercle."

3. An aneurism diagnosed as vertebral may be treated by compression (gradual or rapid, as the case demands) of the root of the vertebral artery in the neck, if this is found feasible.

4. If indirect compression will not stop the pulsation, or if it cannot be borne, the tumour should be subjected to direct compression and refrigeration, to which internal remedies may be added; and possibly the subcutaneous injection of ergotine may be of use.

5. If these means fail, and the tumour appears likely to burst, or if it has burst, the sac should be opened with all due precaution, and an attempt made to tie or plug the wounded artery.

6. A wound known or suspected to be of the vertebral artery should be treated either by direct pressure or by ligature of the vessel in the wound.—*Lancet, July* 26, 1873, *p.* 105.

ALIMENTARY CANAL.

56.—SUGGESTIONS FOR A MORE SCIENTIFIC METHOD OF TREATING REDUCIBLE HERNIA.

By Carsten Holthouse, Esq., Surgeon to the Westminster Hospital.

To keep the pad in position, either a band or a steel spring is necessary; of the former I shall have to say something at a future time, at present I shall confine my attention to the latter.

For convenience of description, I shall call that part of the spring which is continuous with the pad or head of the truss, the neck; that which immediately succeeds the neck and grasps the haunch, the shoulder; next will follow the back of the truss; and lastly, the tail. The neck will include all that part of the spring between the pad and the anterior superior spine of the ilium; the shoulder, that between this process of bone and the sacro-iliac joint of the same side; the back will correspond with the spine, and extend from one sacro-iliac joint to the other; and the tail from the latter to the end of the spring. In the double truss there will, of course, be two necks and two shoulders, a back, and no tail.

There are four varieties of springs made use of by truss-makers: the German, French, English, and what I will venture to call the forceps spring; each differing in their curve or set, and somewhat in their mechanical action.

The German is characterised by the following peculiarities:—It is very strong, but slightly elastic, and its curve is as nearly as may be that of the outline of the part of the body which it encircles; thus it belongs to the class of what are called open springs, maintaining the same form when off as when on the body, and requires scarcely any unfolding when applied. By dint of great force it can be *partly uncurved or* made more open, or incurved or less *open; and in neither case is its elasticity* sufficient to restore it to quite the original curve. Its action, as exemplified by Egg's, Miller's, and Coles's trusses, is to support and resist, but to press only slightly, though, of course, it can be made to do so very powerfully by increasing its curve. Egg describes his truss in the following words:—"Supposing that the body from A to B is 5½ in., the truss is 5¼ in.; thus the pressure is but slight, and, when the truss is properly adjusted, scarcely perceptible." From the great strength of this spring, its resisting power must be considerable; but as its curve or set does not admit of a nearer approximation of the pad to the back of the spring than that just mentioned (the assumed antero-posterior diameter of the body being 5½ in.), it follows that, if the abdomen should recede to, say, 5 in., it would not only cease to exert any pressure, but would actually not be in contact with the abdominal wall. The difference, therefore, between ¼ in. and ½ in. is too little for security; and, to render this kind of truss efficient, the spring must be incurved to a greater extent than ¼ in., when, from its very slight elasticity, the pressure would be too great for comfort. Hence the German, *per se*, can rarely be used advantageously; combined, however, with other arrangements as carried out by Miller and Coles, it is, perhaps, the most useful of any. In the trusses of these makers, the spring is of the half circle type, and acts more as the carrier of the pad than its compressor. In Miller's this is especially the case, the spring being cylindrical and exceedingly strong; and from its being applied to the side of the body opposite to the hernia, it has great firmness and steadiness, and no tendency to slip off the haunch. Coles's is applied to the same side as that of the hernia, and is less strong and resisting, and would not keep in place without a connecting strap. This strap subserves the same purpose as that of the ordinary truss—viz., first to steady the instrument; and next, if buckled tightly, to increase the pressure of the pad by incurving the spring.

The French belong to the class of what are called close springs, being the most curved of all truss springs; it is thin, light, weak, and very elastic; it can be uncurved into a straight line, and will immediately afterwards resume its former shape; consequently, though it is always pressing, it is but slightly resisting; certainly it can be made stronger, but in that case the pressure becomes excessive. The difference as regards the action of a French and German spring may be summed up in a few words: in the former the pressure and the resistance are equal—in the latter the resistance exceeds the pressure; the first is always pressing inwards and resisting outwards in an equal degree—the second presses very slightly inwards, but resists strongly outwards. The advantages of the French spring are, that it is more clinging than other springs, and causes the pad to bite better, so as to do away with the necessity for a thigh-strap. More power also is obtained, in proportion to the weight of metal, than with other springs; because the more a spring is uncoiled the greater force it must exert. If, for example, when its ends are separated to five inches, it should exert a pressure equal to two pounds, it would, if uncoiled to seven inches, exert a pressure of three pounds. Now, the French being much more coiled than any other spring, it follows that to bring its pad to seven inches from its back it will have to be uncoiled to, say, nine inches; it will therefore exert a greater pressure than a stronger English or German spring having a set of five inches and six inches and a half respectively, for to bring the pad of these to seven inches from their back they will have to be uncurved only two inches and half an inch. Thus, weight for weight and distance for distance, the French exerts the strongest pressure; owing, however, to its great curve, part of the weight may be dispensed with, and so greater lightness obtained with equal strength. This spring is less adapted for bad ruptures than either the English or German, but for slight cases it is sometimes preferred by patients to the other springs, and for a night truss it is the best of any.

The English spring is intermediate in curve between the two former, and is the one most generally used in this country. In the trusses of the City of London Truss Society the spring resembles the German more than the French, but I find its curve varies considerably in trusses made by different manufacturers, some approaching more nearly to the German type, and others to the French. Its action likewise partakes of the characteristics of both these springs, being more elastic than the former, and less so than the latter.

The last variety of spring that I shall have to speak of is what I have called the forceps spring, and of which we have an example in Salmon and Ody's truss. The curve of this, as will be seen from the figure, differs from all the preceding. When unapplied, its two ends approach to within about two inches of each other, so that the pads touch. It is strong, well-tempered, and very elastic, exceeding the elasticity of the English spring, and nearly equalling that of the French. It has a back and front pad, connected to its two ends by a ball and socket joint, and, like Miller's, is opposite-sided. Though not shown in the diagrams, which are intended to display only the "set" of the springs, this differs also from the others in not being what is called a shaped spring—*i.e.*, made to fit exactly the outline of that part of the body which it surrounds. In the latter there are other curves besides the set—viz., in the complete circle springs,—one downwards in the back, upwards in the haunch, and downwards again in the groin. In this, on the contrary, there are no such curves, and it is essential for the full action of the spring that it should not fit the body closely, but merely include it between its two ends, which grasp opposite points, caliper fashion.

In all shaped springs there is a certain loss of power, arising partly from the curves just alluded to, and partly from diffusion; much of the spring-force being expended over the hips, while in the forceps spring the whole is utilised. As regards the choice of this or that form of spring, much must depend on the shape of the patient and his idiosyncrasies; some find the concentrated pressure of four pounds less bearable than a diffused pressure of five pounds, while others fret under the constriction of a shaped spring, though of less power than a forceps spring.

What one should always first aim at is efficient retention; and, secondly, that this be carried out with as little irksomeness as possible to the patient.

In former papers I have shown how very unsatisfactory was the treatment of reducible hernia, owing to the want of instruments manufactured according to definite rules and of definite strength and form; and I made certain suggestions as to the size and form of pads, and the strength and curve of springs, with the nomenclature which should be adopted to indicate these. But to render the treatment of this affection still more efficient, we ought to be able to combine the advantages of different instruments, to adapt the peculiarities or special features of one to that of another—a German, French, or English spring to a Wood's, Cole's, or any other kind of pad—in short, to make every spring fit every pad. To render this practicable, it is necessary that the spring and pad should not be, as they now are in the majority of trusses, practically in one piece, but in separate pieces, as exemplified in Salmon and Ody's truss, and the spring made fixable to the centre of the pad, instead of to its upper border. It is further necessary that there should be a uniform gauge for screws, as well as for the other parts of the truss; thus, instead of the present uncertain and imperfect contrivance, we should have an instrument of precision, and this branch of instrumentation would be brought up to the level of others.

If the proposals in these papers were carried out, it would be easy for the surgeon either to supply his patient with the instrument best adapted to his case, or to keep a small stock of pads and springs by him as trial trusses, and, having determined the combination best suited to his patient, to give the order accordingly.

There are two other points to be attended to in the ordering of a truss—viz., the amount of pressure which the pad should make, and the angle at which it should be set on the spring, this being such as to ensure its lower border being parallel with the inguinal fold, and its surface lying flat against the abdominal wall when the patient is standing upright. As regards the first point, I have already shown in a former number of the Lancet that truss springs are gauged from about No. 15, which is exceedingly strong, up to 20, which is very weak. The strength, then, can be always indicated, by simply sending the number of the spring; this, however, does not afford us any exact information of the degree of pressure which it will exert, because this depends, as pointed out in the paper just referred to, not only on the thickness or gauge of the spring, but also on its length and set, and the extent to which it is uncurved. Assuming now that a common circular spring truss is required, it will be necessary to bear in mind that the greater the circumference of the patient's body, or, in other words, the greater the length of the spring, so much will its strength be diminished relatively, and that a 32-inch spring, of 17 gauge, is far stronger than one of 38 inches of the same gauge. So, likewise, if the antero-posterior diameter of the body measure eight inches in one individual, and nine in another, the same spring will exert a greater pressure in the latter than in the former.

As regards the pad being set at such an angle to the spring as shall ensure its lower border lying parallel with the inguinal fold and its flat surface against the abdomen when the patient is erect, there exists no mechanism for this purpose in the common truss; as it is made, so it must remain; hence the above angles ought to be carefully measured, and the truss-pad made to correspond. For the purpose of ascertaining these points, as well as for indicating the form of the pelvis, I have had constructed a half-spring skeleton truss, in which all the above points can be ascertained with great exactness and by a very simple mechanism, and as the instrument will be exhibited in the museum of the British Medical Association, I subjoin a short description of it. It consists of a half-circle wire spring of only one curve, and of a back and front pad, which slide along the spring, and can be fixed at any point and at any axial angle by means of a small side screw; thus the length of the spring required for the truss is indicated, and the horizontal, axial, or surface angle of the pad determined. The angle which the long axis of the pad should bear to the spring is ascertained in the same way as the axial angle—viz., by fitting the instrument on the patient, and fixing it at the required angle by means of a terminal screw. Where a shaped spring is desired, instead of a simple curved one, all that is necessary is to note the points at which the wire is not in contact with the body, together with the size of the interspaces, then, by placing it on a piece of paper, the shape of the outline can be drawn, and the exact curves indicated, with the greatest precision. The instrument is manufactured by Messrs. Maw, Son, and Thompson, Aldersgate Street.—*Lancet, July* 19, *and August* 2, 1873, *pp.* 75, 145.

57.—ON THE RADICAL CURE OF HERNIA.

By **John Wood**, Esq., F.R.S., Professor of Surgery in King's College, and Surgeon to King's College Hospital.

I have long thought that we might, in favourable cases, safely do more than we now attempt, to prevent a return of the protrusion after the operation for the relief of strangulation. After performing operations for the radical cure more than two hundred times, I had grounds for the belief (which other operations on the peritoneum also favoured) that, in a healthy subject, the peritoneum might be dealt with as freely and as safely as any other tissue; and, also, that the chances of bad results from peritonitis would depend upon the injury sustained by the bowel in strangulation, rather than upon any way of dealing with the peritoneal sac and parietes after the strangulation had been relieved, provided that due drainage be secured. In cases where the bowel and omentum are congested only, in young and healthy lads, with strangulated inguinal hernia, I concluded that the attempt would be justified, and would probably be successful. If so, the advantage of preventing a life-long trouble and danger from rupture by the operation which relieves strangulation is obvious. The kind of cases I selected for such an attempt, and the nature and results of the proceedings, will be best conveyed to your minds by a short *résumé* of the three cases where I have had the opportunity of carrying it into practice.

On June 29th, 1868, was admitted into King's College Hospital, a young man, A. F., aged 21, of Warden Road, St. Pancras, with a strangulated right oblique scrotal hernia. The tumour had occurred suddenly from lifting. It was of the size of the fist, and had been strangulated twenty-four hours. He had constipation, violent retching and vomiting, not fæcal, quick pulse and anxious face, pain in the epigastrium, and much pain and tenderness in the tumour, upon which several ineffectual attempts at taxis had been made. I put him well under chloroform, and made a fair and full attempt at reduction by the taxis, but in vain. I then made an oblique incision over the tumour, dividing the layers in the usual way, and laying open the sac to the extent of three inches. The sac contained a moderate amount of omentum, covering a knuckle of bowel, all congested, and the bowel slightly ecchymosed, and there was about an ounce and a half of bloody serum in the sac. The point of strangulation was at the deep inguinal ring, and it was divided by an upward cut. The bowel was then drawn down slightly and carefully examined. It presented the usual indentation, but was smooth and shining. It was then returned, and after it the omentum, the latter being carefully spread over the deep opening. The sides of the sac were then brought together, so as closely to embrace the cord over the whole length of the canal. The handled curved hernia needle, used in my operation for the radical cure, armed with silver wire, was then employed to bring together the sides of the sac, together with the aponeurotic structures along the whole length of the canal, in the way I have described in my work "On Rupture." A good view of the conjoined tendon was obtained, and the wire fixed in it in two places. The loop and ends of the doubled wire were then brought out at the upper and lower ends respectively of the incision. Four interrupted wire sutures were then placed through the skin between these points. The wound was dressed with carbolic lotion, and covered with gutta-percha skin and cotton wool, powdered with McDougall's powder to absorb the discharges. The interrupted sutures were removed on the fourth day, primary union having been by that time obtained throughout, except where the thick wires passed through the extremities of the wound, and effectively kept up the drainage. These were kept in for ten days. There was not the slightest sign throughout the case of the peritoneum being inflamed, and the abdominal tenderness which was present at the time of the operation passed away entirely. The sickness ceased directly after the operation, and the bowels were opened naturally two days afterwards. Erysipelas being present at this time in the ward, the patient was attacked by it on the sixth day. A partial re-opening of the wound was the consequence, together with suppuration in the fundus of the sac of the hernia. The pus passed freely along the wires, and there was no burrowing.

The suppuration in the sac caused obliteration and shrinking of that structure, and the testis was drawn by the subsequent contraction into the upper part of the scrotum. Some delay in the convalescence was then produced, but finally the patient was discharged, wearing a light truss, August 15th, 1868.

During the first year after the operation I saw him twice or three times. There was no cough-impulse whatever when last seen, all the parts being very firmly braced up in the groin and around the cord. As he was repeatedly enjoined to present himself at once if any pain or weakness showed itself, and seemed fully impressed with the danger from strangulation which he had escaped, I have, I think, some right to conclude that there has been no return. The difficulty of following cases for a number of years, in the nomadic habits of the population which furnishes the most numerously favourable cases for the radical cure, is one which I experienced in this case.

The next case was in a patient who had wished much to be operated on for the radical cure before the rupture became strangulated, in consequence of the impossibility of keeping it up with a truss, and the consequent disability from following his employment. I had refused to do so because of his age and the direct nature and great size of the openings.

M. W., aged 52, had suffered from a large, right, direct inguinal rupture for twenty-six years, which, though at first entirely reducible by rest, and the recumbent posture, could not for many years past, on account of the large size of the opening, be entirely pushed back or kept up by any of the great number of trusses which he had tried. He was admitted April 14th, 1872. Had been vomiting for eight hours, and was very weak and low. The tumour filled the scrotum, was of the size of the two fists, and was tense, tender, and painful, with the skin of the scrotum red and inflamed from handling. A somewhat prolonged trial of the taxis being made under chloroform, and after the application of ice for three hours, and by inversion of the patient without success, the operation of herniotomy was proceeded with by a single oblique incision four inches long. On opening the sac, a very large mass of omentum was found adherent at the neck, but not elsewhere, and covered by it was a large fold of small intestine, dark and congested, but still smooth and glistening. After division of the strangulating band at the deep opening, the bowel, being found not much damaged at the strictured part, was easily reduced. The omentum was congested, but not gangrenous, and, on account of its size and the adhesions, somewhat recent, at the neck, could not be fairly reduced or retained in the abdomen. The mass of fat, about three-quarters of a pound in weight, was then cut off close to the adhesions at the neck of the sac. Such vessels as bled were tied separately, and the stump being enclosed between the sides of the serous sac, four interrupted sutures of carbolised hemp were applied through the sac and tendinous parietes, and tied up so as to embrace the cord pretty closely. The two uppermost sutures were passed through the stump of omentum itself. These, and the ligatures upon the arteries, were then tied up in one knot, and left hanging out of the lower angle of the wound to act as a drain for the discharges from the face of the omental stump. Lateral pads of lint and a spica bandage were applied, and the patient placed with his knees drawn up and shoulders raised, and five grains of pilula saponis cum opio given every four hours. The vomiting and distress at once ceased; there was no tympanites and but little tenderness, and the bowels acted freely on the third day. An abscess subsequently formed in the lower part of the enormous sac, and a small slough appeared in the scrotum over the testis. Through the aperture left by this, the gland showed some tendency to protrude, but was easily kept in place by strapping and pads. By the use of a drainage-tube through the lower part of the primary incision, and out at the aperture left by the slough, the matter flowed away freely, and there was no tendency to burrow. The ligatures and sutures had all come away by the 29th of April, and on the 22nd of June he left the hospital, with the parts much thickened, firmly braced up, and wearing a truss with a large ring pad. I have seen this patient several times since then—once in this present year. There was then a bulge felt in the groin when he coughed (and he has always a bad winter cough); but there was no sign of any descent in the scrotum, and a light truss kept him very comfortable and able to do a good day's work without inconvenience. He also promised to come to me at once if he had any more trouble with it.

The third case was that of E. B., a waiter, of Surrey Street, Strand, aged 21, in whom the rupture, a right oblique scrotal one, had not been observed till the day before his admission, when, after lifting a heavy box, he felt pain and sickness, and felt a lump in his scrotum. A doctor to whom he applied gave him an aperient, after which he began to vomit, the pain was worse, and the tumour increased in size and filled the scrotum. This rapid appearance and increase of the rupture were afterwards explained by its proving to be a congenital hernia, with the sac formed by the tunica vaginalis, of which these peculiarities are very characteristic. On the 28th March, twenty-four hours after the occurrence of the rupture, the symptoms were very intense, and the taxis, with inversion and chloroform, having been fairly tried, after two hours' application of ice-bags, I performed herniotomy exactly as in the first case I have described. The omentum and bowel were found in the tunica vaginalis, both congested and slightly ecchymosed, with some effusion of bloody serum. The knuckle of bowel was strangulated, not only by the deep ring, but also by a band of the omentum, necessitating the drawing down of both until the constricted part could be seen and released. It was a case in which the bowel might easily have been passed into the abdomen with the stricture undivided. After the operation the patient was at once relieved; the bowels were opened normally on the third day; no tympanites or tenderness became apparent. The central part of the incision healed by adhesion; a little discharge passed along the wires, which were withdrawn on the fourteenth day after the operation. He was sent out of the hospital April 26th, 1873, with a firm adherent cicatrix, and no bulge whatever. On the 12th May he showed himself, wearing a light horse-shoe pad truss; not the slightest sign of a return was evident. This patient is here to-*day awaiting your* inspection.

There are also other patients upon whom the operation for the radical cure *has been performed.* One of them—the largest rupture I have ever succeeded in curing—was operated on eleven years ago, the patient having followed afterwards a very laborious occupation as a porter in Covent garden market. He wore a truss for only nine months after operation. I have brought him here to-day as a proof of the possibility of curing even a very large rupture by operation, and of curing it permanently, so as to relieve the patient of the trouble and expense of wearing a truss even under the pressure of very hard daily work.

It has been said, with respect to this operation, that evidence is wanting as to the permanency of the cure. and I am free to confess that it is exceedingly difficult to watch a couple of hundred cases for the space of ten or eleven years. The constitution of human nature is such that you cannot hope for the generality of patients to show themselves occasionally for this length of time, or even to write, if they are not further troubled with the ailment of which they have been cured. It requires that powerful spring to gratitude which was said by the cynical French philosopher to consist in "a lively sense of favours to come," which is wanting in a case where there is no more for the doctor to do. I have found that the unsuccessful cases are more likely, by the law of gratitude just enunciated, to return upon your hands than the successful ones are, as a bad shilling expects to be replaced by a good one. I think, therefore, we have the better right to the position of reckoning in the same proportion of failures and cures cases which have not been seen twelve months after the operation, as those which have been examined after that time.

Now, of 188, most of them unselected, cases of inguinal hernia—of the 200 and upwards I have operated on to the present time, of which I have notes, including seven females and four cases of double rupture, both operated on,—in 107 cases the results are more or less perfectly known. I find that 51 of these were more or less unsuccessful, of which 42 returned in the first year after operation—that is, the great majority of these were such as would do without wearing a truss after the first year. By far the greater number were so much improved that they were made comfortable by a truss, which was not the case in most instances before the operation. Some, but not many in number, were as bad as before the operation. Mr. Kingdon, of the London Truss Society, kindly forwarded to me in 1863, the names of three of those who had applied to that institution since the operation for the supply of a truss. 56 out of the 107 were cases which continued to be successful subsequently a year after the operation, as ascertained either by direct examination by myself, seen by other surgeons, or satisfactory to the patient himself, and either wearing no truss at all, or only occasionally, as a precaution, after the first year from the operation. Of these, seven were noted from thirteen to twenty-one months after the operation, seven from two years, seven from three years, seven from four to six years, seven from six to eight years, and four from nine to eleven years. Reckoning operations on both sides and repetitions of the operations, I have done the operation more than two hundred times. Out of these I have had three deaths—one from pyæmia, one from erysipelas, and one from peritonitis. These have been made public to the profession on more than one occasion, because I judged it right and fair that in an operation of this kind the facts should be made known as far as possible. In the last case, as shown by the post-mortem examination, the peritonitis was found not to have originated in the parts operated on, but in a knuckle of bowel which had been lodged in the hernia before the operation, while the patient was wearing a strong truss. The cases in which any signs of peritonitis were observed were not more than about twenty in the whole number. One and a half per cent. is not a high average of deaths from purely accidental surgical incidents, and there are very few operations of the kind—as, for example, for the removal of deformity, the cure of prolapsus of viscera, or of hemorrhoids—which could show more favourably either in this respect, or in respect to the somewhat severe test of the length of time in which they have been known to be without a relapse after the operation; and since 42 out of the 51 known unsuccessful cases proved to be so within the first year after operation, and most of the cases were examined once or more at various intervals of time after the operation, I think that in respect to this point we have a right *to claim the probability* of more, and the certainty of at least as *many, good results for the 81 of* which I have not been able to get notes after the first twelve months, as for the 107 in which I have done so. Under the age of twenty-one years the results in known cases are much more satisfactory, and amount to seventy-five per cent. of successful cases. But of dry statistics you will think that I have given you, perhaps, more than enough.

The determination of the question as to whether the operation for the radical cure is an appropriate alternative to a lifelong wearing of a truss, and a valuable supplement to the slow and very uncertain cure by truss pressure, will continue to depend upon the age, habits, circumstances, mode of life, and, to some extent, the cruel experience of trusses, and wish of the patient after having the matter fairly put before him, and, perhaps, nearly as much upon the anatomical knowledge, skill, energy, and experience of the surgeon, or his disposition to the finality frame of mind I have alluded to. In any case, whether universally, or only occasionally resorted to, it forms, I think, a valuable addition to the resources of surgery.—*Lancet, Aug.* 9, 1873, *p.* 185.

58.—ON A METHOD OF OPERATING IN CERTAIN CASES OF STRANGULATED HERNIA.

By Thomas Annandale, Esq., Surgeon to the Royal Infirmary, Edinburgh.

[The method of operating to which attention is directed in this paper has been previously suggested, but has never been practised on the human subject.]

The principle of this method consists in making a small incision through the abdominal walls, and opening the abdominal cavity *near* the hernia, instead of cutting into the hernial sac itself, or exposing it. One or more fingers are then to be inserted into the abdominal cavity, and the protruded structures contained in the hernial sac drawn back from within.

The chief objections which may be reasonably urged against this proceeding are—1st, the risks likely to follow opening into the peritoneal cavity; and, 2nd, that certain conditions of the hernial contents, such as gangrene, or firm adhesions, would prevent the operation being successfully carried out.

The first of these objections is not, in my opinion, very serious, but the latter is so, and therefore I would advise the method only in *certain* cases of strangulated or obstructed hernia.

The cases which appear to me to be most suitable for this operation are:—

1. Umbilical herniæ in the adult, especially if the coverings of the hernia are thin or ulcerated, or the hernia large.
2. Large inguinal or scrotal herniæ, which have been recently reducible.
3. Herniæ in other regions in which, owing to special conditions, the usual operation cannot satisfactorily be performed.

This operation is, however, only suggested when in these forms of herniæ the condition of strangulation or obstruction gives rise to symptoms which *absolutely* necessitate operative interference, and when signs of gangrene or a history of old standing irreducibility are absent.

The incision into the abdominal cavity does not require to be more than from one to two inches in length, and it is quite possible, as I found in the case related, to examine the condition of the intestine contained in the sac, by carefully drawing it, bit by bit, through such a wound. If thought right, it is also quite practicable and safe to divide, subcutaneously through the same wound, any constricting band or agent which may be felt after the hernial sac has been emptied of its contents.

In the case reported, I was induced to perform this operation because I was aware that the usual operation is very unsatisfactory in cases of large and strangulated umbilical hernia.

The operation completely relieved the strangulation and obstruction; the bowels being freely moved shortly after. No symptoms of peritonitis followed the operation; but the constant vomiting, and the inability to take the slightest nourishment for some days before, together with a fatty condition of the heart, caused, in my opinion, the exhaustion which proved fatal.

Case.—On the 23rd of April last, Dr. David Gordon asked me to see with him Mr. B., aged 61. The patient, who was very stout, and measured round the waist fifty-one inches, had suffered from a large umbilical hernia for many years. Three days before my visit he was suddenly seized, when travelling, with a pain in the abdomen, vomiting, and all the symptoms of *complete* intestinal obstruction. For two days he was treated at home by his friends, but on the morning of the third day he became alarmed at his condition, and sent for Dr. Gordon, who, at once recognising the serious nature of the case, requested a consultation with me.

An examination determined the presence of an umbilical hernia, the size of a large orange. There was considerable inflammation of the superficial textures round the hernia, and a superficial slough of the skin covering the hernia itself. The hernia was not tense, but on laying hold of it its contents felt somewhat hard, and they were not reducible. The abdomen was swollen and tympanitic, and the patient had pain, stercoraceous vomiting, and all the other signs of intestinal obstruction well marked. Attempts to reduce the hernia were quite ineffectual, and an operation appeared to give the only chance of relief. The patient's consent to the operation having been obtained, and chloroform administered, I made an incision three inches in length in the mesial line, midway between the umbilicus and ensiform cartilage; and after cutting through three inches of superficial fat, I carefully opened into the abdominal cavity, to the extent of about one inch and a half. A knuckle of gut at once protruded at the opening, and passing my fingers along the continuation of this portion of intestine, I found it passed into the hernial sac. The sac contained several inches (six at least) of this small intestine, which felt somewhat matted together, and also was adherent to the sac; the neck of the opening into the sac was slightly felt as a firm ring, but this ring was not very tight. Having determined these conditions with the finger, I gently drew open the intestine, which passed into the sac; and was pleased to find that the soft adhesions gave way and the whole contents of the sac were thus drawn out. As each portion of intestine contained in the sac was in this way drawn out, it was brought out through the abdominal wound and carefully examined. After the sac had been completely emptied, I introduced a probe-pointed knife through the wound, and freely divided, subcutaneously, the firm ring at the neck of the sac. The intestine contained in the sac was slightly congested in appearance, and on its surface were the remains of some recent adhesions, but there was no trace of any general peritonitis. The abdominal wound was closed by means of deep and superficial stitches, and antiseptic dressing applied.

Three hours after the operation the patient's bowels were freely moved, and all symptoms of obstruction had disappeared. Next day he was weak, and the bowels were again slightly opened, but he complained of no pain or other bad symptom. The exhaustion, however, continued, and he died somewhat unexpectedly about forty-eight hours after the operation.—*Edinburgh Medical Journal, Sept.* 1873, *p.* 209.

59.—ON COLO-PUNCTURE.

By Dr. Hector C. Cameron, Surgeon to the Royal Infirmary Dispensary, and to the Lock Hospital, Glasgow.

The subject of flatulent distension of the bowel is one of very great importance, as it may occur in every class of case, whether medical, surgical, or obstetrical, and prove the source of great suffering to the patient and of danger to his chance of recovery. There have been cases of puncture of the bowel by a fine trocar, published from time to time, but the success attending the operation has not been very encouraging, although the temporary relief given may have been great. The advantage of puncture of the bowel in acute strangulation has been *very clearly shown* by Mr. Bryant, to whose paper it may be *worth while to refer (Retrospect,* vol. lxv. p. 219).]

Mr. S., about 45 years of age, had been under my care for some months previously, suffering from a surgical affection in the right foot. An operation undertaken for the cure of this resulted in an inflammation extending up the limb, exhibiting itself by the symptoms of hardness and tenderness to the touch, for the most part in the course of the superficial veins of the leg and of the femoral vessels. This was subdued in the leg and lower part of the thigh by means of frequently renewed fomentations, and the exhibition of some mild antiphlogistic treatment. The neighbourhood of the groin, however, and upper part of the femoral region, remained swollen and painful, and the general feverish symptoms, which were very severe, continued comparatively unabated. In the course of another day or two, he began to complain of tenderness to the touch of the abdomen, and tympanitic swelling commenced. This was preceded by one or two sharp rigors, and accompanied with frequent, copious, and watery stools. The pain in the abdomen was soon severe and constant. This state of matters was somewhat insidious in onset, and obscure in origin, but was, we thought, probably an extension of the inflammation which existed in the right leg and thigh. At this juncture, Dr. M'Call Anderson saw him daily with me in consultation. The patient's condition soon became very precarious. The tongue was hard and dry; the pulse varied from 120 to 140; thirst was excessive; sickness occasional; while frequent rigours occurred, followed by most exhausting perspirations. The abdominal distension was extreme, causing pain, inability to take nourishment, and a distressing sense of weakness and breathlessness. It was always to a certain extent relieved by a motion from the bowels, but the gas soon re-accumulated. None of it could be voided, except when the patient was on the bedpan; and, because of the comfort he obtained for a short time after an evacuation, he was led to encourage their occurrence. As in this way they were frequent and always copious, it soon became very obvious that this continued diarrhœa was fast undermining his strength. Finding, therefore, that the bowels could be almost completely controlled by the injection of a drachm of laudanum in starch, given night and morning, Dr. M'Call Anderson proposed that we should by this means stop the diarrhœa, and treat the increased accumulation of gas by the operation of colopuncture. (All the usual remedies for the dispersion of flatulence were administered, some by the mouth, some by the rectum, others, such as turpentine, in both ways, but with no avail.)

Accordingly, on the 20th December, without rules for our guidance or warning, I made an attempt, Dr. M'Call Anderson being present, to puncture. The only canula which I could obtain, of at all suitable character, was small in diameter, but unfortunately fitted ill round the neck of the trocar, the latter having no shoulder on it at all.

Having driven them into the abdomen over the situation of the transverse colon, and withdrawn the trocar, we found that no gas escaped. I again introduced the trocar and, withdrawing the instrument a little, once more pushed it forward, but with no better success. It now became evident that the edge of the canula refused to enter the soft and yielding inflated gut, and we abandoned any further proceedings until the next day, when I obtained a more suitable instrument. This was a canula and trocar of good shape and small in diameter, but no longer than such as is commonly used in tapping a hydrocele. With this instrument I again punctured (21st Dec.), the distension being very great. The site chosen was very much the same as that of the day before, and, on withdrawing the trocar, the gas whistled freely out of the canula. After a very considerable quantity had escaped, but long before the abdomen had regained its natural shape, the escape suddenly stopped; and all attempts to obtain the re-establishment of it were fruitless. The gut in collapsing had evidently freed itself from the very short part of the canula it contained in its cavity. The patient, however, expressed himself as greatly relieved. On the following day (22nd Dec.), the distension was as great as ever; and an instrument of a satisfactory character, which had been ordered, having been obtained (the canula being 3½ inches in length, and ⅛th of an inch in diameter), I again punc*tured with* it in nearly the same position. The gas escaped freely, and the ab*domen fell fast.* Anxious to expel all the gas, we kept up rather firm pressure *on the abdomen*; but to our disgust, before all the gas had been thus got rid of, we pressed a quantity of liquid fæces through the canula. I wiped away carefully what had passed, and, giving time for any that remained in the canula to gravitate back, I plucked it out with my thumb firmly over the orifice, so as to retain, if possible, any fluid that might still be in the canula, in the same way as one lifts fluid in a pipette.

The patient was greatly relieved in breathing, and the pulse fell considerably even before we left the room. No evil consequence ensued, and he expressed himself that evening as being very comfortable.

On the following day (Dec. 23) the operation was repeated with equal benefit, but no pressure was used, so much of the gas only being got rid of as escaped spontaneously.

Again, on the next day (Dec. 24), I punctured in the situation of the descending colon, the patient lying on his back, and the instrument being introduced parallel to the surface of the bed. On withdrawing the trocar, a small quantity of liquid fæces was forcibly shot out on to the carpet of the room by the first burst of gas; but no more followed, and a great quantity of wind was got rid of.

On the following day (Dec. 25) I punctured in nearly the same position, but drove in the instrument at right angles to the surface of the bed, the patient still lying on his back. No fæces escaped, the evacuation of gas was most satisfactory, and the relief to the patient, as on all the other occasions, was very great.

On Dec. 26 it was not thought necessary to puncture, the flatus being relieved during the passing of a liquid stool, and not re-accumulating very rapidly.

On the morning of Dec. 27, however, the distension was again very great. About 11 a.m. I punctured in the situation of the transverse colon with good results. Shortly before mid-night of the same day my patient sent, begging I would repeat the operation. The distension was again extreme, and his sufferings from the embarrassment of the breathing very great.

I did repeat the operation, giving vent to an enormous quantity of wind, and leaving him much relieved, and inclined to sleep. On the following day we found that he had passed a good night, and, although considerable distension existed, the gas had not, we thought, re-accumulated to such an extent as to render operation necessary. Indeed, we never again required to puncture.

The distension, although it continued more or less for a week or ten days, never again attained an extreme limit; and, in a day or two after our last operation, he began to void small quantities of wind in a natural manner.

In a month afterwards, as regarded the affection of the abdomen, he was quite well, and has continued so since, although now in a most precarious state of health from serious ailments of chest and limb. Puncturation was performed, it will thus be seen, eight times in all, and on every occasion, except the first (when the bowel was not entered), to the marked relief of the patient. The operation itself, if effected with a trocar in good order, and by a quickly-delivered firm stab, is almost painless, and may be rendered absolutely so, as was done in the case under review on the last four or five occasions of puncturing by the application of a little ice and salt to the spot selected, or by the use of Richardson's spray apparatus. As to the selection of the point for puncture, I may say that I chose on each occasion a point over the position of some part of the colon, where the percussion was most distinctly tympanitic.

Only one of the little punctures gave any uneasiness whatever. Two days after it had been made, the patient complained of its being tender to the touch, and a little redness began to show itself in an area round it of the size of a sixpence. This culminated in a minute abscess in the skin, which burst, yielded a little pus, and closed without giving further trouble.

The class of cases to which this operation is applicable naturally falls into two important divisions: first, those in which, like my own, there exists no mechanical obstruction to the escape of the contents of the gut; but where the extreme accumulation of flatus is caused and maintained by the fact that the rapidity of its generation is out of all proportion to the power of getting rid of it; and, secondly, those in which some such mechanical impediment does exist. It is in this latter description of case that the operation has, of course, been most frequently performed, because the former is of much rarer occurrence, and can generally, I am aware, be managed by other and better-known methods of treatment. Thus a long tube passed up the gut as high as can be conveniently accomplished, when other means have failed, often accomplishes the object in view. Puncture, however, appears to me to possess certain advantages over the tube, and not, if the necessary precautions be adopted, to be accompanied by greater risks; for it appears there is some danger in the use of the tube, and at all events it will be allowed that there is considerable discomfort to the patient. The difficulty of its introduction is not always slight, and its success by no means uniform. In the cases where mechanical obstruction is the cause of distension, it must, I imagine, very generally fail, although it must be confessed that in such cases it may prove a very important aid in the diagnosis, and for this reason, if for no other, ought to be used. The great objection to the employment of the tube, however, as compared with that of the trocar, appears to me to be the undesirability of repeating the operation on more than one occasion. In the case we are now discussing I feel sure that it would not have been tolerated, even had other circumstances not rendered its use highly undesirable.

The gut has been punctured, I believe, in the case of strangulated hernia, as well as in such cases as I have referred to; punctured, of course, in that part of it included in the strangulated tumour. This has been done, but I fear with indifferent success, with the object of getting rid of the flatus contained in the strangulated bowel, and thus, by reducing its bulk, allowing of its easy return into the cavity of the abdomen. The example is one that the most of us, I fancy, would be very chary in following. Besides being open to all the objections brought against operations that provide for the return of strangulated hernia without opening the sac (that is, without being aware what the condition of the gut is, that we are returning), it appears to me to be open to this additional objection that, if it do not succeed and operation becomes necessary, the free handling of recently-punctured gut might possibly lead to unpleasant consequences.

It may be thought by some that it must be a rash proceeding in a case such as mine, where the peritoneum is in a state of more or less acute inflammation, to irritate it still further by driving through it and again withdrawing so substantial an instrument as the necessary canula and trocar. As a matter of fact, however, the operation appears in no way to increase the inflammatory action; and this will cause the less surprise when we remember, that Mr. Wathen has pointed out, that in one well-known case the operation of ovariotomy was undertaken, and followed with the best results, during an acute attack of peritonitis.

It is a matter for curious speculation why no escape of fluid should take place from these punctures into the cavity of the peritoneum. The gentleman I have just referred to, in a letter in the British Medical Journal for Nov. 25, 1871, wrote in regard to this :—" The size and character of the puncture when made with a fine trocar and canula are such, I think, as to bring the chances of extravasation very low. The puncture merely separates the muscular fibres, and on the withdrawal of the canula, the muscular planes immediately re-adjust themselves, or a protrusion of mucous membrane takes place, which entirely prevents the escape of the intestinal contents, as was demonstrated by Travers in cases of punctured wounds of intestine, when the incision does not extend beyond three lines in length. The influence of pressure of the abdominal walls and contents in cases of non-protruding intestine, is clearly shown by Mr. Erichsen, when he states that fæces escape from a smaller aperture in a protruding than in a non-protruding intestine. He also refers to two cases of wound of intestine without any extravasation. A similar case occurred in my father's practice, when a man, killing a pig, stabbed himself accidentally in the abdomen; the intestine did not protrude, but the patient passed a large quantity of blood per rectum. He recovered without a single symptom of extravasation. Clinical experience, therefore, shows that non-protruding punctured wounds of the intestine of less than three lines in diameter may be said not to allow of fæcal extravasation. The lesion in colo-puncture comes so much within these limits that I venture to think it may not deter us from resorting to this method of, at the least, relief."

In addition to the opinions here given by Mr. Wathen, I think we may be re-assured in regard to any risk of fæcal escape, by reflecting upon what the condition of the gut itself must be after the puncture has been made. Having just been enormously distended, and now being thoroughly collapsed, the mucous membrane must be thrown into very redundant folds, and this of itself will act in the way of occluding the very small wound made through the wall of the gut by the operation.

An idea is very prevalent that the operation of puncturing the intestines is frequently performed in the case of cattle, but this has been pointed out to be a mistake. So general, however, was the belief, that even so well-informed a writer as Sir Thomas Watson, has repeated it in the last edition of his lectures on the Practice of Physic. He writes:—"There is one further expedient which I should recommend in those trying cases which we know (no matter how) are of necessity fatal. In cattle that are *blown* by over-feeding on wet clover, a rough procedure, that of piercing the distended bowel with a hay-fork, has often been practised by farmers with complete success. The distress from extreme distension of the intestines by wind is so intense, the craving for relief from that distress so importunate, and the comfort on obtaining it so great, that were I the subject of such pressing and prolonged torment, I should beg to have the inflated bowel eased by puncture with a fine trocar, even if I might (what is improbable) lose a day or two of painful life." Now, the truth is, that the affection of cattle here referred to (which is known as "hove" or "blown") is an acute indigestion, and the distension occurs not in the intestines at all, but in the rumen or paunch. If a herd of cattle have been, by driving, subjected to a long fast and then turned into pasture, they are very apt to give full swing to their appetites and eat hurriedly and greedily, take no time to stop for rumination, and soon the rumen becomes so distended that it is unable, as sometimes happens in the human subject in the case of the over-distended bladder, to expel its contents. Fermentation takes place, gas is evolved, and the distension rapidly increases until the animal is destroyed by the pressure on the diaphragm causing suffocation.

Tympanites, or wind colic, as it is called, being really a collection of flatus in the bowels, does also occur, and it has been operated on successfully with a canula and trocar, but the operation appears to be attended with great danger, and is hardly thought justifiable except when the recovery of the animal is absolutely hopeless. So little apt, however, is the rumen or first stomach to suffer from inflammation or to resent operative interference, that it is customary to incise it to the extent of several inches, and to remove not only the gas, but the clover and grass which constitute the head and front of the offending. The difference, then, between colo-puncture and such an operation as puncturing the rumen in the ox will be seen at once, and the one in no respect affords a parallel to the other.

Allow me now to sum up these remarks by indicating what I think are the precautions necessary to be taken in the performance of the operation, precautions which are indicated by the several mishaps which, as I have told you, occurred during the treatment of my case.

1. Care must be taken to secure a canula which fits very accurately indeed round the collar of the trocar, so accurately that if the finger nail be run from the point downwards it should hardly be caught at all by the edge of the canula. Unfortunately, instruments are not always so made, although any ill-fitting at this point between the two component parts of the instrument is always, and in every class of case for which it is used, not only very undesirable, but even occasionally a source of embarrassment and annoyance to the operator. I have myself felt difficulty in driving such a badly constructed instrument into the tunica vaginalis, although the parts were put upon the stretch, and the hydrocele rendered as tense as possible by being grasped in my left hand in the usual way. And if this be the case with hydrocele, it will be readily understood how much more likely it must be to occur in the case of a soft and yielding bag of air, lying unsupported, so to speak, in a cavity like the abdomen. The mishap, it is true, is one devoid of any great peril, but at all events it disappoints the operator, and pains and alarms unnecessarily the patient, who, *finding no air issue from* the tube, comes to think that the hopes *of relief which have been raised* in his mind are illusory and vain.

2. Care should be taken that the instrument is of sufficient length. The ordinary hydrocele trocar, if we consider how great a part of it is occupied in passing through the thickness of the abdominal walls, is much too short. Only a small piece of the canula remains in the gut, and, as occurred in the present case, the gut in collapsing frees itself. The canula, then, should be three or four inches long. A certain length of this may be marked off by the finger and thumb grasping just above it, and this may be driven without fear into the abdomen. The trocar may then be withdrawn, leaving a great part of the canula projecting above the skin; and as the belly falls, this may, so to speak, be "paid in" to such extent as may prove necessary.

3. Any great amount of pressure upon the abdominal walls must be avoided. When the gas is nearly exhausted, one is tempted to use a little pressure; but it is just then that the danger of squeezing out fluid becomes greatest. If it is suspected that a quantity of fluid fæces is contained in the intestines as well as the gas, I should think it more prudent and safe to avoid pressure altogether, and to be content with such an escape as may occur spontaneously, and in all cases to make use of pressure with the greatest moderation. Although we have seen that fæces may escape through the canula and do no harm, it must be apparent to every one that such an accident introduces a grave element of danger. If I had to perform the operation in another case, I should take care to supply myself with small syringe, capable of fitting into the end of the canula; and by this means would be enabled to wash back into the bowel with a little tepid water any fluid which remained in the instrument before withdrawing it. The outside of that part of the tube which is in the gut seems to be wiped clean by its passage out of it.

4. Care ought to be taken always to puncture at that part of the intestine which is highest as the patient lies in bed. The neglect of this precaution may lead to a jet of liquid fæces being shot out with the first sudden explosion of gas, as occurred on the occasion when I trapped the descending colon; the direction of the trocar being parallel to the surface of the bed, and the patient lying on his back. The occurrence, I fancy, is somewhat analogous to the loss of fluid by the opening of a bottle of soda water or champagne held in a faulty position.

If such precautions be employed, it seems to me not too much to hope that, by means of this operation, we may occasionally save a life; or, at all events, we may do that which ought to be an object of equal importance,—when it is no longer possible to save life, we may lighten in some degree the shadow of death.—*Glasgow Medical Journal, May* 1873, *p.* 289.

60.—ON ULCERATION AND STRICTURE OF THE RECTUM.

By Christopher Heath, Esq., Surgeon to University College Hospital, and to the Hospital for Women, Soho Square.

One form of ulceration of the rectum is connected with fissure, and is readily cured by an incision through the middle of it. But there are cases of ulceration of the rectum of a different and more extensive character. They are most frequently met with on the dorsal aspect of the rectum, and occasionally extend completely round the bowel within a couple of inches of the anus. Their causes are various, but I believe the most common is syphilis. This form of disease is said to occur sometimes as a result of dysentery, and it is found occasionally without any obvious cause; but by far the most common cause, as I have seen over and over again among the out-patients attending here, is syphilis. I do not mean primary syphilis. Of course, unfortunately ulceration may be found here occasionally as the result of primary syphilis, though I have never myself seen such a case. But I refer to secondary or tertiary ulceration of the mucous membrane of the rectum, which may occur, just as may ulceration of the fauces or palate, during the course of constitutional syphilis. If ulceration is also present in the latter situation, it would, of course, much clear up the diagnosis of the nature of the affection of the rectum.

The symptoms are pain of a wearying constant character and profuse purulent discharge. The amount of discharge that may come from a patient under these circumstances is really quite remarkable. I have here the paper of a patient who is attending at the present moment, and who presents a well-marked example of this affection. She is a woman aged twenty-nine, who married eight years ago, and has had one child and one abortion. She stated that she had "been passing blood and corruption from the anus, and had had much pain, but that there was nothing else the matter with her." On examining the rectum, I found, an inch and a half from the anus, a hard irregular state of the bowel, ulcerating, and pouring out a large quantity of fluid. When this woman came under my care I put down my diagnosis, syphilitic ulceration of the bowel, and proceeded to treat it on the same principles that I should treat secondary ulceration of the fauces; I mean I used mercurial applications locally and gave iodide of potassium internally. With the object of checking the discharge she has been using sulphate of copper lotion, ten grains to the pint, the direction having been given to inject a quarter of a pint and retain it for ten minutes. She has been here to-day; the discharge has wholly ceased, and the ulceration is beginning to heal, and I hope we shall be able to cure her. Of course, there is the danger, to which I shall have to advert directly, that there may be an unhealthy degree of contraction, and a stricture may result. Obviously, the first thing is to get the ulceration to heal, and by passing a bougie occasionally we may prevent the contraction. I am sorry to say we see here a large number of such cases—of married women who become infected with syphilis and get ulceration about the rectum. A large number, too, do not come until the ulceration has got into the stage of stricture.

What is to be done with ordinary ulceration of the rectum?—for no doubt there is a simple ulceration occurring in the rectum, not syphilitic. In the first place, the great thing is to put the patient in perfect rest. Keep her in bed, and give a perfectly bland diet, which cannot possibly irritate the mucous membrane. If, after a fair trial of that for some time, the ulceration does not heal, the best plan, no doubt, is to apply caustic to it, treating the ulceration as you would treat an ulceration elsewhere which would not heal. Apply strong nitric acid, or, perhaps better still, the strong nitrate of mercury, a more certain caustic. Touching the surface with a glass rod or brush which has been dipped in the solution will often effectually check the unhealthy action, and may thus induce the parts to heal.

Whatever the form of ulceration may be, it may result in stricture. You may, it is said, have a stricture without previous ulceration. There may be such cases, and I believe there is one in this specimen which I shall have to refer to, having myself operated in the case by colotomy on account of stricture resulting from deposit in the wall of the rectum. In that case the scirrhous deposit in the wall never got to the stage of ulceration.

The most common form is where you have had ulceration of the mucous membrane ending in contraction and stricture. Of course, in the syphilitic cases, which are so common, you have more than in others the tendency to contraction. The female sex is more liable than the male to stricture and syphilitic ulcerations, for in a general hospital the number of females suffering from this affection is much larger than the number of males. Whether the syphilitic poison is infiltrated through the vaginal wall or not I cannot say. Certain it is that it seems to produce effects upon the lower bowel in the female which are comparatively unknown in the male.

We have, of course, the symptoms of stricture showing themselves in difficulty in the passage of the fæces. The contracted bowel gives rise to straining, and you find that the fæces are marked, flattened into a band, or broken off into pieces, and there is often more or less of semi-purulent discharge. On examining the bowel you readily detect the stricture within reach of the finger. You find a contracted bowel, and that the walls are more or less roughened; perhaps ulceration is still going on. By gradually and steadily introducing the finger you may be able to pass through the stricture, and feel the bowel perfectly sound beyond. When it is perfectly sound, I have often noticed that the mucous membrane is tightened up, feeling quite different from the ordinary healthy rectum; drawn, as it were, into longitudinal bands, much more than is the healthy bowel.

The treatment of these cases must be, of course, in great part mechanical. You have to dilate the stricture, and also, if possible, to produce a certain amount of absorption of the tissue which has given rise to the stricture itself. With regard to mechanical treatment, we come to the use of bougies, which is not unattended with danger. A hard bougie should never be used; it should be softened by being dipped in hot water, and thus any danger to the bowel may be avoided. The surgeon should try first and find what size will go through, and carry out the treatment just as he would with a stricture of the urethra. You cannot be too gentle with either one or the other.

If you have a very obstinate case of stricture of the rectum to deal with, you may have recourse, just as with stricture of the urethra, to some more speedy mode of dilatation. The two diseases are quite analogous. No one would use Holt's dilator unless the stricture were very obstinate; nor should I use this instrument —Armstrong Todd's rectal dilator—in a stricture which would dilate up to a good size. But one does meet with cases of stricture of the rectum which one cannot get on with at all, and there is no doubt that you can use this instrument, if you are careful, with very good effect. The blades of this dilator are parallel to one another, and are separated by an ingenious arrangement of cross bars worked by compressing the handles. Formerly it was found frequently to nip the mucous membrane, and often, in drawing it out, it would tear away a piece of the mucous membrane. The best plan to avoid this is to put over the dilator a finger-stall of india-rubber which can be thoroughly greased. After the dilator is introduced, the blades can be separated so as thoroughly to stretch the stricture, and you may thus make great progress with the case. You cannot, however, be too careful with this plan of treatment. Accidents have occurred with it: one to myself. A death occurred which I attributed to the use of this instrument, although I had not an opportunity of making a post-mortem.

These cases are, I should say, excessively troublesome. You may do some good, but if the patients will not assist you by passing bougies themselves the strictures soon contract, and are really almost incurable. It has become necessary, in some cases, even to do colotomy. I have a patient now in hospital on whom I performed colotomy some weeks ago for syphilitic ulceration and stricture with great relief.—*Lancet, May* 24, 1873, *p.* 725.

ORGANS OF URINE AND GENERATION.

61.—BRIEF RETROSPECT OF THE RECENT HISTORY OF LITHOTOMY, WITH AN ACCOUNT OF A NEW METHOD OF PERFORMING THE OPERATION.

By Dr. A. Davidson, Physician to the Queen of Madagascar.

What is the best mode of performing lithotomy? Formerly, surgeons were pretty generally agreed in recommending the ordinary lateral operation. This unanimity no longer exists. Even those teachers whose skill and success had rendered the lateral, *par excellence*, the operation in this country, have been lately confessing their doubts whether, after all, there may not be a better way of performing lithotomy than the time-honored operation of Cheselden.

Among the reasons alleged why this has again become one of the open questions of surgery, is the introduction of lithotrity. Since the crushing operation has come into more general use, the simpler cases of stone are less frequently treated by the knife; hence lithotomy has been reserved for the graver cases, and a corresponding increase in the rate of mortality has been the result.

This applies more particularly to the practice of a few of the leading surgeons who carefully weed out their best cases for the lithotrite, and in whose practice the cutting operation has doubtless become more hazardous. It is, after all, however, a matter of doubt, whether lithotomy is a more fatal operation now than formerly. Up to the present time, lithotrity has not been so generally resorted to as materially to affect the statistics of the operation. The late Mr. Holmes Coote remarked that, in St. Bartholomew's Hospital, the mortality has been about one in ten since the days of Cheselden down to the present time. This certainly is a remarkable fact; and all the more so if the mortality of this operation in the London hospitals generally be (as some have reckoned it) at from 20 to 25 per cent., one in five to one in four.

When we examine the annals of this operation in the past, we find that statistics then were quite as flexible as now. Raid, whose success induced Cheselden in England to adopt the lateral instead of the high operation, stated that he had operated in 1547 cases without losing a single patient. The only trustworthy statistics of his operation, preserved in the register of the College of Surgeons of Amsterdam, show that, in twenty-two cases operated on in that city, he lost four. This gives an average of one death in five and a half cases. Altogether, it is probable that the mortality at the present day is not greater than that formerly obtained. Be this as it may, there can be no doubt that the reopening of this question is, in a great measure, due to the fact that the success attained by other methods has been such as to cast doubts upon the assumed superiority of the lateral operation. The splendid results obtained by Cheselden, Crichton, Liston, and above all by Martineau, have not by any means been achieved by operators generally. The success in the lateral operation seems to depend a good deal on an uncommunicable knack—a sort of instinctive appreciation of the proper amount of work to be accomplished by the knife—an ability to measure accurately the force to be employed. As this can be neither taught nor acquired by rule, it has happened that the best surgeons and anatomists have often proved the most unfortunate in the practice of the lateral operation. The results of this method have been very unequal; by it have been obtained the *highest individual success*, and the *lowest general averages*.

As this fact has forced itself on the attention of surgeons, many modifications of the operation have, from time to time, been suggested, some of which are now beginning to be adopted; others, after having been prematurely lauded, have fallen into neglect.

There has never been much diversity of opinion about the *principles* of perineal lithotomy. Two points have been admitted by most surgeons. First, the necessity of an external wound, so situated, and of such an extent, as to afford free access to that part of the urethra to be opened, and allow ready escape of urine and other discharges without risking important structures. Secondly, an internal or prostatic wound, not larger than is absolutely necessary to permit the extraction of the stone without undue violence.

The exact position, form, and extent of the external opening has justly been looked upon as a matter of secondary importance. And so it is in itself; but the advantages of having direct and free access to the membranous portion of the urethra for the purpose of rendering the subsequent steps in the operation more easy and precise, has, until recently, been rather overlooked.

What size of internal wound suffices to fulfil the admitted condition has given rise to much discussion. Thus, we find the late Professor Syme, in 1842, stating that he considered it necessary to "cut freely through the whole thickness of the prostate, in order to obtain room for extracting an ordinary calculus;" and he said that he had never seen any bad consequences follow this practice. While, in the succeeding year, Fergusson advocated the principle of limited internal incisions; and, so far from *cutting through*, advised that only a *slight notch* be made into the left lobe of the prostate.

That the advocates of smaller incisions frequently made wounds *unintentionally*, not much different from those advocated by Professor Syme, is very probable.

Mr. Bryan, *a London surgeon, writing* about this time in the Lancet, says: —"*I examined, at every opportunity* I had, the wound made on those subjects which had been used in exemplifying to surgical classes the manner of operating. In every case which I met with, I found the prostate completely divided, and the vesiculæ sliced into; although the incisions had evidently been made according to the rules taught in the schools of surgery.

Those who believed that complete section of the prostate was the smallest wound, fulfilling all the requirements of the case, as well as those who aimed only at notching the gland, all agreed as to the desirability of rendering the wound as exact as possible, both as regards site and extent. Although generally content with the ordinary lithotomy staff and knife, doubts seem now and again to have arisen in the minds of some, belonging to either party, whether the desired degree of exactness could be attained by the ordinary instruments used in the ordinary way. Thus, we find Syme, in 1844, describing a peculiar knife called a prostatome, but he does not seem to have long continued its use. He also used, for a time, a straight guide upon which to make the prostatic wound. About the same time, or shortly before, Sir William Fergusson recommended a knife peculiar for the narrowness of its blade.

With the same object in view—viz., that of securing an internal opening of definite size, Sir Philip Crampton, in operating, made the deep wound with "a narrow knife, blunt for half an inch from the point," which lay concealed within the groove of the staff. After pushing this into the bladder, he removed the staff, and, lateralizing the knife, passed his forefinger along its back into the bladder. He remarked that, by this means, "the opening into the bladder is, in all cases, and without reference to the prostate, exactly sufficient to receive the forefinger of the operator, plus the blade of the bistoury." The importance attached to this point in the operation is further evinced by the interest taken at the time in the discussion about the way in which Liston held his knife—whether "overhand" or "underhand?" It was simply a question whether he held it in such a manner as to make an indefinite thrust into the prostate, or so as to be better able to guide and control the advance of the blade along the groove in the staff.

In 1844, Sir John Fife operated by a method which seems to have been precisely similar to the medio-lateral operation lately introduced by Sir William Fergusson. Two years later this same mode of operating was practised with very good results in the Manchester Infirmary by Mr. Ransome; but it did not at that time receive much attention from the profession.

In the year 1848, another, and what has proved a most important effort towards improving and simplifying lithotomy, was made by Professor Buchanan, of Glasgow, who then described his mesial operation with the rectangular staff, the mode of performing which I need not detail. He claimed for it the following advantages: ease of performance, a small incision into the prostate of definite size, with less risk of hemorrhage and deep-seated infiltrations. This operation, practised with a success that has compelled respectful attention, must be recognised as the first successful effort at median lithotomy. There are men who condemn all attempts at improved modes of operating with new instruments as dangerous devices to make up for deficiency of surgical skill. Such men had nothing good to say of what was then somewhat contemptuously styled the "rectangular operation."

We may just notice, in passing, that Dr. Alfred Post, of New-York, Dr. Warren, Dr. Steven, and others in America, were about the same time advocating and practising modifications of the bilateral. Dr. Warren adopted the Celsian external incision, opened the membranous part of the urethra in front of the prostate, and after introducing into the bladder a probe-pointed knife, having a long handle, he withdrew the staff. He then directed the knife, supported by the forefinger of the left hand, first towards the left tuberosity of the ischium, then towards the right, cutting the prostate to the extent of three-fourths of an inch on either side. He advocated this operation on the following grounds:—1st, The parts cut are more simple; the nerves and vessels being smaller, the pain and hemorrhage are less. 2nd, The access to the urethra is more direct. 3rd, The prostate can be cut with great precision. *4th, The opening* can be made double what can be attained by the lateral *without transgressing* the limits of safety. The advantages of the externa semilunar incision are thus clearly recognised and stated—directness of access to the urethra allowing of greater precision in the prostatic incision.

The result of Dr. Buchanan's ingenuity and success was evident in the renewed attention paid to median lithotomy. In the year 1850, Fergusson recorded a case in which he operated by a ⅄-shaped external incision, as suggested by Professor Eve, of Nashville. He has stated lately that from this time, although he continued to teach and practise the lateral operation, his leaning towards the central part of the perinæum had been leading him, instead of beginning his incisions on the left side of the raphe, to venture more and more to the right.

Allarton's median operation was first described in 1854. In this operation the prostate and neck of bladder are left untouched by the knife, and dilatation pure and simple is made to take the place of incision. Many surgeons, both in London and in the provinces, gave this operation a trial; and various instruments were invented for effecting dilatation.

The mortality in the patients operated on by this method contrasts favourably with that in the lateral operation; but the impression obtained by a study of the reported cases is, that many surgeons judiciously restricted its employment to those cases in which the stone was small, and in which dilatation would suffice for its removal. The greater number of the cases of death after this method have just been when dilatation equivalent to laceration had to be practised in order to extract a large calculus. One thing is certain, that this operation paid little respect to that structure demonstrated by Liston and others, to which the late Professor Syme directed attention and attached great importance,—"the sensitive ring which surrounds the neck of the bladder, at the base of the prostate in the male, and at the corresponding part in the female, which forms an obstacle admitting of ready removal by incision, and cannot be overcome by tearing without almost certain death."

The results of Allarton's median lithotomy, as well as Willis's operation of lithectasy, proved that the danger from forcible dilatation, although real and considerable, had been exaggerated.

Allarton's medial operation differs from lithectasy as proposed by Willis, inasmuch as the former effects by rapid, what the other accomplishes by slow, dilatation. Slow dilatation is safe enough when applied to the intact female urethra, but following and affecting a wound, it is both painful and dangerous. The danger in the case of lithectasy arises from the continued irritation of slow and increasing tension acting on parts particularly disposed to inflammatory action from having a wound on the one side and a foreign body on the other. Rapid dilatation, again, although harmless when not carried beyond a certain point, becomes dangerous when it exceeds that limit. It is probable that, if the urethra be dilated much beyond one-third of an inch, the process then becomes another name for laceration; and when a large stone is dragged through a wound so dilated, bruising follows laceration. Such injuries cannot be treated lightly, especially when, as in the case of lithotomy, they involve large venous plexuses and friable glandular tissues, exposed to the passage of irritating fluids, and at the entrance of a viscus having extensive sympathetic connexions. Experience has confirmed what might have been guessed *a priori*, that death, when it occurs after such operations, is due to violence. The fact that the fatal results are not more common after such procedures, is confirmation of the truth of the observation, that laceration and bruising, with limited incisions, although far from harmless, are less fatal than an excess of cutting.

The late Mr. Lloyd's recto-urethral operation is also a median one, and is the same as Allarton's in so far as the treatment of the prostate is concerned. It has one advantage, however, over Allarton's, for it gives a freer external wound, thus facilitating the dilatation as well as the subsequent extraction. The wound in the rectum communicating with the urethra, not with the bladder, readily heals, as I can myself testify. He continued to use this operation for many years with excellent results, but it has never met with much favour from the profession generally.

We have already observed that various operations, which may be called *medio-lateral ones*, had been practised by different surgeons years ago. These, if we except Dr. Buchanan's, never attracted much attention. In 1867, Sir William Fergusson operated on a boy by an external semilunar incision, making the deep wound precisely as in the lateral operation. He lays it down as a rule, that "the deeper the wound, the more difficult and dangerous the operation," particularly in respect to the seizing and extracting of the stone, and insists that the lunated incision gives a much more extensive, patent, and direct external opening than any straight line could afford. The reports of cases treated by this operation by so eminent a surgeon, and who had all along been one of the most illustrious advocates of the lateral operation, and who had practised it with acknowledged success, directed the attention of the profession to this subject, and brought out the fact that several of the leading surgeons had already given up the exclusive use of the lateral operation.

Thus, Erichsen writes in the Lancet, 11th January, 1868:—"Although practising, as a rule, lateral lithotomy by Cheselden's method, as modified by Liston, I have for a long time had grave doubts whether it was really the best way of getting into the bladder, and especially of extracting a large stone out of it." He prefers Dupuytren's operation, but performed with a rectangular staff. The external wound is semilunar, but he makes the deep wound with a lithotome caché, cutting equally into both lateral lobes of the prostate.

In the same way, Sir Henry Thompson informs us that he also has given up the exclusive use of the lateral. He practises Civiale's operation, the essential features of which are a vertical mesial external incision, as in Allarton's, with a deep incision similar to that practised by Erichsen, made with a similar instrument. Hutchison, who, so early as 1857, had made use of a rectangular catheter staff, which was simply a modification of Buchanan's, reported that he had for some time operated by various forms of the double-cutting gorget, and with a mesial incision.

It is thus evident that the lateral operation is no longer held in the same esteem that it once was. No doubt, there are fashions in surgical incisions and operations just as in other things; but the growing opinion in favour of the semilunar incision cannot be so accounted for. The men who have declared in its favour are men who are well able to compare and contrast it with the lateral incision. The same remarks apply to the pretty general adoption of the rectangular staff.

Before the publication of these innovations, I had abandoned the lateral in favour of an operation which I shall now describe. The staff I use is simply a modification of Buchanan's rectangular one, the acute angle being replaced by a gentle curve, and grooved on its inner aspect. I may just remark, in passing, that Buchanan's staff, as modified, is an excellent instrument for performing any of the ordinary operations. It forms a useful guide to that part of the canal which is to be opened, conducts straight into the bladder, without endangering the rectum. The form of the external wound in the operation I am now describing is semilunar, curved somewhat lower and deeper upon the left side than the right, in order to afford a free drain on the side on which the prostatic wound is situated.

Having exposed the membranous part of the urethra, this is opened, and a guide is introduced along the groove in the staff into the bladder. This guide, consisting of two parallel and connected bars or blades, capable of being separated by means of a screw, forms, when approximated, an instrument about five inches long, somewhat like a female catheter in size, straight, slightly flattened from above downwards, and grooved on the left side so as to permit the bottom point of the knife to slide along without escaping from it. When this guide has been fairly introduced into the bladder the staff is withdrawn, and the blades are separated by the screw *to such an extent as to render the tissues tense.* The knife used has a button-shaped extremity to fit the groove in the guide. The blade is narrow throughout, but is slightly triangular, becoming a little broader towards the handle than at the point. This knife is then carried along the groove so as to divide the tense resisting structures, *by its edge being brought into contact with them rather than by actual cutting.* The blades of the guide are now expanded to a sufficient extent. Should it be found, however, that the required degree of separation cannot be effected without force, the knife may be passed along a second time to cut the structures that still resist. In no case should anything be incised except what is found to oppose gentle dilatation. The size of the calculus determines the extent to which the knife is to be used and dilatation effected.

The blades of the instrument having now been separated so far as the size of the stone seems to demand, it serves as a conductor for the forceps into the bladder. They slip along between the blades of the instrument. The stone is then extracted in the usual way. Should the calculus be very large, the knife may be applied in precisely the same manner to the right side of the prostate, thus making a bilateral incision. This is seldom required.

The chief advantages of this, as compared with other perineal operations, are the following:—In the lateral operation, the incision passes indefinitely into the prostate, sometimes dividing it entirely, and at other times only notching it, and this *quite apart from the purpose of the operator.*

There is thus a want of exactness as regards the extent of the wound in the prostate. Then there is no less a want of discrimination as regards the structures cut; those that require incision, in order to permit easy dilatation, being sometimes left intact, while as frequently the knife passes indefinitely into or through the prostate without reference to resistance. Further, the incision bears no proportion to the size of calculus to be extracted. Mr. Oliver Pemberton, in his Address on Surgery at the last meeting of the British Medical Association, notices this point, and says:—"I hold that it cannot be necessary to make incisions to the same extent to remove a body the size of a pea as we should make for one the size of twenty." I hold so too; but, in lateral lithotomy, this is practically the case. A similar vice, however, lies at the root of Allarton's operation, which Mr. Pemberton has adopted instead of the lateral. It treats all stones, whatever may be their size, in the same way; and if the stone be large, and the tissues tense, then more force must be applied.

In the lateral operation, the manual dilatation which follows the incision has reference to the admission of the forceps; the subsequent dilatation, which is practised in the act of extraction, is the complement of the incision. The extent to which this is carried, and the force required to effect it, vary according to the size of the calculus. In the median operation, laceration and bruising are inevitable when the stone is large. The same holds true of the lateral, when merely the apex of the prostate has been notched.

In the operation I have described, the incision and dilatation are in proportion to the size of the stone and the resistance of tissues.

Mr. Martineau of Norwich, who cut eighty-four patients, with only two deaths, has been reckoned amongst the advocates of large incisions. More truly he may be regarded as an advocate for *discriminating* incisions—incisions bearing reference to the size of the stone—incisions also bearing special reference to the *resistance of tissues.* He says:—"Should the stone be large, or there be any difficulty in the operation, rather than use much force, while the forceps have a firm hold of the stone, I give the handles to the assistant, who is to draw them outwards and upwards while the part forming the stricture is cut; which is easily done, as the broad part of the blade becomes a director to the knife; and rather than lacerate, I have often repeated this enlargement of the inner wound two or three times." I have little doubt that this practice of incising what resisted, and in proportion to the resistance and to the size of the stone, explains his exceptional success. One of the most difficult parts of the operation, or it may be one of the most dangerous, even when not difficult, is the introduction of the forceps. The neck of the bladder is apt to be somewhat rudely disturbed in this part of the operation, more especially if the wound be deeply situated, and just sufficient to admit the forefinger. Remove the forefinger, and the wound contracts, so that the forceps are more likely to push up the neck of the bladder than enter it. Introduce the forceps along with the finger, then the forceps and finger jointly produce a considerable degree of bruising. To introduce the forceps *while in the act* of removing the finger is a good precept, but somewhat difficult to follow out. In this operation the guide for the knife acts as a conductor for the forceps at the same time.

Some of the special accidents of lateral lithotomy are less likely to occur in this operation. *Troublesome or even fatal* hemorrhage is one of the worst of these. *One in eight cases of death after* lateral lithotomy is due to this cause. It has occurred to such men as Sir Charles Bell in England, Klein in Germany, and Dr. Physic in America. Mr. Pemberton of Birmingham relates such a case in his practice, in which death followed after twenty-four hours. The patient was a boy. This surgeon remarks:—"I could find nothing in the post-mortem to account for the bleeding. . . . . It was clear the cutting did it somewhere, and I naturally thought I should like, in the future, to extract such a stone as the one I then removed with less of the cutting in the dark." In operating as I have described, from the open nature of the wound and its situation, dangers of cutting in the dark are prevented. Inflammation of the neck of the bladder and pyæmia caused by bruising, and infiltration of urine and pelvic abscesses, the result of too extensive incisions, are alike avoided.

Statistics.—The number of cases operated on in the Antananarivo Hospital by this method since the arrival of my colleagues, Drs. Borchgrevink and Mackie, is 32. Of these, 11 were operated on by myself, and 21 by my colleagues. One of my cases was followed by a fatal result about a month after the operation, and a few days after the patient had actually left the hospital. A post-mortem examination revealed extensive disease of both kidneys. There were several stones and abscesses in the right kidney; the left kidney also contained a few small calculi, and was excessively engorged. The prostatic wound had completely healed, but the external wound was not entirely cicatrized. Of the 21 cases operated on by Drs. Borchgrevink and Mackie, 13 had occurred before I left the island fifteen months ago; and 4 were still in hospital when I heard last, and were reported as progressing favourably. One death occurred amongst the 13 operated on before I left. He was one of Dr. Mackie's patients. No post-mortem examination was obtained, but the symptoms pointed to kidney disease. I am not able to give at present exact details of the ages of the patients and the size of the calculi in these cases. I hope to publish these particulars on a future occasion.

The cases I operated on myself were most of them very favourable ones for the operation, the patients being all under puberty, and the stones of moderate size. The largest stone that I have extracted *by this operation* weighed 2 oz. and ʒ i., from a boy aged 5 years, who had suffered from the symptoms of calculus ever since he had been a year old.

The patients operated on by my colleagues were some of them adults. I believe I am safe in saying, that none of the stones extracted in the 13 cases operated on while I was there weighed above 2 oz.

In one of the cases that occurred after I left, the stone extracted weighed 1590 grains, *i.e.*, considerably above 3½ oz.

Were the object in view to *contribute* to the already superabundant mass of lithotomy statistics, the four cases (the final result in which may be looked upon as more or less doubtful) would have to be eliminated, and, according to the principles upon which statistics are compiled, the fatal case which occurred in my practice, since it took place after the patient had left the hospital, would also be left out of the calculation, giving a mortality of 1 case in 28. But there is no sufficient reason to exclude this case, as the object of all operations is the lengthening of the life of the patient. In this case I have little doubt that the operation shortened his life to some extent. The general result—1 in 16—attained by this operation, I consider, has been above the average, even admitting, as I do, that the cases were upon the whole more favourable for the operation than those met with in hospital practice at home.—*Edinburgh Medical Journal, June* 1873, *p.* 1057.

62.—ON URETHROTOMY OR LITHOTRITY IN AGED AND DEBILITATED PATIENTS.

By W. L. CROWTHER, Esq., Tasmania.

In the Lancet of the 8th March appeared the report of a clinical lecture *delivered by Sir Henry* Thompson at University College Hospital, and it is stated *in some editorial* remarks in the same journal such lecture "represents that distinguished surgeon's views upon a most important question." It is asked whether, in the presence of a calculus, chronic degeneration of the kidney can in all cases be diagnosed; and, if so, what is the best treatment to be adopted under the circumstances?

Before entering upon this subject in detail, I may say it is not my intention to dispute, even were I able to do so, Sir Henry Thompson's views upon a subject upon which he is so well entitled to speak with authority; nor am I prepared to say aught that would lessen the value and importance of lithotrity as a scientific operation as performed by such a practised hand as that of Sir Henry himself, nor gainsay the fact that there are numerous cases to which it is applicable; but to direct the attention of the profession to the circumstance that there is an operation infinitely safer than lithotrity for the class of cases to which Sir Henry refers, and which, in my humble opinion, gives the only chance for, if not restoration to health, at least prolongation of life—I mean urethrotomy by the median section, with simple dilatation by the finger of the prostate gland. My experience as an operator must of necessity be vastly inferior to that of Sir Henry Thompson and other metropolitan celebrities; but as it has fallen to my lot during the last thirty years, amongst many others, to deal with a few of the class of cases to which reference has been made, I should not be performing my duty either to the profession or the public without making known the result. I believe—and all surgeons of any experience must be agreed upon the point—that there is no certain means, especially in cases of calculi of large size, of determining with accuracy the actual amount of organic lesion to which the kidney or bladder, or both, have been subjected; or, in other words, what there is left, were the calculus removed, as a rallying point to the feeble powers of life. The ordinary tests, chemical and microscopical, give indications of a very reliable character, by which, coupled with the history of the patient, his state of health and mode of life antecedently to the formation of the calculus, and more particularly the effect upon the constitution subsequently to such formation, a diagnosis may be made sufficiently accurate to at least warn the patient and his friends of the danger attendant upon any operation, however well executed, and what the chances may be of success; and the question to be answered is, By what treatment will that danger be least? To that question I am prepared to reply.

I have no data before me other than those supplied by Sir Henry Thompson's lecture, and another case of his, that of the Emperor Napoleon, as to the amount of shock to which the system is subjected by the crushing process, but these will be sufficient for my purpose. Of the four cases two recovered. The second had "*five* sittings in ten weeks," and, *although handled with the utmost caution*, had "prolonged fever, and severe rigors following some of them," and was, for a time at least, in imminent danger. In the third "*five* sittings sufficed to remove *nearly all* the stone; but the last was followed by *severe shivering and vomiting*, and death in a few days." The fourth, that of the Emperor Napoleon, although his condition was to a certainty better than any of the preceding, exhibited precisely similar symptoms, and sank after the third operation, *half the stones only having been dealt with*. Sir Henry Thompson, in his work on "Lithotomy and Lithotrity," makes the following significant remark:—"But a little reflection may suffice to convince us that in all cases it is unwise to cut into, *and still more so to crush* in, the cavity of a bladder which is the subject of unchecked chronic inflammation, as almost all bladders are, in a greater or less degree. In such a condition a very little interference will often set up acute cystitis, and from this source morbid action may invade the kidneys and prove rapidly fatal." And in the leading article referred to the question is asked, "What is to be done when a patient is worn down by long-continued sufferings?" The answer, based upon the opinion just quoted, were there none other, is a simple one: Select for the removal of the foreign occupant from the bladder the operation that will produce the slightest shock, and that *only a single one*, to the nervous system, at once relieves the bladder and constitution of all irritation, involves no cutting either of the prostate gland or bladder itself, and in its performance is attended *in most cases* with the smallest loss of blood, is completed in one or two minutes, entails no risk of subsequent hemorrhage, and *makes but little demand upon* the powers of life for its reparation. That operation, *applicable alike to stones of all sizes*, is the one to which I have previously drawn attention—"urethrotomy by the median section with dilatation of the prostate gland." Mr. Erichsen in 1859 drew attention in the columns of the Lancet to the benefit of the "median operation" with prostatic dilatation, but limited it to stones of small size or such as would now be dealt with by lithotrity. Long before his paper appeared, and adopting the mode of section proposed by Allarton, I had made it applicable to such cases as those referred to by Sir Henry Thompson, believing the safety of the patient, whether young or old, and particularly the latter, was greatly enhanced by the incision passing through structures that were in the main "aponeurotic," and from the restricted use of the knife in the deeper parts the *grave perils of the ordinary lateral operation were wholly avoided.*

Appended are the notes of a case in support of the opinion, if such were wanting, that the shock to the system in this operation is slighter than in any other, and the recovery more certain. I have long been of opinion that the defects inherent in the crushing process in aged people, more particularly when the calculi are above the "medium" size, and whose bladders are in a high state of irritation and inflammation, such as the Emperor Napoleon's was proved to have been prior to the first operation—"urine bloody and purulent,"—arise not only from the direct mechanical embarrassment to which the bladder is subjected at each sitting, but that the shock to the system of any single crushing is equal to, if not far in excess, of that arising from the operation I have been in the habit of performing successfully; and I must be pardoned if I give offence by expressing the conviction—but it is forced upon me, and that by the reasoning of Sir Henry Thompson himself—that in the Emperor's case the system would have borne the single shock of the median section with non-incision of the prostate, but could not bear up, in the presence of so large an amount of organic disease, against the repeated assaults upon it necessary to rid the bladder of a stone of such large proportions as the post-mortem examination revealed the Emperor's to have been.

Samuel H., aged seventy-five, has for the last two years had well-marked symptoms of vesical calculus, but until a short time since did not submit to an exploration of the bladder. Is extremely irritable, and exhibits a great dread of any operation. Health and constitution much broken; countenance pallid and waxy; and the sounds of the heart give evidence of extensive atheromatous deposit. The urine is bloody and muciform, and under the microscope exhibits tube-casts and pus-globules; sp. gr. 1020.

April 9th, 1873. 12.15 p.m.: After having been placed under the influence of chloroform, "urethrotomy" by the median section was performed. The time occupied in completing the operation was two minutes. The loss of blood did not exceed one ounce. Calculus lithic-acid.—5 p.m.: Pulse firm. Has slept at intervals. There has been a copious flow of urine, and slight venous oozing from the wound.—10 p.m.: No constitutional disturbance. Has taken diluents and broth freely; and, beyond a little scalding during the transit of urine, has no uneasiness.

10th. 10 a.m.: Has had a good night. Urine clear, and passed through the wound at intervals only.—5 p.m.: Has full control over the bladder, making water into a bed-pan as occasion demands. Tongue moist and clean. No febrile disturbance.

11th. 7 a.m.: Bowels relieved by an enema. Wound contracting rapidly.—8 p.m.: Has risen from his bed every two hours to empty the bladder, urine passing each time both by the wound and urethra. Slept during the day. Says he is quite comfortable. Tongue moist; no thirst.

12th. Has not passed any water through the wound since 10 p.m. last evening. Empties the bladder every three hours. 6 p.m.: Continues the same.

13th. Complains of pain in the urethra during the passage of urine, and appears to have a little uneasiness about the neck of the bladder, with increased desire to micturate. Ordered a warm hip-bath and domestic enema. Incision contracted to a mere line.—6 p.m.: Had perfect relief by the warm-bath. Has been up in a chair for two hours, and is gaining ground rapidly.

14th. 7 a.m.: Has slept well. Wound closed. Bladder emptied by the natural passage every three hours during the night, and without pain.

15th. Sat up for four hours. Has no uneasiness, and is now convalescent.

17th. Rose early this morning, and, at the time of my visit, has dressed and walked about the house.

In this case, after the first twenty-four hours, the patient had full control over the bladder, and did not soil his bed; and, as soon as the effects of the chloroform had passed away, gave no evidence of either mental or physical embarrassment. In some of the other cases, with much larger calculi, the recovery has been equally rapid, and nearly all were marked by an absence of any important local or constitutional symptoms.—*Lancet, Nov.* 1, 1873, *p.* 624.

[Mr. W. F. Teevan does not agree with Mr. Crowther in his views about the prostate. He says Mr. Crowther thus explains his method:—"I mean urethrotomy by the median section, with simple dilatation by the finger of the prostate gland."]

Now, Professor Ellis has, years ago, shown that the prostate could not be dilated, and I would recommend any surgeon who has a doubt on the point to attend his incontrovertible demonstration on the subject. To say more would almost seem superfluous, but it may not perhaps be remembered that the prostate gland is encased in two jackets of white fibrous tissue, so dense and strong that the capsule is described, in Quain and Sharpey's Anatomy, fifth edition, page 1216, as "rather difficult either to tear or cut;" and yet this coat enveloping the prostate is supposed by many surgeons to stretch like a piece of india-rubber to kindly allow them to take out the stone and then to quietly return to its pristine condition. But facts prove that the prostate has never dilated for any surgeon, for it has only given way when lacerated or cut. Why has Dolbeau introduced perineal lithotrity? Simply because he found, by experiment on the dead subject, that the prostatic urethra could not be dilated beyond twenty millimetres, which is about its normal diameter in old men. The experiments which I myself performed in reference to the subject proved that the prostate will only admit the terminal joint of the forefinger without lacerating, and a personal examination of all the specimens in the various museums in London a few years ago, showed me that in no single instance had a stone been removed from the bladder without lacerating or cutting the gland. Mr. Crowther refers to a lecture of Mr. Erichsen's; but, unless I am very much mistaken, that gentleman has considerably modified his views, and believes that in practical lithotomy, small calculi excepted, a stone must either be cut out or torn out. The objection to the method of so-called dilatation is, that it involves a laceration or bruising of the plexus of veins surrounding the prostate, and hence the common cause of death after lithotomy is pyæmia; and the procedure has, moreover, to answer for those disasters, by no means few in number, which have befallen lithotomists. Those surgeons who extract calculi by the so-called process of dilatation, which is in reality laceration, lose far more patients than those operators who cut rather than tear. No observation can be passed on the single instance related by Mr. Crowther, as he has omitted, quite accidentally I should say, to state what was the size of the stone he removed; nor can any remark be made on the Emperor Napoleon's case, as the details are still withheld from the profession.—*Lancet, Nov.* 8, 1873, *p.* 683.

63.—ON SUBCUTANEOUS URETHROTOMY IN STRICTURES OF THE URETHRA.

By **W. F. Teevan**, Esq., B.A., Surgeon to the West London Hospital, and to St. Peter's Hospital.

The operation of subcutaneous urethrotomy is little known or practised in this country, but it is the one which strongly recommends itself to us for adoption whenever an operation may be required. It may be safely said that if a stricture be large enough to admit of the introduction of a dilator or urethrotome, you can cure the complaint by gradual dilatation without the slightest risk to the patient's life. There are, however, a few cases where, although the stricture may be dilated to the fullest extent, it yet so rapidly contracts again that an operation is indicated; and there are also instances of hard, horny strictures, complicated with abscesses and fistulæ, where an operation is called for, as much for the purpose of giving a free vent to suppurating tissues as for the enlargement of the canal to its natural calibre. In the first case, subcutaneous urethrotomy is indicated, but in the latter instances external urethrotomy. The subcutaneous division of strictures, introduced into practice by Dr. Henry Dick, is the only operation which holds out any prospect of a real cure for stricture, as it is the only method that is in unison with the teachings of surgical pathology and experience; it possesses all the advantages of external urethrotomy or perineal section without the disadvantages of an external wound, and it is superior to internal urethrotomy because it always insures a complete division of the strictured parts, which that operation does not.

J. N., a strong healthy sailor, aged 52, came to St. Peter's Hospital, as an out-patient, on June 1, 1872. He stated that he had suffered from strictures for upwards of twenty years, and that eight years ago he had been operated on at an hospital in London, where his stricture were forcibly split, and an instrument regularly passed for some time afterwards. The complaint, however, returned as bad as ever, and one year and a half ago he visited the hospital where he had previously been, and submitted to a repetition of the former operation, a catheter being regularly introduced for some weeks afterwards. In the meantime the patient had learnt to pass the instrument himself, and used it twice every week till he was forced to desist through the occurrence of some heavy weather at sea. After an interval of ten days he again tried to pass the catheter, but failed, as the strictures had in the meantime contracted. When the patient came under my care I could only pass a No. 3 bougie, and that with considerable difficulty. Examination showed that there were two strictures encircling the urethra like iron rings. The distal stricture was half an inch broad and four inches from the orifice; whilst the other one was a quarter of an inch broad and two inches and three-quarters down the urethra. By the end of the month the urethra had been enlarged by gradual dilatation to No. 9; but when the patient came to the hospital, ten days later, the strictures were found to have again contracted, and would only admit No. 4. He was then admitted into the hospital, and a No. 4 elastic catheter was passed into the bladder and left there for an hour, when it was replaced by a larger instrument. By continuing this method of treatment for six hours, the patient's urethra was dilated to a No. 8, when the patient could no longer persevere, on account of the increasing pain. Next day the strictures were again found to have contracted, so that only a small instrument could be introduced. It was now clear that an operation was called for, and accordingly, on July 26, in the presence of Dr. Henry Dick, Dr. Green, and Dr. Magrath, I passed a fine straight staff through the strictures, and completely divided both of them, subcutaneously, with a small tenotome, which I introduced beneath the skin by making a small puncture. The contracted tissues were so dense and hard that their division was attended with a creaking noise plainly audible to those present. A very large catheter was then introduced into the bladder and left there for half an hour. Blood oozed away from the urethra for ten minutes after the operation, but none escaped through the valvular puncture made by the tenotome. The patient's urine was drawn off the same evening and three times the next day. On the third day the use of the catheter was relegated to the patient, who passed it once a day till he left the hospital, on July 31—five days after the operation—not having had a single bad symptom, nor having been confined to bed. The patient came to the hospital on September 2, to report himself before sailing. He was then passing No. 14 elastic catheter with ease twice a week, and was in all respects quite well. The ends of the divided strictures could be plainly felt beneath the skin united by bands of supple and non-indurated tissue.

The strictures were in the first instance the result of a long-continued gleet, and the splitting (lacerating) operations to which this man had been subjected had only given temporary and delusive relief, for each operation had been followed by a more speedy return of the complaint. One point of great interest was that the patient had had his strictures treated thirty years ago on board the *Dreadnought* by the system of gradual dilatation. It would thus appear that the results of that treatment were more abiding than those produced by immediate dilatation, which was in reality complete rupture or laceration. The strictures became converted into traumatic ones by the surgical lacerations to which this patient had been subjected; for such strictures as his, uncomplicated as they were with abscesses or fistulæ, were rarely seen unless produced by accidental or surgical injury. No two operations were so dissimilar in character as forcible dilatation and subcutaneous urethrotomy, for the former was an exhibition of blind force, whilst the latter was a demonstration of applied skill.—*Medical Times and Gazette, Sept.* 27, 1873, *p.* 351.

64.—DESCRIPTION OF A GROOVED CATHETER WITH CONDUCTING BOUGIE, WITH REMARKS ON ITS VALUE.

By F. W. Teevan, Esq., B.A., Surgeon to the West-London Hospital, and to St. Peter's Hospital.

The instrument I am about to describe is one of the most useful for the treatment of impassable stricture, retention of urine from stricture, and for effecting an entry into the bladder in those cases where the difficulty is caused not so much by the narrowness of the canal as its deformity (Delpech), a condition that is but little recognised in this country, and which I may best describe by saying that it means a crookedness of the urethra, caused by constrictions and the dilatations resulting therefrom, and necessarily occurring posteriorly to such strictured points. The idea of sliding one instrument over the other is by no means new, and originated, I believe, with Nauche, in 1816, and was employed by Civiale in 1823. The method has also been well known for a great many years to M. Auguste Mercier. The Americans would seem to be ahead of us on this point, for they have for some time possessed a most beautiful and useful instrument, designed, I believe, by Professor Gouley, of New York. In this country the method of treating stricture by sliding one instrument over another has been prominently brought before the profession by Mr. Thomas Wakley, but has not received that attention which its usefulness deserves. I am informed that the late Mr. Solly employed an elastic catheter open at both ends, which he slid along a catgut bougie, and I have lately seen this plan tried by my colleague, Mr. Walter Coulson; but I consider it open to objection, for the catgut bougie is soon affected by the moisture of the canal, becomes twisted and knotted, and thus makes it difficult, and sometimes impossible, to slide a cathether over it. Apart from the gentlemen whose names I have mentioned, I believe that the method is but little known in this country. I would premise my observations by saying that the whalebone bougie is really the invaluable portion of the compound instrument I am about to describe. It often happens that no silver or elastic catheter can be got into the bladder, and yet a fine bougie may be passed with ease, and the retention relieved. Such a case I have recorded in the Transactions of the Clinical Society (see vol. iv., p. 124). Having succeeded in getting such an instrument into the bladder, there are often reasons which make it desirable or necessary that we should pass another kind of instrument, either for operation or dilatation, and it therefore becomes of the highest importance to utilise the bougie that has been passed into the bladder and this is best done by retaining it *in situ* till we have been enabled to slide a larger instrument over it. I would, indeed, go further, and say that in some cases a fine bougie is the only instrument which can be got into the bladder, and that unless it is utilised we cannot proceed with the treatment of the case. Again, there are cases, complicated with false passages, where no instrument can be introduced except a bougie with some peculiar twist, and where our only chance of progress lies in enlarging the canal by sliding one instrument over another.

The instrument I am in the habit of using differs in some respects from Professor Gouley's, and is, I think, superior to it. I will first describe it, and then show the differences between the two. The conducting bougie is made of whalebone, and is excessively fine, having each end olive-shaped, which prevents the instrument from pricking the mucous membrane or lodging in the lacunæ. The material of which the bougie is composed imparts great elasticity combined with firmness, and thus admirably fits it for getting through very indurated strictures where the entrance is often reduced to a pin's point. Care ought to be taken that the bougie is at least two inches longer than the catheter, otherwise the former may recede insensibly from the bladder as it is held forcibly between the thumb and forefinger of the left hand to resist the tendency which the cathether has to carry the bougie with it as it is sliding over it into the bladder. The catheter is about the size of a No. 1, English, and its end is cut off to permit the bougie to enter the canal and emerge through a slit, which serves as a groove for subcutaneous or external urethromy. The groove is made by cutting out a longitudinal piece of the cathether. Urine will not escape through it, for the mucous membrane is tightly stretched across it, and so completely closes the groove. The three differences between Professor Gouley's instrument and the one I have described, are:—1. Mine is much smaller. 2. The groove is made by cutting a piece out of the catheter, but in Professor Gouley's it is made by driving in the wall of the catheter, thus necessarily diminishing the calibre of the canal. This explains how it is that a smaller instrument can be constructed after my plan than Professor Gouley's. The bougie in my instrument is contained for about one inch and a half in the canal, but in Professor Gouley's it is only passed through a little bridge at the end, not more than one-eighth of an inch broad, and is thus far more likely to become entwined round the catheter than when it passes through a considerable portion of the instrument. Any surgeon can have one of his small catheters converted into the one I have described for an insignificant sum, and so be spared the expense of buying a new instrument. In introducing the bougie into the bladder, it is well to rotate the instrument as we proceed, for by that procedure we avoid all chance of slipping into the lacunæ on the way. It sometimes happens that, although we can pass the bougie through the stricture, we are unable to make the catheter pass along it, on account of the narrowness of the strictured part. In such a case it is well to leave the bougie in the bladder for several hours, and then try again. The compound instrument I have described will be found most useful, (1) for the relief of retention; (2) for the purpose of dilating the stricture; (3) for external urethrotomy; and, lastly, for subcutaneous urethrotomy.—*Lancet, July* 5, 1873, *p.* 5.

65.—ON TWO CASES OF DEATH AFTER HOLT'S OPERATION.

By Dr. R. W. Tibbits, Surgeon to the Bristol Royal Infirmary.

[The two cases related in the following paper throw considerable doubt upon the absolute correctness of Mr. Holt's statement that the use of his instrument is "free from every danger—immediate or remote."]

The first case was that of a healthy-looking sailor, aged 25, who was admitted into No. 11 ward on February 21. Five years ago he had been laid up with a sharp attack of gonorrhœa, and since then had suffered from a gradually increasing stricture. Several unsuccessful attempts had been made by medical men to pass instruments into his bladder; after each attempt he had shivered and felt unwell for one or two days. He was kept in bed for three days, the bowels freely opened with castor-oil, and on the evening of February 24 a full dose of Dover's powder was administered.

February 25. Holt's instrument was easily passed, and the stricture—a slight one—split with the largest-sized dilator. Afterwards No. 11 catheter was introduced into the bladder.

26th. Patient seemed perfectly well. Had not shivered after the operation, *and passed his urine* in a full stream, with slight pain.

27th. Still going on well. No. 10 catheter passed easily, causing little pain.

28th. Twenty-two hours after the passing of No. 10 catheter, and seventy hours after the operation, he was suddenly seized with a severe rigor, which lasted for two hours. He then perspired freely. Countenance extremely anxious-looking. Complained of much pain in his head and back. No tenderness in the perineum or over the abdomen. No albumen or pus in the urine. Nothing to be detected with the stethoscope. Pulse 120; temperature 101·6°.

March 1. Complained of feeling very ill. No recurrence of the rigor. No pain anywhere. Tongue dry and brown. Morning: Pulse 136; temperature 105°. Evening: Pulse 128; temperature 103·6°.

2nd. Evidently much worse. Complained of no pain or tenderness anywhere. Urine scanty, but free from albumen. Nothing to be detected with the stethoscope. Morning: Pulse 136; temperature 104·2°. Evening: Pulse 116; temperature 104°.

3rd. Perspiring freely. Could not be roused sufficiently to understand the questions asked him. Extremities congested. A dark purple petechial-looking rash had appeared over the whole of the body, especially on the chest, where it was very thickly distributed. Tongue dry and brown. Pulse 160; temperature 104·5°.

From this time he gradually passed into a state of coma, and died on the following night at a quarter past one o'clock, the temperature rising to 107·6° fifteen minutes before death.

Post-mortem Examination (thirty hours after death).—Lungs congested posteriorly; extreme staining of inside of heart and bloodvessels; liver healthy; spleen enlarged, and broke down into a pulp when cut; kidneys presented numerous hemorrhagic spots, varying in size between a pin's head and a small pea, many of these showed signs of softening in their centre; bladder healthy, walls rather hypertrophied. Around the seat of stricture, at the junction of the spongy and membranous portions of the urethra, there was a slight extravasation of blood; the mucous membrane over this part was perfectly smooth—there was no apparent rupture of it. The prostatic veins were considerably obstructed, and one of them was blocked with a firm clot for about an inch and a half.

This, gentlemen, is one case. Let me now briefly read you the notes of the second one, which appeared in our post-mortem room yesterday, and which you have all had an opportunity of observing:—

D. S., aged 46, a muscular Irishman, was admitted into No. 9 ward on December 13. He told us that he had only noticed that his urine came in a small stream during the last two years; that on several occasions he had had small-sized instruments passed, and that once or twice he had shivered after their passage, but this was not the rule.

December 14. A No. 5 sound was attempted to be passed, but only for a short time, as the stricture bled at the slightest touch of the instrument.

15th. Had suffered no inconvenience from the attempted passage of the sound. Ordered ten grains of Dover's powder at night and half an ounce of castor oil the next morning.

16th. Holt's instrument was passed with little difficulty through a stricture in the membranous part of the urethra, which was split with the largest-sized dilator; one or two drops of blood following the operation. Ordered thirty minims of tr. opii and two ounces of brandy to be taken in hot water. In the evening he passed his water freely with little pain.

17th. 4 a.m.: Whilst at the closet was seized with a severe rigor, which lasted for about two hours. 9 a.m.: Temperature 102·2°; pulse 120. Ordered brandy and an opiate draught. 12 noon: Temperature 102°; pulse 130, weak; extremities cold and congested. Ordered five grains of carbonate of ammonia every two hours. 2 p.m.: Pulse weaker. Ordered three ounces of brandy in hot water. 5 p.m.: In much the same condition. Brandy to be repeated. 9 p.m.: Temperature 100·2°; pulse 130. Brandy and ammonia to be continued; hot-water bottles to be applied to extremities.

18th. 9 a.m.: Temperature 100°; pulse uncountable. To continue brandy. 2 p.m.: Temperature 101·2°; pulse imperceptible at wrist; extremities mottled purple. Has passed no water during the last twenty-four hours; bladder not distended. Patient perfectly sensible, but restless. Ammonia to be discontinued; half an ounce of brandy every fifteen minutes; to be dry-cupped over loins and to have hot fomentation of infusion of digitalis applied over the abdomen.

From this time his condition changed but little. He passed a small quantity of water at about 6 p.m., and remained sensible to within a short period of his death, which occurred at 3 a.m. on the following morning—sixty-two hours after the operation, and forty-eight after the first rigor.

Post mortem Examination (ten hours after death).—Moderately firm old pleuritic adhesions on both sides of the chest; lungs healthy, but congested at bases; heart and its valves healthy—a small quantity of clot and fluid blood in each cavity,—no clot in the pulmonary artery; intestines and peritoneum healthy; liver to some extent congested, otherwise perfectly healthy; spleen healthy; kidneys healthy, not even congested. The bladder contained about six ounces of urine; walls slightly hypertrophied; mucous membrane healthy, and not congested, except for a patch about the size of sixpence at the posterior part of the neck of the bladder; the tissues under this were also congested for the depth of about the eighth of an inch. The urethra was slightly congested along its whole tract, and at the juncture of the spongy and membranous portions there was a patch of deep congestion, showing the seat of the stricture. There was a slight extravasation of blood into the tissues around this for the depth of about the eighth of an inch. The mucous membrane over this point, examined under the microscope with a two-inch lens, showed no sign of rupture.

It has seldom been my duty to draw your attention to cases of so painful an interest as the two I have just related. Those of you who saw the first patient will recollect what a fine healthy-looking man he was. You will also remember that in the operation-room I mentioned that it was the slightest case of stricture that I had ever performed forcible dilatation upon. It may be said that in this instance the passage of the catheter on the second day after the operation was the cause of the mischief; but then the subsequent passing of a catheter follows as a necessity upon this method of treatment. In the second case no such question can arise. This man, with all the organs of his body healthy, died in sixty-two hours from the undoubted effects of the operation.

There was a certain amount of resemblance between these two cases. In each the first symptom was a severe rigor, coming on many hours after the operation, and in each there was the same dusky congested appearance of the extremities. Very little either was there to be detected during life, and still less in the post-mortem examinations; for I look upon it as probable that the deep-red condition of the inside of the heart and large vessels in the first case was due to a considerable extent to the effects of decomposition. But the hemorrhagic spots in the kidneys of the first patient were exactly similar in appearance to that which you sometimes see in well-marked pyæmia. Moreover, during life the symptoms and appearance of these patients closely resembled what we see in the worst cases of septicæmia following after other operations or injuries which sometimes, but fortunately very rarely, occur in the wards of our Infirmary. Therefore I should put down the cause of death in both instances to blood-poisoning.

The practical lesson I wish you to learn is, that in performing any operation upon the urethra you cannot be too careful. When the patient's history points to the stricture being a bad one, or more especially should he habitually have suffered from rigors after the passing or attempted passing of an instrument, let me urge upon you the advisability of insisting upon at least twenty-four hours' complete rest before you attempt to do anything. It has always been my rule to do this; and for two reasons—firstly, you will find it much easier to pass an instrument through a bad stricture if your patient has been kept quiet for some time; secondly, I believe that previous rest often obviates the occurrence of subsequent unpleasant symptoms, for I have more than once found that patients who had always suffered inconvenience from the passage of an instrument would stand considerable manipulation with impunity, previous rest having been insisted upon. At any rate, should mischief result, you will have the satisfaction of knowing that all rational means had been taken to prevent it.

Holt's operation is one you have seen me perform moderately frequently, but never without having kept the patient in bed for at least forty-eight hours.

Mr. Holt claims for his operation that cases of stricture treated by this method show less tendency to contract than after any other plan of treatment. My experience relates only to strictures situated in the neighbourhood of the membranous part of the urethra; but it is decidedly in favour of this assertion. At the same time, as a rule, unless a full-sized instrument is afterwards passed with tolerable regularity, you are running your patient to unnecessary risk; for to a certainty will the diseased tissues again contract.

There is one more point of very special interest that these cases help to demonstrate. I mean what actually occurs when you rapidly force the dilator through the stricture. Mr. Holt advances the proposition that the diseased submucous tissue is alone split, and that the mucous membrane is simply dilated. This is a matter of considerable importance, because, with any considerable wound of the mucous membrane of the urethra, there would be a risk of infiltration of urine being the result, though I believe this risk would be slight. We particularly examined both these cases, with the view of obtaining information on this point, and undoubtedly they go towards proving the correctness of Mr. Holt's supposition; but we must bear in mind that during the time these patients lived after the operation a certain amount of rupture might have healed. Still, they certainly seem to prove that, at any rate when the stricture is not a bad one, the injury, if any, that is done to the mucous membrane is of the slightest.

With these two cases before us, it may perhaps suggest itself to the minds of some of you to ask—Why continue to perform the operation? You cannot learn too early that cases of stricture, as a class, are some of the most troublesome and difficult that you will have to deal with, and also that no method of treatment is perfectly free from danger, for death has more than once resulted from the simple passing of a catheter. There are also persons who will not submit to the tedious treatment by gradual dilatation. Specially have I found this amongst the lower classes, with whom time is of importance. These people will remain under treatment till the stricture is partially relieved, and then cease to attend, to come back again in a few months' time worse than ever. But even under these circumstances you would not be justified in performing Holt's operation, if the risk to life from it is considerable, unless the advantages to be derived from it are far more considerable. But what is the increased amount of risk? Sir H. Thompson, in his lectures on "Diseases of the Urinary Organs," lays no stress upon this point, neither is it referred to by many of our best surgical authorities. Moreover, the results published by Mr. Holt are extremely favourable; therefore, we have a right to assume that the cases we have this morning considered are very exceptional. My experience of the advantages of the operation in successful cases is decidedly satisfactory; but unless the patient afterwards submits to the periodical passing of a full-sized instrument, the stricture will again to a certainty contract, and the operation will have been useless.

In treating all ordinary cases of stricture let me advise you, as a rule, to adopt first of all the simplest method at your disposal, and fairly try the plan of gradual dilatation, reserving Holt's operation for the more severe cases, or for those patients who will not submit to lengthened treatment. Remember, also, that there are certain persons whose constitutions resent any interference with their urethra, and who will tell you that they nearly always suffer from a rigor after any attempt to pass a catheter. It will behove you in such cases to be especially careful.—*Medical Times and Gazette, Aug.* 2, 1873, *p.* 121.

66.—ON STRICTURE TREATED BY MR. F. JORDAN'S METHOD PER RECTUM.

By L. Stromeyer Little, Esq., late Surgeon to the London Hospital.

A Portuguese seaman, aged 23, was admitted into the Shanghai General Hospital, suffering from stricture of the urethra. He stated that for more than a year he had had difficulty in making water, and that for some months he had only been able to do so by drops. He had often had gonorrhœa. He was a healthy muscular man; when examined, a large instrument passed easily about two inches and a half down the urethra and then came to a dead stop. The same occurred with all the various kinds and sizes of elastic and metal bougies. He passed water at the rate of a drachm a minute. He was kept quiet for some days, and then small instruments were tried through a Stromeyer guide catheter, but without success. The urethra, from the point where the stricture commenced down to the bulb, was very hard, but not much enlarged externally. Endoscopic examination afforded only negative results. When the patient had been three weeks in the hospital without any prospect of cure without operation, I determined to open the urethra from the rectum on Mr. Furneaux Jordan's plan, and either pass a bougie forwards from the wound in the rectum, thereby dilating the stricture, or by causing all the urine to pass per rectum for a time, give the strictured urethra rest, and then attempt its dilatation. The patient, after having been made to retain his urine for eight hours, was placed in the lithotomy position under chloroform, and, with the left forefinger in the rectum for a guide, the urethra was opened in front of the prostate to the extent of half an inch at least. An elastic *bougie à boule* was now passed from the wound towards the meatus, and although its point could be felt in the urethra in front of the scrotum it could not be passed further. Other kinds and sizes of bougies were tried with a like result. A large elastic catheter was now passed into the bladder from the rectum, and tied *in situ*, and forty drops of laudanum were given by the mouth.

The patient removed the catheter during the following night, but it was easily replaced in the morning. He did not suffer in any way from the operation. The catheter was retained three days, and on the fourth day, as the patient complained of passing urine by the penis, an attempt was made to pass a bougie through the penis, but without success. Three days later, however, a No. 1 elastic bougie was passed through and left in the stricture. This, after eight hours, was replaced by a larger one. From this date, every morning and evening the bougie was changed for a larger one, until No. 12 was passed. Up to this time the urine flowed entirely per rectum. As the stricture was now sufficiently dilated, a No. 12 catheter was passed every eight hours into the bladder and all the urine drawn off, so as to discourage any escape by the rectum. This treatment was continued for five days, when, on the morning of the fifth, the patient being unwilling or unable to hold his water until the catheter was passed, voided it naturally.

Examination with the speculum showed that the wound in the rectum was healed. The patient was discharged from the wards, provided with a bougie to keep his stricture dilated. One month later, he presented himself in all respects sound.

Cases of stricture are common among the sailors in this hospital, and are usually treated by gradual dilatation; but as this case presented difficulties which could not be overcome without operation, and as the operation selected is still a novel one, the case is recorded. The operation did not seem to me quite so easy as it is described; but it is, doubtless, a safe way of opening the urethra without a guide, and very much simpler and less severe than by the perineum. Mr. Jordan recommends operating through a speculum, but I had not sufficient confidence in being able—with the speculum—to keep the middle line. In this respect, therefore, I departed from Mr. Jordan's valuable proceeding, discarded the speculum, and trusted to the forefinger as a guide. The subject of this case being a young man, the sphincter ani was very tight, and closed firmly on the finger and knife, hindering their movements. It would do no harm to divide the sphincter muscle, and such a proceeding would certainly make the operation easier. The operation seems suitable to many strictures which are not impermeable, for when the urine has not to pass over the stricture, dilatation can proceed much more rapidly, with less pain, with less inflammation, and, consequently, with less constitutional disturbance.—*Medical Times and Gazette, Sept.* 20, 1873, *p.* 230.

67.—CASE OF RETENTION, FOLLOWED BY SUPPRESSION OF URINE, LASTING SEVEN DAYS—RECOVERY—OBSERVATIONS.

By Dr. SAMUEL T. KNAGGS, Hon. Surgeon Newcastle Hospital, N.S.W., and Assistant-Surgeon, N.S.W. Naval Forces.

J. G., shoemaker, aged 45 years, living in a suburb near the city of Newcastle. He had been drinking for six or eight weeks. On Sunday, 31st March, 1872, he found himself unable to micturate, but did not suffer any inconvenience until the Tuesday evening following, when at seven p.m. I was hurriedly requested to see him, the case being described as a most urgent one. I found him lying on his back in bed, suffering great distress; pulse 90; respiration hurried; the skin moist and clammy; countenance pale and anxious. He was complaining of great distention in the region of the bladder. He had been vainly endeavouring to urinate, and he described the agony that he suffered as being unbearable. I at once put him into a warm hip bath, which seemed to relieve the most urgent symptoms; while there I made him try to pass water, but the endeavour was fruitless. I then attempted catheterism, first trying most cautiously to insinuate No. 9 gum elastic catheter. It passed as far as the prostate gland without obstruction, and there it stopped. All the usual manœuvres, such as inserting the finger into the rectum, withdrawing the stilette (Hey), depressing the hand, &c., were tried fruitlessly. After a most patient trial, I likewise failed to pass No. 6, 4, and 2. The warm bath, however, seemed to cause an abatement of the most urgent symptoms. At nine p.m., I again saw the patient, in consultation with Dr. Harris. Though somewhat distressed with the feeling of distension about the cystic region, the more urgent symptoms had not returned. The cystic dulness reached almost up to the umbilicus, the prostate gland was found to be very much enlarged, and the cystic tumour was plainly perceptible through the anterior wall of the rectum. Catheterism again failed. We decided upon trying to induce a free action of the bowels; in the event of this failing, the propriety of operative interference could be considered next day. Urgent symptoms, if arising during the night, to be treated by hot stupes and warm baths.

Twenty grains of calomel were at once given in four pills, to be followed hourly by two-ounce doses of black draught until the bowels should be moved.

Fourth day: Wednesday, 3rd April. I visited the patient at day-light, and found that the pills remained down, but every attempt to take any of the draughts was frustrated by incessant vomiting. The patient had been up several times during the night in great pain, unavailingly endeavouring to pass water. After each fruitless effort he found considerable relief by remaining half an hour in the warm bath. Upon examination I found that the cystic tumour had considerably subsided. He complained less of the feeling of distension than he had done the day before. I made a careful examination of the bedding and clothing, but could detect no evidences of his having unconsciously passed water. Symptoms of uræmic poisoning appeared to be setting in, the pupils becoming contracted, an inclination to drowsiness, slight wandering of the mind, and great irritability of the stomach. At ten a.m., according to appointment, I again met Dr. Harris in consultation with reference to the case. By this time the symptoms of uræmia had become more decided. The patient could with difficulty be roused from the comatose state into which he had fallen. There was incessant vomiting of a greenish watery fluid in copious quantities, having a fœtid urinous odour. The skin was moist and clammy; also exhaling an odour similar to the vomit.

We decided that the time for operation had passed, the abdominal tumour having all but subsided. Catheterism again failed. It being considered very desirable to produce free action of the bowels, croton oil, in two-minim doses, was ordered every hour until the bowels should be moved, the oil to be placed upon the tongue. This caused such an aggravation of the vomiting and thirst that it had to be discontinued after the second dose. In the afternoon the patient was still stupid and comatose, yet could be aroused on his attention being attracted. He said that he had had a beautifully quiet sleep, and now had no pain *in the bladder. Catheterism* was again tried while he was in a warm bath, but failed. He expressed himself to be greatly relieved every time he got the bath, but in this instance he fainted on getting out of it to go to bed. I now made what I feared would be the last attempt to evacuate the bowels, administering at once a turpentine injection, but his weak state was such that it caused no action of the bowels. I accordingly followed it up with about two pints of warm water, which so distended the bowels that it was almost instantly ejected. Half an hour afterwards he was relieved of a fair solid motion, but he fainted on getting off the stool. This so dejected him that he settled his worldly affairs, having a firm conviction on his mind that he could not possibly outlive the night.

Fifth day: Thursday, 4th April. Patient very weak and low spirited; slept during the night, but wandered greatly in his mind; excessive watery vomiting, profuse clammy perspirations, the excretions having all the same urinous odour. He has not had a morsel of food since last Saturday, and has not passed water since Saturday morning. He has a firm conviction on his mind of approaching death, and says that he will surely die this evening. He is so weak that the least effort produces fainting. There is an inclination to stupor, no actual pain, and the bladder is flaccid.

Sixth day: Friday, 5th April. I tried to induce him to consent to go to the hospital, but his friends objected, preferring him to die amongst them. His appearance much the same as yesterday, but he was much weaker, and hardly able to raise his hand. Another enema was administered, which returned stained greenish black. He said that this relieved him greatly, but the increased vomiting, cold perspirations, and stupor appeared worse.

Seventh day: Saturday, 6th April. Had a quiet night. Begged to be left quiet on his back, as each movement produced excessive retching. At two p.m. injected per rectum a pint of strong beef tea and two glasses of brandy. He felt almost immediately much stronger. To be continued every four hours.

At seven p.m. he passed a chamberful of water, giving a strong alkaline reaction, after which there was less inclination to vomit. I ordered a draught, containing two grains of oxalate of cerium, one minim of creosote, two minims of dilute hydrocyanic acid, and two drachms of mucilage of gum arabic, with water, to be taken every hour.

The above draught was designed to allay the vomiting and irritability of the stomach. I again saw the patient that night, when he expressed himself as feeling like a "new man." He rapidly improved from this out, and in a week was able to walk about.

Observations.—This case in itself is remarkable, from the fact that the patient, a broken-down, debilitated subject, survived seven days without passing a drop of water from his urinary bladder, and ultimately recovered. It possesses a further interest, from the fact that nature came to the rescue of the physician, and indicated a rational procedure in the treatment. The gastro-intestinal tract and the skin took on vicarious action, as was indicated by the copious watery vomitings and profuse perspirations exhaling a peculiar urinous odour. These organs (stomach, intestines, and skin) voluntarily gave their assistance to the kidneys, and performed their functions while their (the kidneys) portals were stopped, and thus relieved the blood of urea and such effete products as would have accumulated in the system, and have literally poisoned the patient.

The mechanism by which the bladder was relieved of its contents on the fourth day can, I think, be fairly explained, as well as illustrated, by our physiological researches in the study of Asiatic cholera. In Asiatic cholera a prominent symptom is the copious rice water evacuations by mouth and rectum; these are thrown out in such vast quantities and so greatly exceed the ingesta, as thereby to excite surprise as to where the fluid all comes from. Actual experiment has proved that several changes take place in the blood which supplies this fluid. Being deprived successively of its water, its salts, and albumen, then the properties of the corpuscles become changed, and they in turn *rob the tissues through which they permeate of their watery constituents.* There is no obstacle to our assuming that a somewhat analogous change took place in the blood of the patient whose case is under consideration. The gastro-intestinal tract and skin, by energetically taking on vicarious action, deprived the blood of water, urea, chloride of sodium, as well as other effete matters.

The blood having become physically changed in its properties, the corpuscles absorbed the necessary watery constituents from the tissues; the bladderful of fluid in its turn became reservoir, and, for the time being, recouped the tissues with the necessary moisture by yielding up its contents. Thus can be explained why, though the bladder does not physiologically possess absorbent properties, the absorption of its contents actually took place in this instance.

This case I deem a most instructive one, and if the lesson which it inculcates be carefully studied, and the hints which it suggests be followed out in a spirit of scientific inquiry, it cannot but be productive of much good, and materially modify our treatment of urinary retention and suppression.—*Dublin Journal of Medical Science, July* 1873, *p.* 27.

68.—ON THE DETECTION AND REMOVAL OF VESICAL CALCULI BY A NEW METHOD.

By William Donald Napier, Esq.

[At present stone in the bladder is discovered by sound. Mr. Napier, by an ingenious contrivance, proposes to detect it by sight. Not sight of the stone, but of the impression it makes upon the end of his instrument. There is no doubt that stones sometimes escape detection—probably from their surface being friable and soft, and not producing a sound when struck. The instrument proposed, the author says, "will, I hope, render a doubtful result more exceptional than it has ever been."]

Unlike any instrument ever applied to the purpose before, the discovery of the calculus is dependent upon ocular instead of aural proofs,—the outer substance being so peculiarly sensitive and readily impressed by contact with any hard particle, however minute, that evidence of the existence of calcareous matter is obtained, on its withdrawal from the bladder, by the impressions resembling slight scratches that are found on its surface. The black pigment with which the instrument is partially coated is produced by its momentary immersion in a weak solution of nitrate of silver (about one drachm to half a pint of water), with which also the marks it may receive in use must be obliterated. After the application of the solution it should be washed and allowed to dry, and when not in actual use should be enclosed in cotton wool, for the avoidance of any accidental impressions that contact with grit or dust would inevitably cause. As the principle upon which the instrument is constructed renders its success dependent upon the perfection of its outer surface, it may with advantage be examined through a magnifying glass both immediately before and after insertion, that the smallest blemish or indenture should not pass unnoticed. It is called the Calculus Detector.

It is unnecessary for me to enlarge upon the obstacles that beset the path of the operator who desires to perform with skill and delicacy, what is, I think, generally allowed to be one of the severest tests of the surgeon's art, and, when attended with success, one of his most assured triumphs; nor will I do so more than is necessary to my present purpose, which is to point out the more obvious complications that are at times consequent upon the operation of lithotrity, with a view to considering whether their number and gravity might not be diminished, could certain conditions be insured which mechanical art has as yet failed to accomplish.

The performance of lithotrity under favourable circumstances implies the breaking and comminuting of a vesical calculus, so that its fragments may be discharged with the urine; but unless this standard of perfect manipulation is attained in the first instance, difficulties arise immediately on the dispersion in the bladder of fragments too large to find an exit in a natural manner, and the distress that is caused by their presence entails either a premature repetition of the first operation, which is undesirable, or the portions of calculus must be separately grasped with a small lithotrite and drawn away,—a process which causes intense pain to the patient at the time, is constantly followed by protracted suffering consequent on abrasion of the mucous membrane, and not unfrequently results in the production of permanent stricture.

It was when having regard to these various impediments to recovery, whose origin seemed to be attributable to the after treatment rather than to the primary operation, that it suggested itself to me that an instrument might be constructed which by its form and substance should guard the passages from laceration during the withdrawal of the crushed fragment or fragments of stone from the bladder. After eight years of careful consideration of these requirements, I have had an instrument constructed. It is a soft india-rubber tube, terminating at one extremity in a funnel-shaped orifice, greatly resembling in form the ordinary convolvulus flower, and capable of containing a calculus, or portions of calculus of considerable dimensions. Inserted into the bladder, its form and position render it the receptacle of such foreign substances as would, from the position of the patient and flow of urine, determine towards the natural outlet. This receptacle, enclosing such bodies in the folds of its delicate membrane, and permitting itself to be gently withdrawn, retains them in its elastic envelope, and protects the urethra from their direct contact.

There is a silver canula, enclosed in which the india-rubber extractor is introduced into the bladder furnished with a cocoa-butter point, which closes its orifice at the same time that it facilitates its ingress by its lubricating properties. The instrument is shown ready for the hand of the operator, who, after inserting it in the same manner that he would an ordinary catheter, ejects the india-rubber tube and cocoa-butter point at the same time that he draws away the canula. The cocoa-butter point dissolves readily in about three minutes.—*Medical Times and Gazette, June* 14, 1873, *p.* 623.

69.—ON THE TREATMENT OF PROSTATIC RETENTION OF URINE.

By Jonathan Hutchinson, Esq., Senior Surgeon to the London Hospital, Surgeon to the Moorfields Ophthalmic Hospital, and to the Hospital for Skin Diseases.

[The object of this paper is to draw attention to some modifications in the instruments for the treatment of prostatic cases.]

The first which I will allude to is the catheter which I now show. It is of metal, of large size, and with a long curve; and differs only from that which since the time of Sir Everard Home has been known as the "prostate catheter" in the position and size of its eye. Instead of there being two eyes, one on each side, there is a single large one placed in front, at a short distance from the end. The object of this is to permit the introduction of a piece of india-rubber tubing through the catheter into the bladder. Latterly I have used for this purpose not india-rubber tubing, but an india-rubber catheter; having found a very easy method of securing the latter in. Some years ago, having experienced great difficulty in retaining flexible instruments in the bladder, it occurred to me that if I could pass a piece of india-rubber tubing and leave it with six or eight inches coiled up in the cavity of the viscus it would probably be self-retained. We found on trial, in several cases, that such was generally the fact, but not invariably, for sometimes an irritable bladder would succeed in expelling the tube, however flexible it might be, and however long might be the portion which we had introduced. We found also that the tube caused more or less of cystic irritation; and thus, though the plan seemed a decided improvement in all others for certain cases, it yet had its drawbacks. It became a problem, on the one hand, to shorten the tube as much as possible, and, on the other, to prevent its accidental expulsion. After trial of several contrivances, we succeeded in completely accomplishing the latter half of our task by means of the simple apparatus which I now show, and which most of you have, during the last year, repeatedly seen in use in the wards. It consists in principle of a stiletted plug, the stilette being about an inch and a half long, and serving the purpose of preventing the catheter from bending at the meatus. If an india rubber catheter is made stiff for a very short distance—say an inch or an inch and a half—in the first part of the urethra, and if its end be secured to the penis by tapes, the bladder cannot expel it. We have now never the slightest difficulty in retaining these catheters as long as we like, and it does not matter what size we introduce or how soft and flexible the material is of which they are made. All that you have to do after having introduced the catheter is to put a little metal nozzle with rings into its orifice. This nozzle does not pass down more than three-fourths of an inch, and it is fitted with the stiletted plug which accomplishes the end referred to. The nozzle, having being passed into the meatus, is fastened firmly to the penis by means of tapes and plaster. Take care that it is fixed pretty tightly and with good plaster, and instruct your patient never to take the plug out excepting when he wishes to void urine. If these conditions are fulfilled, you need not be under the slightest anxiety as to the catheter coming out. Your patient may leave his bed, may walk, sit, or go about his ordinary affairs without any inconvenience. I have in several instances had old gentlemen who did this, and who assured me that they were scarcely conscious of the presence of the instrument. You will see that with this stiletted plug the catheter may be shortened as much as you like. The means by which it is retained is in the first inch of the urethra and not within the bladder, and if the catheter be long enough to reach the urine, that is all that is necessary. I have told you that my first idea was to secure retention of the india-rubber tube by letting it coil itself up in the bladder, but that plan has of course been wholly abandoned since the adoption of the stiletted plug. It is right that I should here explain that other surgeons have devised methods of securing india-rubber catheters in place. Thus Mr. Holt has invented an ingenious one with wings, and Sir Henry Thompson has published the description of one which, as regards the prevention of the bending of the tube at the meatus, is the same in principle as that which I recommend. His consists in the introduction within the flexible catheter of a thin German-silver tube four or five inches long. This has to be fitted in place by the maker, and as mine can be changed at a moment's notice and adapted to any catheter, I think I may claim for it considerable practical advantages. As regards the contrivance of "wings" as means of retention, my experience has been that they are not effectual, whilst they certainly render the introduction of the catheter more difficult.

Having thus explained the means by which the india-rubber is kept in place, I must next return to the question of its introduction. Whilst I was in the habit of using tubing, I was accustomed to pass it down through the catheter which I have shown; and having introduced so much that at least fifteen inches were fairly within the bladder, we could then withdraw the silver instrument, together with the portion of tubing still within it. This done, the tubing was cut through close to the meatus, and the stiletted plug fitted to it. A far simpler way than this, however, is to take a catheter, or piece of tube of the proper length, pass it through the metal instrument or conductor, and then with a sort of ramrod push it down whilst in the act of withdrawing the latter. You may use as ramrod a common flexible gum bougie; but I prefer the metal one which I now show. You will, of course, take care not to push it quite into the bladder—an accident which may be rendered impossible either by using only a short ramrod or by being careful to withdraw the conducting-tube at the same time that you push down the india-rubber one.

I am very sanguine that by the use of the simple means which I have described, we may take a final leave of most of the troubles and distresses for practitioner and patient which have hitherto been so common in connection with prostatic retention of urine. At any rate, I think we shall be able to manage, with vastly increased comfort both to ourselves and to our patients, all cases in which the patient is seen *early*. If the retention be allowed to persist for several days, and the bladder has been greatly distended, and the urine thrown back on the kidneys, secondary conditions of disease may have been set up which will progress in spite of any subsequent treatment. For all early cases, however, the introduction (and retention if necessary) of an india-rubber catheter is such a simple and safe measure that I confidently predict that it will *supersede all others.* *Here* let me state, however, that I do not recommend the retention even of the softest india-rubber unless it is necessary. The necessity for such retention must be measured by the degree of difficulty in its introduction. If difficulty is encountered, if it is necessary to use a silver instrument, and if more or less of pain be caused to the patient at each introduction, ill consequences are sure to follow sooner or later. Although I by no means allege that a piece of india-rubber retained is in all cases innocuous as regards irritation of the urethra, yet I am sure that under circumstances such as those alluded to it is by far the lesser evil. Indeed, I entertain a great dislike to repeated introductions of silver instruments in prostate cases, even when they are most easily passed. It is true that many patients bear their use three times daily without detriment; but in many others irritation of the bladder, hæmaturia, and cystitis are sooner or later the results. Cases of prostatic retention of urine have in the past not unfrequently ended fatally; indeed, it is not many years since one of the foremost members of our own profession died in this metropolis within about ten days of his first symptoms. It is scarcely necessary that I should enlarge upon the value of this plan of treatment in exceptionally difficult cases, and when the patient's residence is at a distance from that of his surgeon. It was, indeed, from my experience of difficult cases at a great distance from home that the importance of perfecting some method for the introduction and retention of flexible instruments became impressed upon my mind. It must have happened to most surgeons of experience to be placed in this position, and to have succeeded in relieving a patient under circumstances which induced the utmost reluctance to withdraw an instrument which had, after so much difficulty, at length been got in. I would strongly recommend that all silver prostate catheters be made on the pattern which I have shown. They are just as useful as others for ordinary purposes, and they supply the means of introducing india-rubber whenever, from the difficulty which has been encountered, it may be thought advisable to retain a tube. The retention of a silver catheter is inconvenient, painful, and even dangerous, yet how often has it happened that the surgeon has been driven to this expedient in the fear that, if he took it out, he might, on the next occasion, fail to introduce it.

And now, gentlemen, I have to advert to another point in the management of this class of cases of yet greater importance than the contrivances which I have been describing. Although, probably, it is well known to some members of the profession, I am sure that it is not a matter of general knowledge, that *in almost all cases of prostatic retention a flexible india-rubber catheter without any stilette can be passed into the bladder.* The india-rubber which I use is very soft, and so flexible that, as you see, you may tie knots in it. Yet the cases are very exceptional in which it cannot be got in, and it will in many cases pass readily when the use of a stiff instrument is difficult. The explanation of this is that it can follow the windings of the distorted urethra. In prostatic cases the urethra is not narrowed, but is merely lengthened and pouched and bent by the projecting lobes of the enlarged gland. The india-rubber is just stiff enough to permit of its being pushed down the urethra when there is no impediment, and it is flexible enough to allow of its following any windings, and mounting over any projections that it may encounter. In using it without a stilette there is of course no scope for either skill or dexterity. All that you have to do is to oil it well and then push it in inch by inch. If it stops, withdraw it a little and then try again. In most cases the patient can do it just as well as the surgeon. It causes no pain, and scarcely ever produces bleeding. Its introduction is indeed so simple and easy that it may be repeated two or three times in the day without any risk, and thus you may avoid the necessity of retaining it. I have not the least hesitation in asserting that the time-honoured silver catheter ought at once to be superseded by the india-rubber one, and that no one ought ever to think of trying metal or even gum-elastic unless he has failed with one of those which I now recommend.* It will also in the future, I hope, only be in very exceptional cases that the means which I have recommended in the first part of my lecture will be found necessary. I have myself of late scarcely ever failed to get the india-rubber in, and in a few in which I have been unable to do so the conducting silver catheter has served an excellent purpose. Nor is it only in prostatic cases that you will find india-rubber catheters useful. They are very valuable for exploration of the urethra in cases of suspected stricture, since they cause the patient less pain than any other kind. They will prove useful, also, for retention in cases of ruptured urethra, and perhaps in cases of paraplegic incontinence.

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* These catheters are not vulcanised, but are perfectly smooth on the surface. They are much more pliable than those which have until lately been in use. They are made by Mr. Warner, of Tottenham. They may be procured of Messrs. Krohne & Sesemann, Duke Street, Manchester Square, and probably of most instrument makers.

Now and then, even in cases of stricture, in which the canal is irregular, and the stricture perhaps in part due to spasm, you will find unexpectedly that a small-sized india-rubber instrument can be got in. I have succeeded with them in one or two cases of this kind greatly to my astonishment, even after some difficulty had been encountered with metal.—*Lancet, June* 7, 1873, *p.* 797.

AFFECTIONS OF THE EYE.

70.—ON A NEW METHOD OF EXTRACTING CATARACT BY PERIPHERAL SECTION OF THE IRIS, WITHOUT INVADING THE PUPIL.

By Dr. CHARLES BELL TAYLOR, Surgeon to the Nottingham and Midland Eye Infirmary.

[However much a previous iridectomy may facilitate the extraction of a cataractous lens, and may obviate the tendency to inflammatory action subsequently, the cosmetic defect resulting from the sacrifice of a portion of iris is so considerable as to counterbalance its advantages.]

Before the adoption of Von Graefe's, Mooren's, my own, and other modern methods of operating, prolapse of the iris was always considered the bugbear of extraction. We have got rid of this danger by the sacrifice of a segment of iris, and the question that presented itself to my mind was, whether the same end might not be attained at less cost by merely removing a minute portion of the periphery of that membrane, so as to obviate the risk of prolapse during the few days following the operation, and yet, in the end, maintain the pupil in all its integrity, and secure the chief advantage of the old flap method without its principal attendant danger. This end I have attained by excising a small piece of the outer edge of the iris, and through the aperture thus obtained, separating it with very fine scissors from its marginal attachments to an extent corresponding to the size of the wound required for the easy exit of the lens.

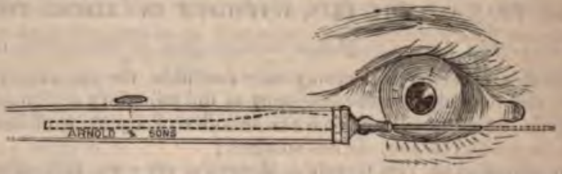
The sphincter of the pupil is thus not stretched by the passage of the cataract which glides from its posterior surface; the iris is not drawn out of the wound during the excision of the minute portion it is necessary to remove. The wound is so small that there is no bleeding from the iris to obscure the future steps of the operation, and the portion of iris corresponding to the wound so frequently the initial seat of fatal inflammatory mischief escapes the bruising which Von Graefe's researches have shown was so frequently the cause of disaster in Dariell's method.

I find also, as a matter of experience, that a slight wrinkling, to be seen only when sought for, is frequently the sole change to be observed in the margin of the iris after the recovery of the patient, while, as a result of the procedure, the pupil is maintained intact, central, and moveable. I have in some cases endeavoured to facilitate the first part of the operation by the preliminary application of the calabar bean, and the adoption of a minute section in the corneo-sclerotic junction at the summit of the globe, as also by occasionally performing the minute iridectomy six weeks before the extraction. These precautions, *however, may generally* be dispensed with, and the section *completed at once, either by the small knife* which I commonly use for extraction, or Von Graefe's. The capsule may be lacerated either through the slit in the iris or the natural pupil.

Since devising this method I have operated in the manner described upon thirty-five cases. In two of these all chance of vision has been lost; three had iritis with closed pupil, and in three others, owing to prolapse of the vitreous, the marginal pupil was larger than I could have wished it. In the majority, however, the result was very brilliant, and quite equal to the most successful flap extractions. The operation is somewhat tedious, but may be easily enough performed with care—a fact which, through the kindness of Mr. Bader, the distinguished ophthalmic surgeon of Guy's, I have been enabled to emonstrate on more than one occasion to the students of that hospital.—*Medical Press and Circular, April* 2, 1873, *p.* 285.

71.—INSTRUMENT FOR SLITTING THE PUNCTUM LACRYMALE AND CANALICULUS.

Mr. **Greenslade** has recently invented a little instrument which will certainly prove very useful in ophthalmic surgery. It is manufactured by Messrs. Arnold and Sons. It consists of, as will be seen in the adjoining woodcut, a canaliculus director, and knife, combined in one handle, thus permitting the operation of opening the duct to be performed with one hand, whilst with the other the surgeon depresses the eyelid. The knife is so constructed as to be hidden in the handle, and with one finger can readily be slid along the director when in the punctum.



By this simple little contrivance, it will be seen that all danger is obviated of wounding other structures than those required to be divided in opening the duct—an accident hitherto liable to occur with an unruly patient.—*British Med. Journal, June* 14, 1873, *p.* 673.

72.—ON STRYCHNIA IN AMAUROSIS.

By Dr. **Julian J. Chisholm**, Clinical Professor of Ophthalmic and Aural Surgery in the University of Maryland, U. S.

M. J. H., of Kentucky, aged forty, has been under medical treatment for four years, for progressive optic-nerve atrophy, both eyes being equally defective. He had been in the habit of using tobacco and whiskey freely, and, from the great nervous irritability exhibited after an over-indulgence in tobacco, his physician had very properly considered this the cause of his defective vision. He was advised by him to abstain from the use of both tobacco and whiskey, which he acquiesced in. Medical treatment was then instituted, and modified from time to time, but with no good results, vision remaining very cloudy. At the end of the four years of treatment and abstinence he had lost, rather than *gained, ground.* At this time he applied for treatment at the Baltimore Eye *and Ear Institute.* I found his strength of vision, with either eye, No. 16 of *Jaeger's test-type* at one foot. This type corresponds in size with the heavy type on the title-pages of books. An ophthalmoscopic examination of the vitreous chamber gave clear media, an apparently healthy retina, with retinal vessels of good size, but the optic nerve-disks were white.

As this was one of a class of diseases, for the correction of which experience had taught me the value of strychnia, I determined to bring this patient under its stimulating influence without delay, notwithstanding his statement that he had already tried the remedy, and had found no benefit from its use. For safety I commenced with the dose that is usually prescribed by physicians, viz., one-thirtieth of a grain of the sulphate of strychnia; but unlike the majority of the profession, who seldom exceed this amount, and therefore get no good results from its administration, I only used the one-thirtieth of a grain, as a minimum dose, to guard against idiosyncrasies. As this quantity was well borne, it was increased by small additions from day to day, until I reached the largest amount the system would tolerate. In referring to my note-book, I find the increase in this particular case as follows: $\frac{1}{30}$ gr., $\frac{1}{24}$ gr., $\frac{1}{20}$ gr., $\frac{1}{15}$ gr., $\frac{1}{10}$ gr., $\frac{1}{8}$ gr., $\frac{1}{7}$ gr., $\frac{1}{6}$ gr., and $\frac{1}{5}$ gr., the interval of time required by the system to tolerate the largest dose, from the commencement of the treatment, covering a space of two weeks. These doses were given three times a day until one-tenth of a grain was reached. With the larger dose the night-dose was badly borne, causing too much excitement of the nerve-centres, accompanied with very uncomfortable stiffening of the leg and spine muscles. It was found that the large doses could be taken with no discomfort in the morning, but not in the evening, on account of the slow elimination of the drug, so that by active absorption the night-dose was added to that portion of the morning-doses still in the circulation. This night-dose was, on this account, always made the smaller dose, viz.: when one-fifth of a grain could be taken after breakfast and dinner without much discomfort, one-tenth of a grain was all that the system would tolerate after supper. Under this course of treatment, keeping the patient constantly excited by increasing doses of strychnia, a marked improvement in vision showed itself even within the two weeks. The patient then returned to his distant home, with instructions to keep up the remedy in full doses. An appreciable stimulation was daily experienced from one-half of a grain of strychnia a day, when continued for even five weeks, and the improvement in vision, although slow, was steady. By this time the system had so fully tolerated the remedy that the usual stimulation evidenced by the toning up of the muscles was no longer experienced by the one-half of a grain, and the improvement in vision seemed to be arrested. For three weeks the one-half of a grain was continued daily, but no progress detected. The dose was now increased to seventeen-thirtieths of a grain per day, when the stimulating effects were again renewed, and within twenty-four hours there was an appreciable improvement in the sight. The patient has, at this time, been taking these large doses of strychnia for eleven weeks. In general health he is stronger and more vigorous than for many years. Now his sight has so much improved that he is beginning to read ordinary print, and will soon be ready to resume active business. This case, one taken from a very large number of cases, is reported in order to show why it is that strychnia, the chief of nerve-stimuli, so often fails to sustain its well-deserved reputation, because administered in doses too small to be of any service. When given judiciously, in doses as large as the system will tolerate, constantly increasing the dose with the tolerance, so as to induce continually an appreciative excitement, good results may always be expected from strychnia in cases of deficiency of nerve-power. The size of the dose must be regulated by the stimulating effect, and must be increased as this stimulation diminishes under the remedy. So far I have had no occasion to exceed one-fourth of a grain of the sulphate of strychnia at a dose, although a case may yet show itself in which this large quantity may fail to produce the necessary nerve-stimulation, under which circumstances I would not hesitate to increase the amount at each dose. After many careful experiments, I have found no appreciable difference between the hypodermic and mouth administration, and now give the medicine in the form of sugar-coated granules, to conceal the bitter taste.—*New-York Med. Journal*, *Feb.* 1873, *p.* 187.

73.—ON BELLADONNA IN THE TREATMENT OF IRITIS.

By JABEZ HOGG, Esq., Surgeon to the Royal Westminster Ophthalmic Hospital.

In my opinion it is in the early or acute stage of the disease, and when certain well-known symptoms are present—as sclerotic and ciliary injection, extreme tenderness of the eyeball, circumorbital pain, lacrymation with intolerance of light, and dimness of sight, slight change in the colour of iris, with an increased vascularity which impedes its motion, so that the pupil can be seen to contract and dilate slowly on the withdrawal or admission of light—that a free application of belladonna or atropine is of the utmost value and importance. The late Dr. Mackenzie thought highly of this remedy in the treatment of iritis. "Belladonna," he writes, "ought to be employed in every case, and in all stages of the disease." The late eminent Von Graefe, speaking of the treatment of this affection, says, "Although I can easily understand that some practitioners are not successful in the mydriatic treatment of acute iritis, because they proceed without discrimination, still, I cannot understand how they can yet consider it irrational. In inflammation of a muscular tissue, what indication is more urgent than to keep it quiescent, at the same time relaxing its most irritable and most threatened parts, in the present case, the pupillary margin? Is there a method more likely to induce reabsorption of an exudation than to deprive it of its source of nourishment? and does not the diminution of intra-ocular pressure facilitate the choroidal circulation, and afford an especial safeguard against the complication with choroiditis so much to be dreaded?"

This is good practical reasoning, for beside the danger to the iris by the formation of adhesions between the posterior capsule of the lens, posterior synechia, there is the danger of the inflammation rapidly proceeding to affect the choroid and retina. It is at the pupillary border of the iris the disease seems to commence, whence it quickly spreads to the deeper seated structures, and the danger to guard against in all attacks is an inflammation of the adhesive kind, which, if not met by active remedies, produces obliteration of the pupil by the rapid effusion of coagulable lymph. It is quite possible that the inflammatory symptoms may, after having run a certain course, abate, and wear themselves out. In such instances the attack may be attended with only a small amount of pain and inconvenience; this has led to a belief in the minds of some practitioners that they may discard such remedies as mercury, iodide of potassium, &c., in the treatment of iritis, but if such a course be pursued, contraction of the pupil—*atresia-iridis*—will most assuredly be followed by total loss of sight.

Every care must be taken to preserve the iris intact, and prevent damage to its muscular fibres from the inflammatory effusion of lymph, which is at times thrown out in the course of a few hours, and without much pain; this can only be done by treating the case actively from the first; and by making a free use of mydriatics. We can scarcely expect to place the iris at rest, and out of harm's way without belladonna, or the instillation of a solution of atropine; and this gives time for other remedies to act. Should the eye prove at all intolerant of atropine, and which it sometimes is, either from the loaded state of the vessels of the iris, or from a slight conjunctivitis, a local abstraction of blood will be needed before its use is persisted in. This will be best effected by the application of a leech—just below the lachrymal tubercle—and far enough away from the angle of the eye to avoid chemosis. When the leech falls off the bleeding should be arrested by pressure, and in a short time it will be seen that the eye tolerates the solution of atropine or belladonna, and the pupil expands under its influence. As during the night the disease often makes great progress, and as whilst asleep there is a natural closure of the pupil, which must in some measure favour permanent contraction, it is as important that the belladonna should be freely applied to the brow at bedtime, as that the instillation of atropine should be persisted in morning and mid-day.

As the extract of the pharmacopœia is generally too hard for use as an external application, I order it to be mixed with a few drops of glycerine, and applied on oiled silk—this prevents it from drying up; when dry it no longer produces mydriasis. Solutions of atropine will occasionally set up a good deal of conjunctival irritation, and cannot be proceeded with on this account; but this often depends upon the character of the solution employed—it may be too strong (2 to 4 grs. to ℥j. of water is of sufficient strength), or the solution may be made with *spirit*, as ordered in the liquor-atropia of the "British Pharmacopœia." This is clearly a mistake, as the spirit invariably produces inflammation or irritation and pain. The chief difficulty appears to be from want of care in the preparation of the commercial salt, and which almost always contains a certain quantity of *free acid*. The alkaloid must be *perfectly neutral*, and dissolved in distilled water. Dr. Mackenzie pointed out an occasional effect of belladonna, which he thought might afford grounds for objecting to its use in the acute stage of iritis, namely, its action on the proper substance of the iris, causing it to contract, and at the same time leaving the *uvea* attached to the capsule of the lens. This is a rare and very occasional effect, and one I have never seen, and therefore venture to think in no way detracts from the great value of the drug; it is one less likely to occur when the case is treated in the ordinary way, that is with mercury or iodide of potash, as well as atropine.

The advantage of the mydriatic treatment is not confined to one stage of the disease; it is equally useful in general iritis; constitutional or specific, as well as in the secondary form of the disease, even that affecting strumous children; and when it occurs as an extension of the inflammation from some other texture—keratitis. In connection also with injuries of the cornea and iris; or in a purely sympathetic character, as when the opposite eye has been injured. —*Medical Press and Circular, April* 30, 1873, *p.* 373.

DISEASES OF THE SKIN, ETC.

74.—ON ACNE AND ITS TREATMENT.

By Dr. Tilbury Fox.

Acne, when fully developed, might be defined, and is usually simply defined, as an inflammatory state of the sebaceous glands, and their follicles, and perhaps the perifollicular tissue, due as a rule to irritation induced by the plugging up of the duct by inspissated secretion, which may perhaps have undergone some chemical change whereby it has acquired an acrid quality. But this description does not satisfy me; for the inflammatory condition in acne is modified as to its extent, its persistence, and its character by various conditions, such as hereditary peculiarity; by constitutional or diathetic conditions, as struma; by derangements of internal organs, as dyspepsia; by altered states of the blood-current, as in gouty persons; and by local influences acting directly upon the seat of the acne, as external irritants. It is necessary, therefore, to regard acne as composed of a primary and essential condition, the plugged and inflamed gland and duct; and a number of secondary items, which influence and modify this inflammatory state, and which must be looked at from the physician's point of view.

First, as regards the general principles of treatment. These consist, as regard preventive measures, in keeping up the general health, and especially a vigorous action of the skin functions, so that the follicles may be kept emptied of their contents. Good friction of the healthy skin, and thorough ablutions with a non-irritating soap, are good to this end. But I need not say more on the point than this, that those causes which lead to hyperæmia of the face should not be allowed to operate.

The treatment of acne itself, as a whole, consists in preventing acne punctata from passing on *to acne vulgaris*, by getting rid of obstruction in the glands, *by checking the hyperæmic condition*; acne indurata, in diminishing hyperæmia and promoting the absorption of inflammatory products; the acne rosacea, in checking hyperæmia, and destroying the new growth of connective tissue. Now, it will be observed how much stress I lay upon checking hyperæmia; and this is to be effected not in one, but in various ways, since the causes of its presence are various. Hyperæmia may be controlled by internal remedies, by local remedies, or by removing conditions that by reflex action gave rise to its continuance. But its disappearance may always be helped out by excluding the air from the face as much as possible. In the chronic stage of the disease, revulsives and irritants may be used; but I am sure harm is always done if the disease be acute, or, in its early stage, if the hyperæmia be severe, by remedies other than those that soothe. Why should we treat an hyperæmic or inflamed skin-gland differently from that of any other inflamed gland or part?

Next, it is necessary to prevent the operation of the several causes of intensification of hyperæmia to which I have referred, and especially dyspepsia, uterine disorder, constipation, and a loaded state of the system. Having done this, we may tone up; and remove, by special means, the consequence of the hyperæmia, viz., the induration by stimulants, absorbents, astringents, and caustics, if need be; and counteract any strumous tendency by cod-liver oil and the like, and so prevent or diminish the implication of the perifollicular connective tissue or the pus formation, if excessive.

Now for the application of these principles to the treatment of the several classes of cases that come to the practitioner for treatment.

For practical purposes, cases of acne that most commonly come to the physician for treatment are divisible into four classes.

In the first may be placed the young boy or girl of lymphatic temperament, come, perhaps of a phthisical or strumous stock, and with languid circulation, pallid countenance, more or less anæmic conjunctivæ, and belonging to a family amongst the members of whom acne is common. Such a patient may suffer from no dyspepsia, but the tongue is not clean, and the bowels are irregular. The indications as regards the general health are those of debility and semi-anæmia; the acne, in fact, being more or less hereditary, perhaps its development is favoured by the debility connected with the lymphatic temperament. The acne attacks the face and back. If the strumous tendency of the patient is marked, the acne spots have a large, more or less livid, base; whilst the suppuration in their centre is marked, and the spots leave behind well-marked cicatrices.

Now, in such cases as these, a general tonic regimen is needed, with good diet, good air, moderate exercise, regular hours, frequent bathing, and the avoidance of sedentary habits and occupation. There is usually malassimilation, and, as I think, especially of fat, which indeed is avoided in the food. The medicinal treatment required for this malassimilation consists in the use of the mineral acids with bitters, pepsin, with iron if the patient be pallid, and, finally, iodides and cod-liver oil, and the fatty matters in the way of fish, eggs, milk, butter, cream. Ioduret of iron, or arsenic if preferred, is to be commended. But it is likewise necessary to exhibit simple aperients to get the bowels to act regularly. The cod-liver oil should be pushed in proportion as the evidences of struma are marked. As regards local treatment, this depends upon the degree to which the acne is developed. Supposing there be very little hyperæmia about the acne spots, and they take the form rather of comedones than those of acne simplex, it may be advisable to wash the part very freely with a mild soap and water, using gentle friction subsequently, together with an astringent or a mild sulphur wash or ointment.

For my own part, I avoid doing this—which is supposed to loosen and get rid of the sebaceous plugs in the follices—if there be dyspepsia present, or the face is in any degree liable to become flushed under stimulation of any kind.

The German plan is to soap almost everybody for acne. Well, the English and Germans, in my experience, are not the same people. I much prefer, where there is hyperæmia, hot-water bathing, or bathing with a mild alkaline wash of carbonate of soda, and the use of the mildest astringents. I scarcely ever use anything but a calamine lotion, ℥ ss. to ℥ vj. fluid, with gr. i. bichloride *of mercury*, and ʒ ij. of oxide of zinc.

The truth is, that as soon as the general condition improves, the acne mends, and, locally, what is needed is the prevention of any undue inflammatory action. Some cases are very obstinate, especially where the avocations and status of the patient render proper care of him or herself, or the obtaining of proper nourishment, an impossibility. When the disease becomes chronic and indolent, the vessels of the affected spots lose their tone and need stimulating. Whilst the inflammatory induration can only be got rid of by stimulants, I prefer a weak tarry or mercurial application to anything else—ʒ ss.- ʒ i. of the huile de cade to ℥ i. of lard, or 5 or 6 grains of nitro-oxide and ammonio-chloride of mercury to ℥ i. of lard. Some cases do well with a mild alum and sulphate of zinc lotion. What I want to bring out prominently to notice, however, is the necessity of avoiding such remedies in the early stages of acne in the patients to whom I am referring, and until the general health has been improved, and the hyperæmia has lost some of its activity.

Another, and, indeed, the commonest kind of case, is that which occurs in those who are the subjects of dyspepsia, and who are debilitated, who suffer from constipation, and are, it may be, anæmic. Any number of these cases may be observed in hospital practice amongst domestic servants, seamstresses, dressmakers, clerks, porters, and, in private practice, amongst people following similar avocations, and in those of the middle and upper classes who live unhealthily. These dyspeptic acne patients, if I may so term them, often get little exercise, live and work in a vitiated atmosphere, in crowded, ill-ventilated workrooms; their diet is often scanty, and their meals are hurriedly taken; and then, at the time that the circulation in the face becomes more than usually active, glandular derangement occurs, excited, perhaps, by exposure to great variations of temperature, and aggravated by the dyspepsia and the debility. Such patients tell you they always flush and feel distended after meals, when the face burns and is redder than usual; and this excitation of the vessels of the face is kept up by the dyspepsia. The urine is loaded with lithates, and the bowels are sluggish. These are the easiest cases of acne to treat. They all get well. They need aperients, antacids (I prefer a gentian and soda, prussic acid, and ammonia mixture, a dose being taken an hour before each meal, with bismuth for a time, if there be pyrosis) together with moderate exercise in the open air; the avoidance of beer, sugar, pastry, seasoned dishes; and, when the tongue cleans, the exhibition of iron and quinine and arsenic combined. The arsenic, I believe, acts as a nerve tonic, and not as a specific against acne. Locally, I never use anything but the calamine lotion before mentioned, followed by a weak sulphate of zinc and alum lotion, three or four grains of each to the ounce. The use of these local remedies should be conjoined with that of hot fomentation, night and morning, and the avoidance of all soaps.

Most practitioners commence at once with the internal use of arsenic and iron in these cases. I feel confident that the removal of the dyspepsia is the proper thing to be first effected, and I can speak from a rare success in the management of these cases of acne. In some of these cases, there is more or less disorder of the catamenial function—amenorrhœa may be present; but the uterine functions are restored to their natural state when the general health improves under the influence of tonics.

But these same cases of acne in dyspeptic subjects are seen, not only amongst the lower and the more indigent of the middle classes, but amongst the upper and well-to-do classes. The dyspeptic and the general lethargic condition which exists in these patients is the result of the inactive and luxurious lives which they lead, the late hours which they keep, and the rich diet of which they constantly partake at all kinds of evening hours, till the bodily functions become torpid, the general system enervated, and the stomach and excretory organs oppressed, dyspepsia and debility being two of the main results. These cases are to be treated as other dyspeptic cases, only the more care is needed in the prescription of diet. Plain and wholesome diet should alone be allowed, wine and condiments, pastry, and sugar be proscribed; whilst hygienic measures, cold-bathing, horse exercise, change of air, and like measures, calculated to invigorate the body, provided the face is not exposed to external irritants too much, are to be commended. The form of acne is usually described as acne *vulgaris—it may be A. indurata* if the disease has lasted a long time.

But I have two more classes of acne cases to consider. The next kind of case which I may deal with, is that which is observed in patients in good health, and in the condition which the older writers would have described as plethoric. These patients are not particularly young. There may be a certain amount of dyspepsia in their case, very often none; but there is oftentimes a loaded state of system. The bowels are inactive; the kidneys do not excrete freely and fully; the pulse is full; there may be some headache. The face is not only studded over with acne spots, but is more or less generally congested. These patients are oftentimes what may be not at all inappropriately, in their cases, styled, gross feeders. They eat plentifully, and oftentimes take a full quantity of beer or other stimulant. Some exhibit apparently a gouty tendency. The acne spots are well marked, numerous, and large in size; they possess indurated bases, and are easily inflamed. The back and face and the forehead are the parts attacked in these cases. Now, the main thing to be done in such circumstances is to "cut off the supplies" for a while, and to give aperients with a sufficiently free hand, together with mild tonics, and, in gouty subjects, colchicum in addition. After a while, tonics alone, with or without arsenic, may be given. The combination of aperients with tonics in these cases was the secret of Mr. Startin's success, I believe. It stands to reason, that, if the emunctory organs have not been doing proper work for some time in these acne subjects who "make blood fast," the blood-current will be unduly charged with effete products, which depress the powers, and so induce debility indirectly; and, moreover, set up, or at least intensify, local congestions—such as acne of the face. Such a prescription as the following may be written:—Sulphate of magnesia, ʒ ss.; aromatic sulphuric acid, ♏xx.; sulphate of iron, gr. iij.; quinine, gr. i.; colchicum wine, ♏x.; syrup of ginger, ʒ i.; and water, ℥ j., twice or thrice a day, with an aperient pill if needed. It soon reduces the facial hyperæmia, the more so if the diet be unstimulating; in practice, stimulants should be interdicted, and the patient be directed to take a course of Vals water, which keeps the kidneys active, and prevents any undue acidity of stomach. By-and-by, after the face is less heated, and the acne spots less inflamed, the colchicum may be omitted, and some preparation of arsenic substituted for it; but a gentle aperient action is to be kept up all the while the patient is under treatment. Now, as in the two other kinds of cases before described, the local treatment must always be soothing at the outset. But, as the general state improves, more active remedies may be used in these plethoric subjects, and I think sulphur is especially useful in them. I use generally a weak sulphur ointment, gr. x. to ℥ i. of adeps, to begin with, and prescribe, in addition, a simple calamine lotion as a protection to the face; but if the sulphur does not succeed in improving acne indurata, I then prefer touching the acne spots with acid nitrate of mercury, so as to lead to their resolution; or if this be objected to, or be inconvenient, I apply mercurial plaster at night.

In some cases, the acne spots are very large and inflamed, and, indeed, the whole gland, in its deepest part and its surrounding connective tissue, is actively involved in the inflammatory swelling. I think this condition is one most benefited by the free use of aperients with iron, and also, if the patient be strumous, of cod-liver oil. But a course of Kreuznach or other waters seems, in some cases, very efficacious, if the acne is chronic and holds to the patient with any special pertinacity.

The last kind of case to which I shall refer is that which occurs in middle-aged men and women, and takes the form of acne rosacea. Some writers have disputed much whether this is in reality an acne at all. It is not of much consequence whether we class acne rosacea with acne or chronic inflammations of the skin. The disease is a composite affair. It is a chronic inflammation of the face made up of acne spots, periglandular inflammation, erythema, and new growths of connective tissue formed independently of the glands. The first stage consists in congestion of the face, and more or less dilatation of certain capillaries. Particular points of the papillary layer become hyperæmic; and certain of the glands are similarly affected, so that acne spots are produced, an excessive amount of sebum is often secreted, and the skin feels *greasy (seborrhœ).* The general surface of the skin of the face is more or less congested. In the next stage, as the consequence of the continued inflammation, the connective tissue around and about the glands hypertrophies—that is to say, the acne spots become indurated and hard; and the independent non-glandular papules become more marked. The colour of the redness is bright red; the vessels become varicose, and ramble freely over the surface of the diseased parts; suppuration is not very marked, but the integument generally is thickened as a consequence of the general congestion. Now, this disease is rarely seen in the young. It occurs in women of middle age who suffer from uterine troubles, and attacks the nose and the cheeks by preference. The disease is aggravated by trouble, by stimulating food, by exposure, by dyspepsia, and by alcoholic drinks.

It is difficult to manage. Some of the cases combine in them the special features of the "dyspeptic" and "plethoric" cases, but then there is no "physiological gland activity" element in the case. The truth is, that the face is generally congested, and the glands are involved as part of the face. The pustulating points indicate the seat of the affected glands, and the minute vessels running up to the indurated base are the best indication of the seat of the newly-formed tissue—the hyperplasia. The skin generally is, as I have said, more or less hypervascular.

I suppose these cases are seen most commonly in women of nervous temperament who have suffered for some time from dysmenorrhœa or even menorrhagia, and who have experienced much mental excitement or anxiety; or in men who have indulged freely in alcoholics or lived badly. The women are often affected by atonic dyspepsia as well. In the management of these cases, what is needed is the rectification, in the first place, of the concomitant condition of ill health, leaving the acne to take care of itself for the while, so far as internal remedies directed to cure it are concerned. The diet in all cases must be unstimulating; alcoholics are to be avoided; when the tongue becomes clean and all evidences of dyspepsia are disappearing, and the uterine functions are restored to their proper state, then the acne itself may be treated. In cases in which there is dysmenorrhœa, with anæmia, and in which the face gets worse before the periods, the combined use of bromide of potassium and perchloride of iron with simple aperients, and if the flushing of the face is marked after meals, a course of Vals water has succeeded in doing much good in my hands. Supposing that there is much induration about the individual spots, if the patient is in fair health and there be no dyspepsia, a mild mercurial course may be prescribed with much success. I give 1-20th or 1-24th of a grain of a bicyanide of mercury with quinine, twice a day for three weeks or so, to be followed by 5 or 8 drops of Donovan's solution twice a day for some little time; cod-liver oil being likewise exhibited at the same time if desirable. Supposing the skin of the face generally to be thickened, I think Donovan's solution the best remedy to begin and continue with. If the amount of blood in the face varies under slight influences, then I prefer saline aperients with iron and quinine. In those cases in which the patient seems to be suffering from what we know as "nervous debility," then I have recourse to arsenic without hesitation, and push its exhibition in connexion with quinine and allied tonics or nux vomica. In the case of the man who drinks, and has semi-pickled his gastric mucous membrane, the difficulties are great. The best course to adopt is first to send him to drink the waters at Aix, Harrogate, or Kreuznach as a preparatory treatment, and then to exhibit arsenic and iron with aperients.

But there is one thing more to be said about these cases of acne rosacea. It is the necessity of ascertaining if there be any gouty tendency or syphilitic taint about the patient. If the patient be gouty, so much the more reason for a course of Kreuznach waters, and if syphilitic, for a mercurial course; although, as I have said before, a mercurial course may be beneficial in hastening the removal of the inflammatory indurations. In very chronic acne indurata, and still more so in syphilitic cases, the forehead and temple regions are specially involved.

Locally, the sooner the acne spots are destroyed by acids the better. But then my argument will be the same here as in regard to other phases of acne. So long as any *concomitant condition* which leads *per se* to an intensification of the hyperæmia of the face exists, so long will it be necessary to withhold stimulation and to use soothing remedies in order to keep hyperæmia—I mean of course when that is active—in check; and I hold it to be doubly necessary to observe this rule in dealing with *acne rosacea*, since in that disease there is a large amount of hyperæmia, not merely of the glands and periglandular tissue, but the derma generally. In the early and active stage, then, of acne rosacea, I am content to use hot water and poppy bathing, with inunction of oil or cold cream, followed by the application of warm lead and opiate lotion, the face being diligently protected with a powdery layer deposited from a calamine lotion. Then stimulate with hypochloride of sulphur ointment, or apply a zinc and alum lotion, and touch the several spots with acid nitrate of mercury as they spring up or are obstinate in disappearing. This plan will best rid patients of their trouble.

But I hasten to add, that though I have set forth in order the general treatment of acne, I do not forget, of course, that acne cases come to us for treatment at all stages, and under varied circumstances, and it is the duty of the physician to adapt his treatment thereto.

I have said nothing as yet about the diagnosis of acne, and will content myself with one practical remark, that in middle-aged men and women, but the latter especially, there is a condition of face which is universally regarded as acne (*A. rosacea*), but which has nothing to do with acne. The patient comes to the physician stating that she has had the rash on her face for perhaps several years. It gets a little better at one time, but only to become worse again subsequently. It developed in the form of little red pimples scattered over the face, especially about the cheek and sides of the nose. If a careful inspection of the spots be made, they will appear like little fleshy lumps, varying in size from pins' heads to a quarter of a split pea; they are dull red, and they look fleshy—they are, in fact, little new growths, neoplasms—and have no central aperture indicative of the opening of the follicle,—in fact, are clearly formed not at the follicle, but in the papillary layer. On inquiring into the antecedents of the patient, a past history of syphilis may be made out: the patient has had sore throat, which ran a chronic course; she miscarried several times, or had still-born children; and has had a sore tongue. Perhaps there are evidences of past ulceration about the fauces—in fact, indistinct evidence of a syphilitic history may be occasionally obtained. I only briefly refer to these cases, and will only add, that they are cured by antisyphilitic remedies and tonics, whilst other means fail.

In conclusion, I may be permitted to say, that although I have not perhaps brought forward anything new, yet I hope to have put the facts connected with the development and the course of acne in a truer clinical light, and to have indicated with tolerable clearness what are the exact principles upon which the treatment of the disease should be conducted.

I have tried to show, *firstly*, that acne must be regarded as being modified in degree and aspect by a variety of accidental conditions, that in treating acne it is necessary to deal with these conditions, and to negative their influence before, or at the same time that, the acne itself is treated; *secondly*, that in the early stages of acne the only admissible or successful treatment is one that soothes the hyperæmic surface. Stimulants and revulsives must only be had recourse to in the chronic or indolent stages, and after the influences of agencies that aggravate or that keep up the hyperæmia have been checked.—*Edinburgh Medical Journal, May* 1873, *p.* 1005.

75.—CASES OF RINGWORM TREATED BY OLEATE OF MERCURY.

By Dr. LEONARD CANE, Queen Square, W. C., London.

In introducing the use of oleate of mercury, in a clinical lecture published *in the Lancet on* May 25th, 1872, Mr. Marshall mentions its applicability to certain skin diseases, and the record of the following cases of ordinary ringworm (tinea circinata) treated by oleate of mercury may be serviceable.

Case 1.—Here there was a well-defined, slightly-raised circular-patch of tinea circinata on the side of the neck, about the size of a two-shilling piece. The spot was rapidly extending itself, and its edge was marked by a circle of small vesicles, situated on a slightly inflamed base. The centre presented a "branny" appearance, owing to its being covered with fine brownish scales. These scales when treated with liquor potassæ were found under the micro scope to contain numerous spores and threads of fungus.

On June 3rd, a few drops (about twenty) of the oleate of mercury (10 per cent. strength) were *gently* rubbed over the spot with a piece of lint. The oleate was applied beyond the diseased patch.

On the third day the patient was again seen. The sight of the patch was marked by a flat, circular, reddened spot of the same dimensions as the original. There were no vesicles, and no traces of any fungi could be found. The spot appeared to be completely cured, but, as a matter of precaution, it was deemed advisable to reapply the remedy. It was therefore dabbed over the surface, about five drops being used.

Since then there has been no reappearance of the disease. In this case the oleate caused no inconvenience whatever. There was no staining of the skin, no pain after application, and the cure was rapid.

Case 2.—This was a much more severe case. The disease had been treated by various remedies—namely, by glacial and ordinary acetic acid, by iodine, &c.,—and although these had stopped its progress for a time, it had invariably broken out afresh. When first seen the disease was extending almost round the whole of the back of the neck, from the angle of the jaw on one side to that on the other. It was also spreading very fast up into the hair, and for a distance of more than an inch the hair on the back of the head was invaded by the disease. There were also isolated patches on the chin, below the angle of the mouth, and on the upper lip. There were several scars where glacial acetic acid, and I believe also strong nitric acid, had been applied, and in one place there was a troublesome sore produced by the strong acid, which was kept up by the rubbing of the collar. The disease had existed for several months, and a second case had occurred in a house where the patient had been staying whilst the patches were on his neck. The newer spots were well marked and highly characteristic, and afforded abundance of sporules, &c. The site of the older ones was reddened by inflammation, and discoloured by the application of the various remedies mentioned.

On June 16th, after cutting off as closely as possible all the short hairs, so as to remove as much of the fungus as it was possible, I applied the oleate of mercury (ten per cent.) over the whole of the affected skin, gently rubbing it in with a piece of lint. About half a drachm was used altogether. The isolated patches on the face were similarly treated. Care was also taken to rub the hair above the diseased part with the oleate. The patient was then ordered to wear a loose collar and not to wash his neck during the day. Some slight irritation followed the application, but this was trivial, and not for a moment to be compared with the pain produced by strong acids, &c. There was no staining of the skin, and, as the spots were in a prominent position on the face, this was no slight advantage.

On the 18th patient was again seen. There had been no fresh appearance of the disease, and the old spots had not extended themselves. There was still considerable redness over the old scars, and a few minute pustules produced by *rubbing* in the oleate, but there were none of the small vesicles which had been so evident on the former occasion. The oleate was reapplied over the places to ensure success.

On the tenth day after the first application the disease appeared to be completely cured. There were several pale red patches showing where it had been, whilst the spots on the face and chin had quite disappeared. The hair had begun to grow again, and showed no trace of the spores. The sore produced by the acetic acid before he came under my notice had scabbed over, and was now *nearly well.* A *third* application was made, as a precaution, *where the hair had been affected.*

When seen a short time afterwards, the skin appeared perfectly well, and there has since been no reappearance of the disease.

The advantages which oleate of mercury seems to possess over other remedies are:—

1. It is a *certain remedy* if carefully applied.

2. It *produces no staining* or injury of the skin. In cases where the disease appears on the face, it is of great importance to avoid any disfigurement or staining.

3. It is *painless* in its application. This is not the case with the ordinary strong parasiticides, most of which produce vesication, &c.

4. It *readily penetrates* into the sebaceous glands, hair follicles, and even into the hairs themselves, the mercury being in a state of solution in an oily medium, and it is therefore much more likely to destroy the fungus than the spirituous or aqueous solutions of mercury, &c. This penetrating power of the oleate may be increased by adding a small quantity of ether (one part to eight) to it.

In very sensitive skins the irritation sometimes produced by it may be avoided by using a weaker solution (five per cent.), and by applying it with a camel's-hair-brush. In slight cases this method is all that is necessary, but where the fungus has invaded the hair it is advisable to rub in the oleate gently.—*Lancet, Aug.* 16, 1873, *p.* 227.

76.—ON THE TREATMENT OF ULCERS OF THE LEG.

By Dr. J. Gordon Black, Surgeon to the Hospital for Sick Children, Newcastle-on-Tyne.

The perusal of the valuable reports which have appeared in the Journal on the treatment of ulcers, at the various London hospitals, induces me to offer a few remarks, in the belief that good will accrue from the further ventilation of the subject.

It seems pretty generally admitted, that the treatment of ulcers of the leg, in the out-patient room, is unsatisfactory and disheartning. Whilst some instance the intemperate habits, the poorly fed and over-worked condition of the patients, to account for this want of succes, I am more inclined to blame a wide-spread belief in the profession, as expressed by Mr. Lawson, or the Middlesex Hospital, that "for the effectual treatment of all ulcers of the leg, absolute rest of the limb is the first element." Having for some time past been in the habit of curing cases of this kind without requiring the patient to neglect his ordinary duties for a single day, I certainly cannot hold such a belief. If it be possible to cure a large ulcer of the leg without rest, and in quite as short a time, to say the least, as would be required to heal the same by recourse to the horizontal position, what becomes of the theory that absolute rest is necessary?

Moreover, during the time that the patient remains in bed, the circulation through the limb is rendered more efficient, and healing of the ulcer ensues; but no sooner are ordinary duties resumed, than the old conditions recur, bringing back with them the inevitable ulcer. A cure, therefore, under the absolute rest system, can scarcely be alleged, because it is not permanent; whereas, if an ulcer be healed without rest, it is clear that a cure has been effected, provided similar therapeutic conditions are maintained. This latter provision cannot, of course, be observed under the former plan of management.

Knowing, as I do, the easy practicability of healing ulcers without rest, I cannot think it proper to admit such ailments to the wards of a hospital. To do so, seems wasting the funds of the institution, no less than the time and labour of the sufferer. At the same time, it is not creditable to surgery that such patients should be neglected, or given to understand that their weary and loathsome malady is incurable.

The plan which I adopt is practically the same as that recommended by

Baynton, nearly eighty years ago, but with the important modification of using it antiseptically. Baynton's strapping has long been recognised as valuable and effective, and is described by Mr. Erichsen under the head of "Indolent Ulcer." Such management, however, taxed too severely the time and patience of the surgeon, for its successful practice. Unless the plasters were very frequently removed (Mr. Erichsen says every forty-eight hours, at least), the pent-up discharge became very offensive, causing the dressings to be disagreeable, both to doctor and patient.

In order to avoid these disadvantages, I now warm the plasters by passing them through hot water, to which a little solution of carbolic acid has been added. The sore having been washed clean by the patient, is then saturated with weak solution of carbolic acid, and the straps, first treated as described, applied. The pieces of plaster (stout emplastrum saponis), should be two inches broad, and long enough to overlap four inches, after passing completely round the limb. They should be applied after the manner of a "Scot's dressing," from about three inches below the lowest diseased surface, to about the same distance above the highest. In their adjustment I think it most important to use no compression, but simply to lay them down evenly, so as to fit the limb accurately, and leave no creases in the plaster. Should pain be produced, the strap has been improperly applied, and must be at once removed. The bandaging of the limb, lightly and carefully, from the toes to the knee, finishes the dressing, which latter need not occupy more than ten minutes altogether. The patient may be told to return at the end of a week, when, on removal, the plasters will show only a slight moisture, instead of the profuse and offensive discharge seen when no antiseptic is used.

The advantages of the above plan of treatment are briefly these. It is cleanly; it saves the time and labour of the surgeon, for the dressings need rarely be changed oftener than once a week, and occupy only a few minutes. And, finally, whilst the healing process is conducted with a minimum of pain and discomfort to the patient, he is in no way restricted from pursuing his ordinary occupations.

In cases of irritable ulcer with much pain, Baynton recommended the sufferer to remove the bandage occasionally, and pour cold water upon the strapping for a few minutes, afterwards drying lightly with a soft towel, and reapplying the bandage. The plan is an excellent one, and usually very grateful to the patient's feelings.

Instead of employing carbolic acid, another good antiseptic may be used, namely, sulphurous acid. This is easily applied by playing upon the ulcer and surrounding diseased skin with a Dewar's spray-apparatus. The plasters may then be adjusted, after passing them through hot water, simply. A little smarting ensues, which, however, soon passes off. The effect of the sulphurous acid, in checking discharge and mal-odour, is quite as good as that of the carbolic, whilst its application is perhaps less troublesome and disagreeable. The sulphurous acid is especially suitable to ulcers of moderate size.

Baynton's strapping, especially when used antiseptically, may be employed for nearly all kinds of ulcers. The surface of a weak, indolent, or inflamed ulcer, speedily assumes a healthy appearance, without the preliminary use of astringent, soothing, or other lotions, being necessary. The most irritable sore may be strapped if care, and no compression, be used. Occasionally, however, it may be found advantageous to substitute linen or calico for the plaster straps.

For varicose ulcers, no treatment could be better. The horny edges of the "callous" variety quickly disappear, without recourse to such a dangerous excitant as blistering, which may easily set up unmanageable inflammation in the old or infirm. In eight or ten dressings, even very extensive ulcers may be healed by strapping, so that the cases must be few where skin grafting is really needed.

The administration of medicines internally is unnecessary. In most cases opium may be given to relieve pain, but the healing process goes on steadily, without such assistance.

I observe that Mr. Nourse, of Brighton, has used strapping and bandaging with great success. *The plan, I feel sure, only requires more extensive employment to be better appreciated.* For the frequent failure of the ordinary treatment by lotions, &c., patients are often blamed, because they do not strictly obey the instructions given. This neglect is, however, due rather to the well-known feebleness of such remedies, than to any lack of pains or inconvenience on the part of the sufferers. On the other hand, the interest which the latter manifest in carrying out directions under the treatment by strapping, is by no means the least recommendation of this method.—*British Medical Journal, May* 31, 1873, *p.* 608.

77.—ON SOME POINTS IN THE SURGICAL AND MEDICAL TREATMENT OF CHRONIC ULCERS OF THE LEG.

By Dr. John Kent Spender, Surgeon to the Mineral Water Hospital, Bath.

More than forty years ago my father devised an ointment containing a very large quantity of prepared chalk, in order to form an artificial crust over an ulcer. Let us ask ourselves what occurs when nature alone accomplishes the cure, and which may be sometimes observed in the case of injured animals. Sir James Paget tells the story in his usual lucid way: "Sometimes . . . the blood shed from a wound coagulates and dries on it, and, remaining as a scab, permits healing under it; or, if this do not happen, a similarly effective scab may be formed by the serous fluid or lymph by which the surface of an exposed wound usually becomes glazed; or, more rarely, the pus of a granulating wound may scab over, and sound healing take place beneath it." We see, then, that there are three agents which may be concerned in forming a protective scab over a recent wound—blood, lymph, pus. A scab constructed of either of these materials shields the part from outward injury; and, thus covered and guarded, a new skin is formed, and the crust drops off. When the cure is left to nature, it is only a superficial ulcer which readily scabs; for if the ulcer be deep the quantity of matter is too great to be speedily evaporated into a crust. Therefore our object should be to assist the natural process by the addition of some harmless substance which will thicken the discharge and produce an incrustation; and this is very efficiently done by covering the ulcer or wound with the ointment described above. The chalk must be in a much greater proportion than that which enters into any ointment in the Pharmacopœia, consisting of about three pounds of chalk to two pounds of lard. And the best way of preparing this application is, not by rubbing the chalk down with the lard, but, having previously reduced the chalk to a very fine powder, melt the lard in any convenient vessel over a slow fire, and then add gradually the chalk to the liquefied lard. Stir and thoroughly mix the two until nearly cold, and the ointment is then ready for use.

Now, it will be found that several capital advantages accrue from the use of this ointment:—1. It generally produces much ease and comfort. 2. When the lard becomes melted by the heat of the part, the chalk is disengaged, and a portion of it combines with the secretion from the ulcer. This secretion is often so acrid as to excoriate the neighbouring skin; but, when united with the chalk, it is converted into a neutral innocuous compound. 3. A crust is formed out of this compound; first on the surrounding skin, then on the margins, and finally on the surface of the ulcer. 4. Little disturbance of the applications is necessary. At first the quantity of the discharge may require the frequent renewal of the dressings; but this will be less and less needed as time goes on, and our object should be to maintain the mechanical integrity of the chalky incrustation.

The ointment should be spread smoothly and pretty thickly on the soft side of good French lint, and the ulcer and a fair circumference of the adjacent skin should be covered with it.

This preliminary operation being completed, a bandage should be put round the limb with surgical neatness and care. I almost always use bandages made of "domette flannel;" this material is thin and yielding, and yet almost any degree of compression can be exercised with it. In œdematous swellings generally the flannel is soft to the skin, and accommodates itself to the greater or less distension of the limb. The calorific properties of flannel, too, are useful, as in all structures in which the capillary circulation is obstructed the vital heat is reduced. The bandage should be at least six yards long if required for an ordinary male adult; the breadth should be under two inches. Every portion of the limb, from the toes to the knees, should be equally and evenly compressed. Indeed it may be said that good bandaging is of such absolute importance in the treatment of varicose ulcers of the leg that without it everything else will be comparatively ineffectual. This being so, the rapidity and completeness of the cure will depend very much on the manner in which it is employed. Without practice it is not easy to place a bandage properly on the leg; and probably this difficulty is the chief reason why preference is often given to adhesive plaster, as this sticks and remains wherever it is put. The blistering and excoriation often produced by strapping, and the time consumed in the application of it, are sufficient reasons for acquiring skill in the art of bandaging—an art whose comforts and advantages are appreciated by a patient commensurately with the dexterity with which it is practised by the surgeon.

In late years I have sometimes employed bandages made of a very thin and somewhat elastic calico. They are suitable to persons who have an irritable skin, and they are generally useful in summer time.

Now I recommend the chalk ointment above described, not because it has any empirical virtues, real or supposed. No one pretends that any therapeutic miracle or mystery is worked by it. The plea advanced on its behalf is simply this—that it admirably helps the natural healing process, and it helps nature by imitating her. Accidental circumstances may in any case prevent nature from being so easily imitated and so smoothly put right; but the experiment is always worth a fair trial. When unforeseen difficulties present themselves, the first impulse is to "try" all sorts of unusual remedies, in the hope that by pure chance the right remedy may be found out. A philosopher's healing stone will perhaps be discovered in this way some day; but at present it is more safe to be guided by principles of common reason, and we shall thereby sometimes reap a success when we least expect it. The literature of this subject is very desponding, but we shall clear the road a little if we point out some rules and landmarks which will now and then save us from going quite wrong.

Next, let our inquiry be, What ought to be done when we have to treat an ulcer which not only does not heal, but which obstinately wears a foul and unpromising aspect? In the first place, then, the use of a nitrate of silver lotion (one scruple to one ounce of distilled water) stimulates a healing surface, and often prevents it from degenerating into superficial gangrene. Every time the leg is dressed, the ulcer and the circumjacent skin should be painted over with this lotion; it acts as a true sedative, even though for a little while it may cause a trivial smarting. Suppose, again, that the sore is always covered with a thin greenish slough, what are our resources for this state of things? Steep a pledget of "chloralum wool" in equal parts of water and the solution of chlorinated soda (liquor sodæ chloratæ, Ph. B.); fill up the cavity of the ulcer with the saturated wool lightly teazed out, and cover it with a chalk-ointment dressing. No disinfectant is more potent than chlorine, and in this little-used preparation of our national Pharmacopœia we have a means of applying it locally and effectively. It is, in my judgment, very much superior to carbolic acid in its cleansing power; and the latter has such a disgusting smell as almost to need some disguise to make it tolerable. Even the weakest solutions of carbolic acid are apt to irritate and even inflame a delicate skin, and this fault can never be alleged against chlorine. I can speak in the highest terms of "tarred oakum" as an application to superficial sloughing sores in the coarse, thick integument of wayfaring people, and I have used it much in dispensary practice. The stuff called "tenax" is good for more delicate folk, but it is hardly enough impregnated with the tarry principle.

"Senile gangrene of the integument" is the name given to a subtle but formidable disease, considered by some surgeons to be a variety of senile scrofula. A dirty, buff-coloured slough creeps along, until large tracts of skin may become involved; and very severe pain is a frequent concomitant. The necessity of arresting (if it be possible) this slow molecular death ought to be perfectly clear; otherwise it may lead to remote but fatal septicæmia. Now, what are we to do? Are we to cover each patch (say on the foot and leg) with a linseed poultice, with the idea of getting rid of the dead skin and finding a clean granulating surface underneath? I have seen this plan tried, and with the natural result of increasing the sloughing area. Heat and moisture are precisely the agencies which hasten decomposition and encourage any approach to it; and the sloughs, instead of being loosened and shed, are much more likely to extend. Moreover, I have found that the pain is even increased by the persistent application of poultices of any kind. Out of a number of cases of "senile gangrene" which have come under my care during the last few years, I relate one which illustrates pretty clearly what seems best to be done.

At the end of last November I was asked to visit a lady, 67 years old, who had a black gangrenous patch, four inches long by an average of an inch and a half broad, on the lower and back part of the left leg. The general health was bad (there was an attack of nearly fatal syncope on the second day of my attendance), the local pain was great, and the disease was slowly extending. There had been no medical treatment, and only some simple cerate had been applied to the sloughy surface. The whole of this was brushed with a solution of nitrate of silver (strength specified above); it was then dressed with lint on which was spread chalk ointment, mixed with an equal part of ointment of benzoated zinc; and, finally, I put on a domette bandage (with moderate pressure) from the toes up to the knee. In about ten days (the dressing having been renewed daily) a profuse "laudable" secretion loosened the slough at its edges; within ten days more it came entirely off, leaving a healthy granulating surface, which healed pleasantly and gradually before the end of January. The general health was sustained with tonic medicines and an abundant supply of protein food.

Poultices may be fine and useful things, but only an utterly wrong estimate of their capacities would lead the surgeon to apply one to a sore which is unhealthy or mortifying from intrinsic sources of decay.

The second topic of my paper relates to the non-necessity (and even uselessness) of absolute rest of the ulcered limb, provided that the mechanical element of the treatment be sound and sure. In this matter the principles of surgery seem to have retrograded, if we compare a recent summary of the "Treatment of Ulcers" in some London hospitals with what we may read in the "Practical Observations on the Cure of Wounds and Ulcers on the Legs, without Rest," written and published by Mr. Thomas Whately, in 1816. Mr. Whately tries to obey Pott's immortal maxim, that "it is the business of good surgery to assist nature;" and, in the third chapter of his excellent book, after a minute relation of the way in which an ulcerated leg should be dressed and bandaged, he says that "from the first application of the bandage we may freely allow the patient to follow the most laborious employment, and to walk or stand the whole day. I can with great truth say that I have scarcely ever found any impediment to the healing of these wounds by such a constant motion, provided that they were dressed and bandaged in the manner just described."

The great dogma of rest has been enforced with much eloquence; but I venture to assert that the therapeutic pendulum has swung too far that way, and we are in positive danger now of a caricature of the whole thing. Firm and equal compression with a bandage answers every object to be gained by the recumbent posture; and the help which this may be theoretically supposed to afford by no means counterbalances the acknowledged evils dependent upon the loss of constitutional exercise. Continuous rest, on account of a small local lesion, implies (assuming the body to be otherwise healthy) a large capital of bone and muscle lying fallow, and even deteriorating from want of employment. Further, it is with many persons a matter of serious inconvenience and cost to abandon all active pursuits for weeks or months together; nor is the subject of less moment in the economy of hospitals, in which we often find beds occupied by cases of ulcer of the leg to the exclusion of other more urgent and certainly more interesting forms of disease.

An immense array of facts in my experience have practically settled this question, and authorised me to state that the treatment of *ordinary* cases of ulcer of the leg does not require any rest of the limb at all. Any casual attack of inflammation must of course be met with suitable measures, and these will include rest and an elevated position; otherwise the moderate use of a limb, when properly bandaged, is not only no bar to the cure of a varicose, or any other variety of ulcer, but is an actual help to it. To lay up a part of the body in idleness admits of the healing of an ulcer by a degree of vital action, which is unable to maintain the permanence of that healing when the part is restored to its natural use. It is only fair to add that Dr. Underwood and Mr. Chapman fully recognised the value of exercise as a curative stimulus; and more modern writers (as Mr. Hunt and Mr. Houghton) have endorsed the same view.

It is a point of good practice to bandage a leg enlarged and varicose, although no ulcer may have yet appeared. We note the signals of coming danger, and, if our science be so often at fault in the attempt to remove an evil, we ought to discover in this the stronger inducement to ward off its approach.

The *medical* management of the various forms of ulcer ought not to be disparaged. The clue of syphilis should be sedulously followed up to its source, and treated with precision accordingly. To Mr. Maunder we are indebted for a useful diagnostic observation. He says that all those sores which are situated *above* the middle of the leg are syphilitic in their origin, and are mostly multiple also; while the varicose ulcers are found *below* the middle of the leg, and are usually solitary. Gout and lead poisoning may aggravate and exasperate the most innocent ulcers; and chronic alcoholism is the most potent influence of all in causing universal degradation of tissue. So far as medical treatment is concerned, a large number of non-inflamed chronic ulcers are curable, or made much better, by a course of citrate of iron and iodide of potassium, with now and then an aloetic purgative.

This is not the place to discuss the large question of skin-grafting as a surgical remedy for ulcers. I tried it in a few cases during my long tenure of office at the Eastern Dispensary here, with doubtful results; but in private practice I have never had occasion to resort to it, so great has been my success with the plan of treatment described in this paper.—*Lancet, May* 3,1873, *p.* 623.

78.—TREATMENT OF CHILBLAINS.

Mr. Fergus (Canada Medical Journal) recommends sulphurous acid in this affection. It should be applied with a camel-hair brush, or by means of a spray-producer. One application of this usually effects a cure. The acid should be used pure. A good wash for hands or feet affected with chilblains is sulphurous acid, 3 parts; glycerine, 1 part; and water 1 part. The acid will be found particularly useful in the irritating, tormenting stage of chilblains.—*Medical Press and Circular, Aug.* 27, 1873, *p.* 201.

79.—ON SUTURES, AND THE TREATMENT OF INCISED WOUNDS.

By Dr. J. C. Ogilvie Will, Aberdeen.

Sutures are undoubtedly the best means we possess for maintaining the edges of cut parts in apposition. Unfortunately their employment is attended by certain disadvantages: such as the irritation, giving rise to suppuration, when silk is the substance of which they are composed; the danger, during their removal, of tearing apart loosely united surfaces; and the pain experienced by the patient when they are constituted of metallic bodies. Another grave objection to the use of sutures is, the unsightly marks—consequences of suppuration—which remain at the points where they had been inserted. These cicatrices are of course chiefly objectionable in parts exposed to view, as in wounds of the face etc. The object of the present paper is to draw atttention to sutures formed of a substance which can be employed without fear of the foregoing undesirable effects presenting themselves. This substance is catgut, which Professor Lister brought into notice some years ago for this purpose, but which he has since discarded, though he still retains it for ligatures. Regarding its use for the latter purpose I cannot speak personally, as I have always found acupressure answer every purpose; but with regard to the former I can speak most emphatically, for during the last two years in every wound where I have had reason to expect early union—and it is only in such cases that I advocate its use—I have invariably employed catgut sutures, and their employment has been attended by the happiest results. In proof, I may cite a few cases.

1. A child came against a pane of glass in a window, broke it, and in so doing almost severed the fleshy part of his nose from the cartilages. The surfaces of the wound, having been washed and exposed to the air for a few minutes, were brought into apposition, three catgut sutures were inserted, and cold-water dressing applied. In three days the sutures were removed, and a day or two after hardly a trace of the injury remained. The sutures did not give rise to the slightest irritation.

2. An injury causing a somewhat similar but much more severe lesion, the cartilage of the septum being fairly divided, occurred to an old woman. In this case the edges of the wound were very jagged. The same treatment was adopted with like results.

3. Case of excision of the mamma. Eight sutures; one vessel acupressed; pin removed seven hours after; immediate union. The sutures, not giving rise to the slightest inflammation, were allowed to remain till the thirteenth day after the operation, when they (or rather what remained of them) were removed. Shortly after the points where the sutures had been inserted could not be discerned.

4. Another very striking case was that of a young child who was brought to me with the point of his middle finger hanging merely by a thread, like a piece of skin, the whole of the other tissues, including the bone, having been divided. After carefully cleansing the wounded surfaces, and allowing them to become glazed, I brought them into accurate apposition, and inserted two catgut sutures, remarking at the time to the boy's mother, that I had little hope of a favourable result, and that I would probably have to remove part of the finger, but that I had given him a chance. Fortunately I was not obliged to resort to such a proceeding, as the parts healed kindly, and on the sixth day I removed the sutures. On that day only one small point remained unhealed.

5. Amputation of the third finger, with removal of part of the metacarpal bone, in the case of a boy. Three sutures; wound healed without the slightest suppuration. The sutures were removed on the seventh day, when the child was sent home perfectly well.

6. Same operation as last, on a man. Three sutures; one vessel acupressed; pin removed four hours after. Œdema and slight suppuration, notwithstanding which the sutures were allowed to remain; and on the twelfth day the wound had entirely healed.

This case showed that catgut may be employed with advantage in a different class of cases from that in which I have advocated its use, as, unlike wire, which will cut its way out, and silk, which will resist, catgut will stretch to a certain extent, I might quote many other cases of wounds in other parts, but must content myself with the above-mentioned.

If surgeons will only give catgut sutures a fair trial, at the same time not disregarding other essential precautions to which I shall presently refer, I feel assured that they will have every reason to feel satisfied with the results following their use. The other points to be attended to, in the treatment of incised wounds, are that there be no foreign bodies in the wound—such as blood, dirt, or "dead pieces of meat," as Sir James Simpson quaintly termed ligatured vessels,—and that its surfaces be exposed to the air till they have taken on a glazed appearance. This last indication is not, I am aware, in accordance with the views entertained by many surgeons at the present moment; but experience here has proved that union takes place more readily when air has been freely admitted than when wounds have been hurriedly closed. It is probable that if there are spores in the air of other places, Aberdeen has its due share of them, but they do not exert the baneful influence ascribed to them in some other cities; and the fact that union by the first intention has been obtained here in so many instances of amputation, excision, &c., after they have been exposed to free currents of air, for periods varying in duration from ten to thirty minutes, seems to me to be a proof that the germ-theory of putrefaction in wounds is hardly a tenable one. The only explanation that could possibly be given by the supporters of that theory would be that the air in this city is pure and free from germs; but to explain the why and wherefore of this exemption would be a still more difficult matter. Even if this could be done, it would not be sufficient; for in Edinburgh, where germs are supposed to exist in their highest degree of development, I saw more than one case of wounds after excision of tumours unite *without one drop of pus*,—after Sir James Simpson, not content with the ordinary access of air, used a *bellows* to ensure every part of the raw surfaces being, so to speak, flooded with it. Further, regarding dressings, I find the less the better; when any are employed, a simple strip of wet lint suffices. I have not thought it necessary to enter upon the necessity of obtaining accurate coaptation of the edges of cut parts, as this point is so universally recognised as a *sine quâ non* in the treatment of incised wounds that even mention of it seems superfluous. The treatment I have indicated is applicable to all incised wounds; excepting that I would only counsel the use of catgut sutures in those of the slighter order, or in those where, from the nature of the case, there is reason to expect union by the first intention.

I may add that, as I have received several applications from medical men requesting me to obtain for them a supply of catgut, what I have employed has been prepared by Messrs. Young and Son, North Bridge, Edinburgh.—*Lancet, June* 28, 1873, *p.* 907.

80.—SPINA BIFIDA CURED BY INJECTION.

By Dr. J. R. Watt, Consulting Surgeon to the Fever Hospital, Ayr.

The following case of spina bifida occurred in my practice some time ago, and was considered an excellent opportunity for testing Dr. James Morton's method of treatment by the injection of a solution containing ten grains of iodine, and thirty grains of iodide of potassium in an ounce of glycerine: this, with Dr. Morton's cases, make three treated in this manner, all of which have been successful.

On June 28th, 1872, Mrs. M. R., an anæmic woman, was delivered of her third child, a small and weak boy of full time. In the position of the third, fourth, and fifth lumbar spines, was a sessile semi-transparent flesh-coloured fluctuating tumour, as large as an ordinary-sized hen's egg, and so sensitive that simple contact of the finger brought on fits of crying, during which the investing membrane became very tense. On firm pressure with the finger, a vacant space, bounded at the sides by bony ridges about half an inch apart, was found in the usual position of the vertebral spines; and, when viewed by the transmitted light in a darkened apartment, cords were seen passing near the surface. A pad of cotton-wool was placed over it under a firm binder, and the child was daily seen for a week. It moaned almost constantly, slept little, and took the breast badly. Its legs dangled, and no voluntary attempt was made to move them—slight movement of the toes being the only reflex action from tickling its soles, and the sphincters seemed very weak.

On August 2nd, the tumour was much larger, measuring eight inches in circumference. The most prominent part was ulcerated superficially; the lower border overhung the sacrum. Two drachms of clear fluid were removed by Wood's syringe, (which was used throughout,) care being taken to avoid the nerve-cords. The cotton pad and binder were replaced; and the child was ordered *to have half a teaspoonful* of spirits in water every three hours.

On August 3rd, the tumour was one-half less; from the needle-aperture drops of clear fluid still oozed, and from the soaked condition of the pad and binder a constant escape must have gone on. The child looked much exhausted, but had had no convulsive symptoms.

August 11th. The tumour was as large as before interference. From its upper edge a second quantity of two drachms was removed. The child was kept with the face down on the nurse's knee, with the back uncovered; and the aperture closed in a short time.

August 17th. A third quantity of two drachms was removed, and half a drachm of Dr. Morton's solution was injected. The child cried immediately. The spirits and water were continued.

August 18th. The child had slept very little, and appeared very weak. The tumour was unchanged in size, and along the left border a faint blush was present, which faded in about four days.

August 26th. The tumour was slightly decreased; the membranes were lax, and the fluctuation-wave less distinct.

September 13th. The tumour was nearly level with the back, and was replaced by a large half-dried resistent scab. The child was firmer; the legs never moved together, but when the soles are tickled one or other leg was drawn up a short distance. The sphincters were more under control.

February 21st, 1873. A cake of firm condensed tissue occupies the site of the tumour. The child was much grown, plump, and thriving.

It may be remarked that, even although the tumour and the cavity of the spinal membranes communicated, as shown by the nerve-cords in view, and the greatly depressed fontanelle after the first withdrawal of fluid, yet no irritation spread along the cord from the injected solution, and the result must be considered successful.—*British Medical Journal, April* 26, 1873, *p.* 462.

81.—THE TREATMENT OF ALOPŒCIA.

By Henry Finch, Esq., Colchester.

A gentleman who had lost nearly all his hair after a very severe attack of fever, consulted a French physician of great reputed success as a hair restorer. The prescription was a drachm of the tincture of phosphorus to one ounce of castor oil; the bare spot to be rubbed with this mixture three times weekly for half an hour each time, after the skin of the head had been thoroughly cleansed with warm water without soap. This treatment was faithfully carried out for about six months; the hair soon began to grow, and in a year from the time of first following the doctor's advice his head was as thoroughly covered as ever, the new crop of hair being about two shades darker than the old.

On several occasions I have used the same means, slightly supplemented, and always with some success. I commence with having the few thin, straggling, and colourless hairs on the bared spot cut as short as possible with sharp scissors; the skin is then thoroughly cleansed from scruff by washings in warm water. I then direct a skull-cap of oil-silk or gutta-percha, fitting well round just above the ears, to be made, and in this every night to be applied a warm linseed-poultice, well moistened with castor oil instead of water. In some folk the linseed causes an ugly-looking superficial irritation of the skin, with thick yellow discharge. I then use bread. Such a case I had quite recently in which the linseed caused a straggling, superficial sore, with swelling and discharge, taking some fourteen days to heal, and leaving for some time a disagreeable dark mark. In the morning, when the poultice has come off, the head is washed, and then well rubbed for a quarter of an hour or twenty minutes with homœopathic tincture of phosphorus, half a drachm to an ounce of castor oil, or about half per cent of the phosphorus. Any hairs that appear are carefully cut with scissors. This treatment must be continued three or four times weekly for some months, the head never being allowed to get dry, but from *time to time moistened* with some kind of oily preparation for the hair. It is *in no way inconvenient*, a good thick night-cap of stocking material being worn to avoid soiling the pillow-slips, and keep the poultice *in situ*. Of course if the scalp is polished and smooth as a billiard-ball, and you can detect no delicate thin hairs with a strong magnifying-glass, the probability is that the hair follicles have ceased to be productive, and I believe no treatment can renew their fruitfulness; also in cases where baldness is hereditary, little success is to be hoped for. The poultice continuously applied, with the resulting warmth and moisture, I fancy to be the strong point of the treatment; the oil only three times or twice weekly. And if any one doubts the efficacy of this treatment, let it be tried for a time on the arm or some part of the body where the hairs, though present, are few and weak.—*Lancet, July* 19, 1873, *p.* 101.

MIDWIFERY,

AND THE DISEASES OF WOMEN AND CHILDREN.

82.—ON THE ORIFICES OF THE UNIMPREGNATED UTERUS, AND THEIR SURGICAL TREATMENT.

By Dr. J. Henry Bennet, late Obstetric Physician to the Royal Free Hospital, London.

I will recapitulate in a few words the opinions which I entertain and teach respecting the uterine orifices.

1. *Anatomically*, the uterine cavities are two in number; the cavity of the uterus, triangular; the cavity of the cervical canal, infundibuliform. The cavity of the cervical canal is limited—externally, by a contraction of the os externum, usually patulous, and easily dilatable; internally, by a more decided contraction, which separates it from the uterine cavity, and is called the os internum. This internal contraction, I maintain, is formed by the circular fibres of the cervix, and constitutes a species of sphincter, although no positive sphincter, like that of the anus, is anatomically demonstrable. This double sphincter reproduces, anatomically, the sphincters which close the other large splanchnic cavities—the stomach, the intestines, the bladder.

2. *Physiologically*, these sphincters, especially the uterine one, are vital, close and open by a vital contraction; and a more or less vital contraction and closure is the usual condition during life. After death, this vital contraction ceases, as well as that of the sphincter ani, and must not, therefore, be looked for. These sphincters open for menstruation, probably for conception; and the internal one closes on the contact of a foreign body, such as the uterine sound. They are also more or less susceptible of contraction, according to individual idiosyncrasy. Thus in a perfectly healthy woman (in the interval of menstruation) having a perfectly healthy uterus, the uterine sound or a bougie (No. 8 or 10) separates the lips of the os externum with ease, passes into the infundibuliform cavity of the cervix, where lateral motion is generally possible, and reaches the os internum, one inch and a half from the os externum, there to be arrested, generally speaking, by its vital sphincter-like contraction. Sometimes slight pressure will pass it into the uterine cavity, sometimes it will not. Nothing short of unjustifiable force will succeed; although in these same cases a smaller wax bougie, warmed and bent, will generally pass.

3. *Pathologically*.—Such being the case anatomically and physiologically, the healthy os internum in the perfectly healthy uterus usually resisting the passage of the uterine sound or of a moderate-sized bougie, it is a most egregious pathological error to consider this mere impediment to the entrance of the sound, apart from other morbid phenomena, a pathological condition warranting dilatation or division. So far from resistance to the sound at the os internum being a sign of disease, it is a sign of a healthy natural vital state of the parts. The free and easy passage of the sound into the uterine cavity, without pressure or force, is, on the contrary, generally an evidence of an actually morbid state of the organ. The extension of inflammation of the mucous membrane of the cervical canal to the cavity of the uterus, relaxes and paralyses the os uteri internum; so do endometritis itself, or any morbid state that enlarges the body of

the uterus; metritis, acute or chronic; non-involution of the uterus after labour or abortion; or a fibrous or other growth developed in its tissues. Inflammatory hypertrophy, however, of the cervix may mechanically approximate, partly close, the entire cervical canal. The hypertrophy removed, the canal relaxes of itself.

Such are the views and statements which Dr. Matthews Duncan has stated to be *seriatim* wrong. I repeat that it is of the most vital importance that the real anatomical and physiological condition of women should be cleared up and firmly established as a basis of pathological work. If the above views be right, any healthy woman in the kingdom, casually examined, may be pronounced pathologically wrong, and treated for strictures which do not exist, dilated or divided—a really frightful state of things. Indeed, I firmly believe that hundreds are so treated every year, conscientiously, but under the influence of mistaken ideas as to the anatomy and physiology of the uterus. Indeed, many who have written and practised during the last few years seem to look on the uterine canals as comparable to the waste-pipe that carries off the water from the roof of a house, and to them nearly all uterine pathology is centred in fancied or real obstructions; just as another class of modern gynæcologists had got into their heads that the uterus, the most movable organ in the economy, the "animal in animale" of the ancients, is a mere joint, like the knee or elbow-joint, liable to dislocations, twists, and turns of every kind, and that nearly all uterine pathology lies in these fancied and exaggerated displacements.

Under the influence of these fallacies and errors, I am constantly seeing errors in practice, cases of constitutional life-long dysmenorrhœa (or painful ovulation) divided or dilated by one, treated by pessaries, left for a year or two by another, irrespectively of all constitutional considerations, and with no beneficial result whatever. The dependence of menstruation on ovulation, of ovulation on the nervous system, of the nervous system on diathesis, on hereditary and health antecedents, is often entirely ignored; and in one of the most complex branches of pathology, imperfect surgical notions are often substituted for rational treatment or non-treatment. In many of the cases thus surgically mistreated, actual lesions, important lesions, inflammatory and nutritive, are not unfrequently overlooked and ignored.—*British Medical Journal, May* 24, 1873, *p.* 583.

83.—ON THE TREATMENT OF SUPPOSED STRICTURE OF THE CERVIX UTERI.

By Dr. William Cumming, Edinburgh.

True stricture of the cervix uteri is rare; not so rare, certainly, as is stricture of the urethra in the male who has not had gonorrhœa, but nearly so. Incision of the cerviv, or hysterotomy, therefore, for the cure of stricture, should be one of the rarest operations in obstetric surgery. But of late years it has not been so; and it may be a legitimate inquiry, what purpose it has served when the consequences seemed to sanction its use, which unquestionably they often do.

1. There may be, or rather there is often, a quasi-stricture at the internal os, when there is congestion, hypertrophy, or other disease of the lining membrane of the uterus. This condition is relieved temporarily—it may even be cured permanently—by the hemorrhage resulting from the incision. In my own experience, the relief generally is only temporary.

2. The same condition of quasi-stricture exists when there is hypertrophy of the body of the uterus—in many instances induced by the efforts to expel the clotted menstrual and the accumulating leucorrhœal discharges. In such cases, the hemorrhage, coupled with adequate general treatment, reduces the hypertrophy, prevents accumulation, and helps to restore the healthy state of the lining membrane, and so relieves the supposed stricture.

3. The preceding condition is almost invariably connected with enlargement

and congestion of the ovaries, either as cause or as effect; and the same hemorrhage reduces and relieves this.

But the operation, if effectually and thoroughly performed, is one of very considerable risk, especially when followed by the introduction of sponge or tangle-tents. The interesting mechanical, or rather dynamical, experiments of Dr. Matthews Duncan prove with what force these bodies act; and it is not difficult to estimate (if it were not sufficiently and disastrously confirmed by experience) the mischief that must in too many cases result from the laceration and irritation of the delicate textures cut into by the incision. How often this mischief has succeeded the operation, probably no one knows. Is, therefore, the operation altogether unjustifiable? I do not say so; but, as a *cure* of this quasi-stricture, it should be almost the rarest operation in surgery. Is it justifiable as a cure or relief of the conditions I have alluded to above? I believe not, for three reasons: 1. Because it does not, as a rule, either cure or more than temporarily relieve these morbid states; 2. Because it is always attended with danger; and that danger is increased the nearer the incision approaches to the internal os and the various plexuses adjoining, where the supposed need for the operation exists; 3. Because there is a much more simple, and in my opinion and experience, less dangerous, treatment that may be employed with a larger probability of success. I cannot say utterly without risk, because every gynæcologist knows that, while there are some uteri to which almost anything may be done with impunity, there are others to which nothing can be done without startling, even fatal results. I have known the gentle introduction of a moderate sized bougie lead to violent metritis, extending to the appendages, and not recovered from without permanent mischief; and I have also been cognisant of the same leading to acute suppuration of the ovary. But these results are so rare that, except as warnings to be both preparatory and cautious, they need scarcely be taken into account; and my present belief is, that if the gentle depletion I am disposed to advocate were employed before using the bougie, such complications would, probably, never occur.

Assuming, therefore, that true stricture is extremely rare, and that incision of the ora and cervix uteri, with subsequent dilatation by tents, is unnecessary, and may be, and often is, injurious, but that the hemorrhage resulting from it is beneficial, I have long practised, with fair success, scarification of the os uteri, followed by dilatation with graduated bougies. By scarification the amount o blood abstracted can be limited; as much or as little can be taken away as is thought necessary (which with leeches cannot be done); congestion of the lining membrane of the womb and of the ovaries is relieved and the passages are relaxed. By this means the bougie is more easily introduced; certainly requires less force in its use; does not rouse or increase irritation of the canal and interior of the uterus, probably even soothes them: and if there be any deposition of lymph environing the cervix, facilitates its absorption.

It is satisfactory to observe that there is now a tendency to return to the more simple and less meddlesome treatment of uterine disease, and to discourage heroic modes, the details of which are sufficient to make one's hair stand on end.—*British Medical Journal, April* 26, 1873, *p.* 461.

84.—ON THE VALUE OF THE CORPUS LUTEUM AS A PROOF O IMPREGNATION; WITH A CASE IN WHICH AN UNIMPREGNATED OVUM WAS FOUND IN THE VIRGIN UTERUS.

By Dr. William T. Benham, Assistant Medical Officer, City and County Lunatic Asylum, Bristol.

Since it has become a rule in the principal lunatic asylums of this country—and in this one amongst many others—to make careful post-mortem examinations in every available case, much light has been thrown on certain pathological conditions which were before clouded in obscurity; and there can be no doubt, that much more may yet be demonstrated by the careful registration of all such appearances as may from time to time be seen by individual pathologists in their special departments of medical science.

It is not the good fortune of many observers to meet with such a pathological phenomenon—so rare indeed as to be almost, if not quite, unique—as was seen at the examination of the body of a female patient, who died on the third day of the catamenial period, in the status epilepticus, in the Asylum for the City and County of Bristol. This case, I am sure, will be of great interest to many of your readers, throwing light, as it certainly does, on a medico-legal question of great importance; and I would just premise, that whatever is demonstrable by the facts here recorded, is all the more valuable from the conditions which surrounded the patient at, and for a long time previous to, her death; making it quite impossible that the pathological appearances observed in this case can be explained away by there being the slightest doubt of her having had sexual connexion with any one for at least several years before.

Charlotte S., æt. 25, single; of no occupation; was admitted into the Bristol Lunatic Asylum in December 1864, suffering from epilepsy. She died on the 26th of March 1873. During the whole of that time she was subjected to periodical attacks of epilepsy with excitement, which usually came on a day or two prior to the appearance of the catamenia, which were generally of a normal character, and which ceased at their termination. On the 22nd of March, having complained the day before of not feeling very well, she went into the status, and remained in bed in a single room, attended by a nurse, from that time until her death. A mixture of brandy, træ. sumbul., and spt. æth. sulph. (which is usually given here in such cases with good effect), was ordered to be given her every three hours, but she never rallied, and died in the afternoon of the 26th. On the morning of the 23rd it was found that she was menstruating, and was still in that state at the time of her death.

At the autopsy, which was made forty hours after death, the surface of the whole body was found to be much congested, and thickly scattered over the front of the thorax were numerous minute extravasations resembling flea-bites; there was also considerable sugillation of the depending parts; the internal organs, the brain, spinal cord, and their membranes, were all intensely congested; the case thus far presenting the appearances usually found when death has taken place under similar conditions; but the results of the examination of the generative organs were such as to make it incumbent on me to lay them before the profession, and to offer a few remarks on their physiological and anatomical bearing. I can only regret that this most interesting case did not fall into abler hands than mine, so that a more elaborate account might have been given of it, and thus have made it of more value and interest than any attempt of mine can do.

On examining them externally, the organs of generation presented in a marked degree all the highly characteristic signs of virginity. On removing the uterus, ovaries, and Fallopian tubes, they were found to be in a state of intense congestion, the superficial veins standing prominently out, and being filled with dark-coloured blood. The left ovary was more congested than the right, and on its upper anterior surface, situated rather more internally than externally, was seen a bean-like prominence, surrounded by a plexus of blood-vessels all the more apparent from their congested condition; it was of a polished appearance, and of rather a bluish shade of colour. On examining this body it was noticed that its upper surface presented a small nipple-like projection, apparently formed by the distension of a small portion of the peritoneal covering of the ovary, and containing some soft distending material; some small blood-vessels could be observed ramifying round the base of it. At the centre of this prominence was a small irregular dark spot as if a small orifice had existed there, and had been closed up by a clot of blood. On making an incision through its long diameter, there was at once seen an oval shaped cavity filled by a mass of partially decolorised fibrin, and entirely surrounded—except at the point corresponding to the nipple-like process—with a thick yellow substance of considerable firmness, pierced by a number of small blood-vessels, which sprang from the vascular plexus surrounding it externally; and on using a magnifying-glass of low power, these minute vessels could be seen traversing its substance, and losing themselves on its inner edge, which was contiguous to the surface of the partially decolorised fibrinous clot filling up the cavity. On measuring this cavity it was found to be five-eighths of an inch in its long, and three-eighths of an inch in its short diameter. The yellow substance surrounding it was found to be of the uniform thickness of one-eighth of an inch, the whole structure measuring seven-eighths of an inch in its long and five-eighths of an inch in its short diameter. On again examining this yellow substance, it was seen to be of a firm granular-like matter, with many minute oil-globules scattered over its surface; and in parts where the blood-vessels were the most numerous, they gave it a pinkish yellow appearance. It will be at once seen that we have here occurring in the virgin ovary a corpus luteum, possessing such decided characteristics as to make it *per se* quite indistinguishable from the so-called *true* corpus luteum of impregnation of the same period, asserted by many writers—Paterson, Lee, Montgomery, Bernand, Deschamps, Müller, Ramsbotham, and others—never to occur in any but in the ovary of an impregnated female, and consequently regarded by them as an infallible proof of a recent pregnancy.

Hoping almost against hope, that the ovule, which had evidently been only very recently extruded, might still be found in the cavity of the uterus, a section was made through its anterior wall, and, on laying it open, there was seen a small circular body of a reddish-white colour, almost buried in the pulpy purplish-coloured decidual débris, which thickly covered the whole of the anterior surface of the uterus. On carefully removing what I could of this decidual matter from its surface, and examining it with an inch lens, it appeared to be more of a pinky-white colour, and having a fine velvety surface, which, under a higher magnifying power, was seen to consist of multitudes of white filaments. It measured, with the decidual matter surrounding it, and which could not be entirely separated from it, one-twentieth of an inch in diameter, and was situated a little more than seven-twelfths of an inch above the os internum, and slightly to the right side of the median raphé of the uterus. As I have already said, under a higher power its surface was seen to be composed of very short white filaments, which gave it much the appearance, only on a smaller scale, of one of the earliest impregnated ova on record, discovered and described by Velpeau as measuring about five-twelfths of an inch in diameter, and the filaments of which were so far developed as to measure one-twelfth of an inch in length. This ovum he stated to be fourteen days old.

On making a section through this ovule, with the object of examining it more minutely, I noticed that a very small quantity of albuminous fluid escaped from it, and it lost its globular form directly. A partially collapsed cavity of very minute size was seen to occupy its centre; and although I carefully examined the excised ovule and the fluid which had escaped from it, I could discern nothing more of importance.

It would be interesting to know at what time this ovule was extruded—before, at, or on the day after the time of the commencement of the catamenial flow? Whether it underwent the same external changes in its passage to the uterus as the impregnated ovum? Whether, irrespective of impregnation, an ovule in its descent usually takes on the velvety appearance above described? How long it remains in the uterus before passing away? and whether it passes off in the menstrual fluid entire, or in a state of dissolution? Whether the ovule theory—that at each menstruation an ovule is discharged, as in this case—is capable of demonstration by facts which are as yet undiscovered in the human female? One solitary case like this will not go far to answer any of these queries; but it does prove this much, that an unimpregnated ovule can and has descended into the cavity of the uterus, and remained there for some days without being, as stated by some, immediately washed away with the menstrual fluid, or without, as stated by others, having undergone such rapid dissolution as to have immediately passed away; and, what has been denied by many, that an ovule is ever impregnated in the cavity of the uterus itself, is now made extremely probable. There can I think be no doubt that, had this girl had sexual connexion, this ovule might certainly have become a fecundated ovum; unless, indeed, which has never yet been demonstrated is the case, that the menstrual fluid, through which the spermatozoa would have to pass to reach the ovule, possesses the power of destroying these bodies immediately on coming into contact with them. Before passing from this subject, let me state that I think this individual case goes a long way to ake it probable that where an ovule is extruded it takes place at the commence-ent of menstruation more frequently than at any other time. This ovule had evi-ntly been extruded for some time – that is, not less than two days; and that it had t been extruded immediately before death is evident from the fact of its having been und in the cavity of the uterus, embedded in the decidual débris. It must, there-re, have taken some time to have been conveyed there, considering the dis-nce it had to travel; and, besides, the condition of the fibrinous clot filling e cavity it left makes it certain that it had been extruded at least two days reviously. It appears, then, that in this case the ovum was extruded at the mmencement of menstruation; and that it is usually so in other cases seems, think, to be indicated by this, that at the time of, and for a day or two pre-ously to menstruation, a much greater supply of blood flows to the ovaries an at any other period; and the fact of the distended Graafian vesicle shar-g in the increased vascularity of the whole structure at that particular time, akes it extremely probable that the extra pressure then put upon the captive ule should be sufficient to cause it to burst its coverings and to become extrud-d, and should it not be sufficiently developed to do so at that period, in all robability it would not become extruded until the ovary should be again the bject of increased vascularity; and this seems to show the reason why a enstrual period often passes without the extrusion of an ovule at that particu-r time. It would appear, then, that the increased vascularity which takes ace at the menstrual period goes a long way to explain the mechanism by hich an ovule is extruded more frequently at that than any other time; also, ere can be no doubt that the increased vascularity is of great importance in roducing those changes in the Fallopian tubes and their fimbriated extremities, hich result in those parts of the sexual apparatus taking on the turgid and rectile character necessary for the due performance of the function of grasping e extruded ovule and of conveying it to the cavity of the uterus. But where, cases by no means uncommon, an ovule is extruded and becomes impregnat-d at an *inter-menstrual* period, I am bound to believe, from instances which ave come under my own observation, that it is owing to the fact that, from ome exciting cause more or less potent, the ovaries have taken on an increased ascularity of action resembling that incident to menstruation, and sufficient to xtrude a well-developed ovum at an abnormal time. Thus, the pleasures and xcitement of the opera or the theatre, of the "mazy dance" and supper-rooms, f the merry dinner-party, or of the return home after a prolonged absence, ave doubtless often been the cause of that increased rush of blood to the ova-es and Fallopian tubes, which has resulted in the extrusion and possible im-regnation of an ovule at an inter-menstrual period. In support of this I may ust mention a case which has occurred since the foregoing was written, afford-g considerable negative evidence that such is the case:—E. T., aged 26, an pileptic, died under precisely similar conditions to the last patient; but al-hough her death happened on the fourth day of menstruation, her uterus, ova-ies, and Fallopian tubes presented only very slight, though sufficient evidence f such being the case; there was only a small degree of increased vascularity, nd on examination it was quite evident that no ovule had been recently ex-ruded, although the right ovary contained a Graafian vesicle of such mature evelopment, that it was easy to conceive that an ordinary amount of ovarian ascularity must have resulted in its extrusion; but the fact that only the very lightest degree of congestion was present would sufficiently seem to indicate he true cause of the non-extrusion of the ovule in this particular instance. I efore suggested that the non-extrusion of an ovule might be, and probably vas, due to the state of the development—or rather of the undevelopment—of he ovule itself; but it would now seem that the *amount of ovarian vascularity* as also much to do with it: for in this case, although there was a fully ripen-d Graafian vesicle on the surface of the ovary, the amount of congestion was oo slight to have any effect sufficient to cause the extrusion of its ovule. I vould also add that there were the remains of two former corpora lutea, well narked in character, the one more so than the other; and I think it extremely robable that they were the result of the two previous menstruations, which I scertained were of a *severe character*, and accompanied by considerable proof f a *well-marked vascular disturbance.*

Let me now pass on and state, in as few words as possible, the bearing this case has on the question of the value of the corpus luteum as a test of impregnation.

It has been observed by Dr. Dalton, that the corpora lutea following menstruation and impregnation, for some time take the same course of development; and that there is the same rupture of the vesicle, the same extravasation of the blood, followed in either case by the gradual absorption of the colouring matter of the clot, and hypertrophy of the membrane of the vesicle; and thus far he agrees with those who look on the sign of the so-called true corpus luteum as fallacious; but then he goes on to state "that the corpus luteum of impregnation continues to develop itself, and does not become decidedly retrograde until the termination of the pregnancy, whereas the corpus luteum of menstruation reaches its maximum of development at the end of three weeks, and then rapidly becomes atrophied." There is, however, considerable doubt on this point, and it is by no means clear that such is often the case. Bischoff, amongst many other observers, may be quoted as believing that the so-called true corpora lutea may be produced independently of impregnation, and that they consequently cannot be received as proofs of pregnancy. Casper believed that the corporea lutea of menstruation "were in no way distinguishable from those of impregnation;" and Kirkes states that similarity in the character of the two kinds of corpora lutea "necessarily renders unsafe all evidence of previous impregnation founded on the existence of a corpus luteum in the ovary."

That these authorities are right in their conclusions, this case, I think, materially helps to prove. The fact of a corpus luteum of menstruation having been found to so exactly imitate those of impregnation, if, indeed, this was only a *solitary* instance, must considerably weaken the theory still held by many, that a so-called true corpus luteum is a sure sign of impregnation. When we come to consider why impregnation should make such a vast difference in the appearance and structure of the corpus luteum resulting therefrom—as is said to be the case—it is by no means easy to see the reason why such should be so.

If it could be proved that in every case in which impregnation takes place, it does so whilst the ovule is still in its vesicle in the ovary, and at no other time or place whatever—and this, connected with the fact that the corpora lutea in the impregnated female as a rule possess such decided characters—it would be only fair to suppose, although, perhaps, even then difficult to demonstrate with our present knowledge of the subject, that the proximity of the fecundating element which causes such vital changes in the ovule itself, might have some sufficiently potent action on the structure immediately surrounding and in actual connexion with it, to account for the characteristic changes they are known to undergo. But that it is not the case that fecundation takes place in the ovary alone is sufficiently proved by the fact of the unimpregnated ovule having been found, as in this case, elsewhere than in the ovary, at the same time possessing all the highly characteristic features which distinguish it, when it is examined, in the Graafian vesicle itself, and therefore quite as capable of undergoing fecundation when brought into contact with the necessary elements. Granted, then, that the ovule may become fecundated away from the ovary itself, we cannot suppose that the fecundating material in that case would have any direct material action in the formation of the corpus luteum, which would, in that particular case, be found to exist; but under such circumstances the resulting corpus luteum, not having come into contact with the fecundating material, would certainly have the same formation as it would have did fecundation not take place at all. Can we, then, be surprised that, subject to the same early conditions—subject to the same absence of contact with any fecundating material—the corpus luteum of the ovule, which has passed on into the uterus, and has only received impregnation after remaining there for some time, should exactly resemble the corpus luteum of the ovule, which has passed in the same way to the uterus, but which in the end has not become impregnated at all? We must, therefore, conclude, granting the absence of the fecundating element in both cases, that there is no reason why the corpus luteum of menstruation should not exactly resemble the corpus luteum of impregnation in all such cases as come under the conditions I have named. In fact, it does so, as I hope I have sufficiently demonstrated; and, consequently, it follows that the presence of a so-called true corpus luteum, of at all recent formation, in the ovary of a female, has not the slightest legal value whatever in determining the question as to whether impregnation has taken place or not. If that is proved, as I believe it is, by the facts I have brought forward, it should be of considerable importance in its medico-legal as well as in its anatomical and physiological bearings; for, in one case at least recorded by Dr. Guy, the existence of a corpus luteum was held to *prove* that conception had taken place, when the uterus itself presented not the slightest signs of such having been the case.

Much more might be written on the facts I have endeavoured to bring forward, but so much has at various times been said on the subject, that it has not been my object to go over that which has, from time to time, been written by others; but rather to produce such new facts, valuable in themselves, as my case demonstrates, with the hope that they might, instead of being buried in obscurity, be of service to those who make this and allied subjects their especial study.—*Edinburgh Medical Journal*, *Aug.* 1873, *p.* 127.

85.—A SAFE METHOD OF INDUCING PREMATURE LABOUR.

By Dr. Beverley R. Morris, Nottingham.

[Supposing it decided to bring on premature labour in any given case, what is the best method of effecting it? No doubt the nearer the natural process of parturition can be imitated the more successful will be the result.]

The process that I have to describe is an adaptation of the third method of inducing labour—that is, by galvanism; but, as far as I am aware, by an entirely different application of the principle from any before attempted. The principle involved was introduced by Mr. Dancer of Manchester, many years ago, for the purpose of arresting *post partum* hemorrhage, and this it undoubtedly effected satisfactorily: but the apparatus was so cumbersome, that few practitioners could carry it about with them, and, probably from this cause, I do not think it was ever generally used. The instrument invented by Mr. Dancer was so arranged that one pole of the galvanic current could be introduced into the uterus, while the other was applied over the abdomen; it was so constructed that either a continuous or an interrupted current could be applied. The instant effect was a powerful contraction of the uterus, and a consequent cessation of hemorrhage.

The application of this principle to the induction of premature labour was made by Mr. John Varley, surgeon, of Nottingham, through whose kindness I am enabled to exhibit the instrument as modified by him. The mode of using this instrument is to insert the metallic point within the os uteri, and then, placing the other pole to the abdomen, pass a slight continuous current through the uterus for ten minutes or a quarter of an hour. This induces a dilatation of the os, which is further increased substituting a larger conical point, and again continuing a gentle current for a few minutes. In each case in which this method has been used so far, labour has followed in two or three days; but, should this not be the case, it will only be requisite to apply the current daily until it does. The safest way is to expose the os uteri by a speculum, and then insert the point of the instrument through the speculum, which may then be withdrawn over the instrument. The great portability of the instrument and battery will allow it to be readily carried in the pocket, and it is always ready for use at a moment's notice; and the induced current seems to me amply sufficient for the purpose. The arrangement for giving a continuous or broken current is very simple, and entirely and instantly within the operator's power.

The instrument consists of a metallic sound, covered, except at the point, with a non-conducting material, and having a metallic connection at the handle, and so arranged as to be either broken or continuous by a touch of the finger. This intermediate part is connected with one pole of the battery; while the other pole is attached to a metallic tube or conductor for external application, either direct, or through the hand of the operator. The extreme portability of Gaiffe's battery led Mr. Varley to this adaptation, which, I trust, will soon be widely used, and lead to a safer and more efficient method of inducing uterine contraction than any at present in use. It is manifest that there are other cases in which it may be most usefully applied, as, for instance, in sluggish or atonic labour, and other similar states.—*British Medical Journal, Sept.* 6, 1873, *p.* 276.

86.—WILL IT BE A BOY OR A GIRL?

[We extract the following from Dr. Braxton Hicks's address in the section of obstetric medicine at the meeting of the British Medical Association in London last autumn.]

The interesting fact that the sex of the child can in a large proportion of cases be ascertained by auscultation during pregnancy has also lately been made known. When the fœtal pulsations number 144 per minute, the child is probably a female, when they are 124 per minute, probably a male. It is said that a little variation from 124 upward, and from 144 downward, will not alter the diagnosis, provided auscultation be practised towards the end of pregnancy. Steinbach was correct in forty-five out of fifty-seven cases which he examined, while Frankenhäuser was right in all the fifty cases which he examined with a view to determine the sex of the child.—*British Medical Journal, Aug.* 16, 1873, *p.* 187.

87.—ON THE COMPARATIVE ADVANTAGE OF FORCEPS, OF TURNING, AND OF PREMATURE LABOUR IN CONTRACTED PELVIS.

By Dr. Angus Macdonald, Lecturer on Midwifery and Diseases of Women.

Before attempting any statement of opinion regarding these points, let me briefly go over the clinical history of two women, whose cases are brought forward more as a theme from which to start the discussion, than as data on which to found deductions.

Case 1.—That of Mrs. D. is peculiarly interesting. She is at present in exceedingly good health and spirits, buoying herself up in the hope that her next labour may be twin-boys, as her mother had twins once. Should this occur, she believes she may yet have a living boy, which is an object of great ambition with her. Her age is 41. She was married on the tenth of March 1854, since which time she has given birth to nine children at full term, to two at between 7½ and 8 months, and has had three abortions, which make in all 14 conceptions. She is of middle size, strongly made, and apparently well formed. Her pelvis, however, is very much contracted in the conjugate diameter of the brim, though roomy at the sides, and also roomy and regular at the outlet. The narrowing is effected by jutting forwards of the promontory of the sacrum, and is such as to contract it to close upon three inches so far as one can judge. The account I give of her confinements is, with the exception of the last three labours, obtained from her own statement, so that if there are mistakes in regard to any of them, some of the members of this Society, who happen to be referred to by her, will, I hope, put me right.

Her first confinement took place on 19th of December 1854, when she was delivered of a female child. She was three days and three nights in labour. On the third day it was agreed upon between Dr. Pattison, who had charge of her case, and Drs. J. Y. Simpson and Sidey, to meet at 7 p.m., with the view of effecting delivery, provided the natural efforts were not successful before then in terminating the labour. She, however, gave birth to a living child, unaided, an hour before the appointed meeting. Her recovery was very slow, but complete. The child lived for several years, and died of hooping-cough.

This pregnancy was folllwed by two abortions. Her second confinement took place on the 2nd February 1858. Dr. Sidey attended her. She lingered for a whole day, and Dr. S. waited with her all night, till between two and three o'clock the following morning, when Dr. Keiler and he having chloroformed her, first attempted to deliver by instruments, but finally completed the labour by version; the child was a female, and is still living. Her third confinement occurred on the 17th June 1859. Dr. James Sidey, along with the late Mr. William Scott Carmichael, attended. She was delivered by turning, under the influence of chloroform. The child was a male, and dead.

She was delivered by turning, under chloroform, of her fourth child, a girl, and living, by Dr. James Sidey, on 19th March 1861. In the end of this year she had an attack of bilious fever, and in January 1862, was delivered of a female child, at about the seventh month. This infant survived only nine hours. The delivery was easy, and unaided. On 29th January 1863, Dr. Andrew Inglis, assisted by Dr. James Sidey, induced labour at the eighth month. In this case neither instrumental assistance nor turning was needed. The baby was a female, and though always weakly, lived till she was eighteen months old, and died in teething. The mother disliked the necessary interference, in connection with the induction of premature labour, so much that she ever since declared that she would rather die than again submit to the same ordeal. In her seventh pregnancy she was allowed by Dr. Andrew Inglis to go on to the full time, chloroformed, and delivered, by turning, of a female child, which lived. This happened on the 6th December 1865. On the 23rd of May 1867, Dr. Andrew Inglis again delivered her of a living female child, at full term, by version and chloroform.

On the morning of the 29th June 1869, I was hastily summoned about 6 a.m., to attend her. She had previously insisted that she must be delivered by turning, and as Dr. Andrew Inglis, who had handed over the case to me, was of the same opinion, it was agreed that on this occasion, I should deliver her by turning. The os uteri was completely dilated, and the water had escaped about half an hour previously to my arrival. The pains were excessively severe, yet the head had never entered the pelvis, and in the interval between the contractions was quite movable at the brim. I chloroformed the patient, turned with no difficulty whatever, and delivered her of a male child, which, however, never breathed. The promontory of the sacrum left a deep indentation on the child's head. Great force was needed to bring the head through the brim. Mrs. D. made a speedy recovery.

On the 22nd of October 1870, I was again called hurriedly to attend her. I found her in labour: the os fully dilated, the pains severe, but the membranes unruptured. I accordingly chloroformed the patient, and was proceeding to effect delivery by version, but on introducing my hand through the membranes, I found I had to deal with an ill-developed fœtus, floating in an excessive collection of amniotic fluid. On allowing this to escape, a small male foetus was expelled, which made a few faint efforts at breathing, and then died. On the 2nd of January 1872, she had an abortion at about the third month.

On the morning of the 28th of January 1873, she was delivered by me by turning, under chloroform, of a living female child. On this occasion also great force was needed to pull the child's head through the brim. The side of the child's head, which looked posteriorly, was deeply indented by the sacrum, and the consequent depression on the skull still remains to a certain amount. On this occasion I must confess that I was surprised as well as delighted when the child gave indications of life. The infant remains a healthy strong child.

2.—Aged 25, was seized with labour pains on the forenoon of 22nd 871. My then *pupil, now Dr.* Shapter, of Exeter, sent for me late in ing. *On my arrival I found* the os completely dilated, the pains very severe, but the head still stood over the inlet, and refused to enter the pelvis. The sacral promontory was very easily reached, and the whole pelvis—as, indeed, the woman's whole person—was remarkably small. After a little consideration, I thought that I should best succeed in effecting delivery by turning. Version was effected ultimately, but with some difficulty, owing to the spasmodically contracted condition of the uterus. The child had to be removed with the exertion of great traction force, and it took me nearly half an hour of interrupted pulling before I got the head clear of the pelvis. On an examination of the child after delivery, it was found that the connection between two of the cervical vertebræ had yielded during the extraction. The mother made a good recovery.

From the want of success in this labour, it was resolved, with the patient's consent, to endeavour to bring home a living child in the next pregnancy by inducing premature labour. In this case, my then pupil, now Dr. Oswald Wood, had the general charge of the case. A condensed abstract of his report of it is as follows: "It having been deemed proper to induce labour at about seven and a half months of pregnancy, on the evening of 20th April 1872, Dr. Macdonald began the dilatation of the cervix by means of Barnes's gutta-percha dilators. Numbers 1 and 2 were introduced successively, and so far the dilatation advanced normally. But after the removal of No. 2, the cervix threatening to become spasmodic, it was deemed inadvisable to proceed to No. 3, and after introducing a flexible catheter between the uterus and the membrane and leaving it there, the case was left till next evening. At that time Dr. Macdonald, accompanied by Dr. Affleck, succeeded in effecting nearly complete dilatation of the cervix by No. 3 dilator.

"Pains by this time became established, but were much more of an irritating than of an expulsive nature; and as the head was, after waiting for an hour or two, making no progress, delivery was finally effected by the long forceps. The child came round and cried loudly, but died in about six hours. The patient on this occasion also recovered excellently."

This woman again becoming pregnant, and having a distinct aversion to a repeated attempt at saving her child by premature labour, I resolved to wait till the full term, and then attempt to deliver by forceps, leaving craniotomy or cephalotripsy as the alternative in case I failed with forceps. Accordingly, the patient having been seized with labour pains at full term, on the 4th May 1873, she was attended by my pupil, Mr. John Bisset Smith. She had weak pains throughout the day, but these became exceedingly severe between seven and eight o'clock p.m. The membranes had ruptured immediately before ten p.m., at which hour I was called to attend her. I found the head presenting at the brim of the pelvis, with the sagittal suture approximating the transverse diameter of the inlet, with the occiput towards the left side of the mother. The head at this stage exhibited well-marked Roederer obliquity. At half-past eleven, as the head was making no progress, although the pains were long, strong, and frequent, and the cervix completely dilated, fearing rupture of the uterus in case of further delay, I applied long forceps, and after a pretty tight pull for the space of a quarter of an hour, succeeded in effecting delivery. The forceps were found to have been so applied that one blade was over the right eye, one limb of the blade being in a line with the ridge of the nose, whilst the other blade had become fixed behind the left ear, which proved the truth of the observation in regard to the transverse position occupied by the head at the brim. The child, a large, well-developed female, was semi-asphyxiated, but soon began to breathe under the ordinary restorative applications. The placenta, along with some clotted blood, was speedily expelled by the natural mechanism. There was for some time slight hemorrhage. The patient and her child both did well.

The conjugate diameter was, on the strictest examination, made both by Dr Affleck and myself, after her second confinement, determined to be about 3 inches; the diagonal conjugate, 3¾ inches.

The first remark I am inclined to make in reference to these cases is, that i seems to me that we are too apt, in Great Britain, in deciding as to the preference of one operation over another, to fix our attention exclusively upon th amount of contraction in the antero-posterior diameter, without, at the sam time, keeping before us the presence or absence of contraction of the other diameters of the pelvis. Though there is no doubt that, generally speaking, though not always, the conjugate is the narrowest diameter in deformed pelves, yet according as we have a contraction in the antero-posterior diameter merely, or general smallness of the pelvis, with or without a marked contraction of the sagittal at the brim, the operation of turning is followed with very different degrees of success. Now, the pelvis, in Case No. 1, presents to my mind a good example of the transversely flattened pelvis, roomy at the sides, but markedly contracted in the antero-posterior diameter of the brim. Such pelves, as they afford a considerable amount of room for the occiput to occupy in one or other of their sides, present favourable conditions for successful turning, while, at the same time, the narrow conjugate renders them somewhat less suitable for the application of forceps. In confirmation of this statement, it is to be observed that of all the three operations of forceps, premature labour, and turning, the latter gave the best results in this case. But No. 2 being one of generally contracted pelvis, possibly, however, or rather probably unequally so, was very unsuitable for turning, whilst it gave an opportunity of effecting good results by forceps, and perhaps also better by waiting a considerable time before applying the instruments. The view that this pelvis belonged to the class of generally contracted pelvis, is made evident from the extreme flexion or Rœderer obliquity of the head, which was noticed in the third confinement. Such pelves are exceedingly ill suited for delivery by version. This is prominently pointed out by Borinsky, Archiv für Gynäkalogie, B. iv. s. 230. That British authorities, although no doubt aware of this fact, do not point it out with sufficient clearness, is, I think, unfortunate. At any rate, it was not sufficiently prominently before my mind three years ago, else I would certainly have preferred forceps to turning in the first confinement of Mrs. N.

It is also very curious to notice how small an increase of the size of the child's head is found to be an obstacle of the most serious nature in cases of contracted pelvis. It will be observed that of Mrs. D.'s children, not a male was able to be brought to the world alive,—the great variety in her medical attendants may fairly be held as eliminating all specialty in respect to relative skill in treatment,—whereas almost all the female children lived.

And now, a few words on the general proposition of this paper. In regard to the operation of premature labour in connexion with pelvic deformity, two very able statistical papers have lately been published in Germany by Spiegelberg and by Litzmann, in the first of which it is broadly asserted that artificially-induced premature labour gives, in contracted pelvis, on the whole, very much worse results than allowing the cases to be terminated by the unaided natural efforts.

The results of Spiegelberg's collection of recorded cases of labour in contracted pelves are decidedly against the operation. The fatality in cases of contracted pelves generally he finds to be 6·6 per cent. of mothers, and 28·7 per cent. of children. The mortality, on the other hand, in artificially-induced premature labour generally, he finds 18 per cent. of mothers, and 66 per cent. of children; whilst the operation of premature labour undertaken for narrow pelves only gives a resulting mortality of 15 per cent. of mothers, and 66·9 per cent. of children. The percentages embrace a very large number of cases, from a wide circle of operators; the total number of labours with narrow pelves being 1224; of artificially induced labour for all causes, 271; of the same for narrow pelves, 219.

Litzmann, in the elaborate article already referred to, is inclined to believe that the results of carefully-selected and well-managed cases of artificially-induced premature labour ought not to prove so fatal to the mother as Spiegelberg makes out. But that the fatality to the children, taking into account not merely the number born dead, but the number who die within the first week or two after artificial premature labour, is quite as great as Spiegelberg makes it.

Of 373 cases of contracted pelves recorded by him, and variously treated, 6·9 per cent. of mothers, and 20·3 per cent. of the children died. Of the same cases, 34 of artificially-induced premature labour gave a maternal mortality of 14·7, *and a fœtal mortality of* 55·8. He divides deformed pelves into four grades of contraction. His *first grade* of contraction includes—simple flat pelves with conjugate = 3·7—3·3 inches; generally narrowed, flat pelves with conjugate = 3·9—3·6 inches. Of these he records 316. Total mortality of mothers, 5·06; of children, 12·6. From artificially-induced premature labour, of which there were two cases, the maternal mortality was *nil*, but one of the children died = 50 per cent.

His second degree of contraction embraces—simple flat pelves, conjugate = 3·3—2·9 inches; generally narrowed, flat pelves, conjugate = 3·6 inches. Of 47 labours with pelves of these dimensions, the total mortality of the mothers was 10·6 per cent.; of the children, 61·7 per cent. Those deliveries effected by premature labour artificially induced gave a mortality of mothers 7·4, of children 51·8.

Classifying still according to degree of pelvic contraction, his results from 8 labours with pelves contracted to his third degree, which embraces—simple flat pelves, and generally contracted, flat pelves, conjugate = 2·9—2·2 inches. Total mortality of mothers, 50 per cent.; of children, 75 per cent. Of five cases of artificially-induced premature labour, 2 mothers died, or 40 per cent., and 4 children, or 80 per cent.

Of his fourth degree of contraction, which embraces pelves with a conjugate of 2·2 inches and under, it is not necessary to speak. Litzmann records only 2 cases, 1 in which delivery was effected by Cæsarean section, and mother and child were lost; and another in which spontaneous premature labour came on, mother living and the child being lost. It would appear, however, from the statistical results both of Spiegelberg and of Litzmann, that spontaneously originating premature labour gives considerably better results in contracted pelves than when it is artificially induced. This result is obviously referable to irritation of the mother's parts by the various methods employed to bring on labour. It seems also evident to me that Litzmann's habit of practising, more particularly the method of intrauterine injection of fluid, has something to do with several of his unfortunate cases. Litzmann concludes as follows:—"The results of my investigations, as bearing upon practice, I should wish briefly thus to formulate. The artificial induction of premature labour in narrow pelves is offered especially in the interest of the mother; the saving of the child by its means is at least doubtful. The operation is, even in primiparæ, indicated in the second degree, and, at most, just at the commencement of the third degree of pelvic contractions" (*i.e.*, from 3·3—2·9 inches in conjugate); "in the first degree (3·9—3·3 inches conjugate) it can only be defended through the existence of specially difficult relations and complications."

It will thus be seen that while Litzmann—than whom no obstetrician in Europe is better entitled to speak in a matter involving contracted pelves—is of opinion that premature labour may give the mother a better chance than doing nothing, so far differing from Spiegelberg, yet the fœtal mortality is simply enormous. Now, as premature labour has always been argued for, most of all on the ground that it gave the child a better chance, without bringing additional danger on the mother, it seems to me that Litzmann's apology in its behalf is rather weak. There is a general impression throughout the profession that premature labour ought now to be much more seldom induced than formerly. I think it is a right one; and, from such records as I have brought before the Society, it seems to me that it were better for the mothers, and much better for the children, that the operation in connexion with deformed pelvis were altogether given up. I do not profess to speak from much personal experience, but from reading and oral evidence. I have seldom found cause to employ it. This I do not regret; for, so far as all the information I can gather from reading and otherwise goes, and from the little practice I have had in it, it seems to be proved incontestably, that on very few occasions indeed is it the preferable operation. In one of the cases recorded by me, a weakly child was born, and lived some years, but succumbed to teething. I am not sure but in such, and in many other cases of death from children's disease, we are right in following the views brought before the profession lately by Virchow—that congenital weakness has much more to do than we usually think with such fatal results. *Thus, a child* is said to die of hooping-cough, of measles, of teething, or of *bronchitis* ostensibly, but it really dies of congenital weakness, or of congenital-ly defective development. In the other instance, the infant survived its birth only a few hours.

But now, to turn very briefly to the comparative advantage of turning by the foot as an alternative to premature labour or forceps. This was argued to be, in reference to long forceps, a preferential operation by the late Sir James Y. Simpson, with his acknowledged ability and acuteness. Schroeder has also lately expressed himself as strongly in favour of the operation in the interest of the mother, especially, in contracted pelvis, even down to 2 inches 8 lines of conjugate. Borinski, in the paper above cited, has discussed this question at great length, and with much ability. He points out with great precision the danger likely to arise from attempts at delivery by turning in generally contracted small pelves, and restricts it to the simple flat contracted pelves, and considers turning is more particularly required when the head presents at the deformed brim with lateral obliquity of the neck, so as to make the posterior or anterior parietal bone present with the sagittal suture running transversely, either forwards near the symphysis, or far backwards near the promontory of the sacrum.

My two cases illustrate the truth of Borinski's view. Case 2 being one of generally contracted (flat ?) pelvis, was of the worst shape to be benefited by turning. The fatal results of turning to the child, and the successful results of forceps, point, I think, to what ought generally to be our practice on these occasions. There must be in the recollection of every obstetrician of considerable experience a good few cases in which he had prepared his craniotomy or cephalotripsy instruments, and found, on applying forceps, that after a period of careful but vigorous traction, his efforts have been rewarded by a living child and a healthy mother. On the last occasion when Mrs. N. was confined, I carried my cephalotribe to her house, in case forceps should fail. I well remember also another case in which I attended along with my then pupil, the late lamented Mr. Thomas A. Peddie. It was that of a girl married at 16 and pregnant at full term before she was out 17. On this occasion there was no deformity, but the pelvis was altogether too small, and the child was large. I waited a whole night and a day, but though the os was fully dilated, the head would not engage in the brim. I took with me also on this occasion my cephalotribe, &c., but determined to give the child a chance by forceps. To my astonishment, after a great deal of tugging, a living child was born. It did well, and so did its mother. Such cases are cropping up every day.

The cases in which turning is found to succeed best are such as my first case, in which we found on the whole that it did very well. But the great majority of such cases are made up of second and subsequent cases, which proved in the primiparous condition excessively difficult, and led to craniotomy and other serious results, as it is seldom in a first case that a difficulty suitable to be overcome by turning is proved to be so in time to allow of this operation being performed. These cases, however, are found to terminate just as easily by the application of forceps, if not without any interference at all. It is quite clear, on reading Schroeder's note after his paragraph on turning in deformed pelvis, that his views are arrived at chiefly from cases of great difficulty in first labours proving easy in subsequent labours, when turning was employed. But I hold that this reasoning is purely the *post hoc* fallacy. In the earlier part of my practice I turned much more frequently than now in such cases, and on the whole got fairly good results. Now I interfere much less readily, and find the results far better. I am not, however, inclined to hold that turning is such a purely harmless operation for the mother as Schroeder and others maintain. I can look back to two occasions on which I regret, for the mothers' sakes, that I did not employ forceps instead. I must also protest *in toto* against the views of Borinski, which are also to a certain extent shared in even by Schroeder and others, respecting the danger incurred by mother and child in using forceps. Such serious results may be true in employing those German and French instruments that are too long and approach too closely at the points, but are certainly not to be dreaded from a careful use of the English instrument, which is light and little bulky, and whose blades at their distal extremities do not approximate within an inch when out of use, and are much wider apart when applied.

Another misconception seems to have possessed men's minds in reference to the effect of the application of the forceps in increasing the tension of the already too small conjugate. Obstetricians persist in ascribing to the forceps the power of being a compressor. Now, every one possessing a knowledge of mechanics, sees at a glance that the amount of pressure which is needed to prevent the forceps from slipping is very little indeed, and seldom, if ever, such as to interfere so injuriously with the progress of the head as Simpson and Schroeder will have them to do. If evidence on this score were needed, a single glance at such an instrument as Professor Inglis's long forceps, which I know to work well, ought to convince the most sceptical. The forceps is essentially a tractor, and nothing else, when properly applied and used, being able to exert the necessary traction with the slightest amount of compression, and mainly by friction of the blades on the child's head.

But without further discussing this question at present, I may conclude by stating that, so far as my own views of practice lead me, they are as follows:—

1. That, looking at the statistics of Spiegelberg and Litzmann, it seems exceedingly doubtful if the operation of inducing premature labour ought ever to be employed in cases of contracted pelvis.

2. That turning does not present any proved advantage to the mother over long forceps in cases of contracted flat pelvis (so far at least as my experience and my reading enable me to form an opinion), and is undoubtedly more dangerous to the child. That it is entirely unsuitable when the contraction is general, being much more dangerous to the mother than long forceps, or any of the higher operations.

3. That in contracted pelvis, as a general rule, it is on the whole safer to let the case go on to the full term of utero-gestation, and then give the patient a fair trial, so as to ascertain what nature is likely to accomplish unaided, without waiting so long as to allow the mother to run unnecessary risk. Then, in case there is room for the introduction of forceps, they ought to be applied, and delivery attempted by their means. If this is impossible, then delivery ought to be effected by cephalotripsy, craniotomy, or Cæsarean section.—*Edinburgh Medical Journal, Sept.* 1873, *p.* 214.

88.—ON THE USE OF THE ELECTRO-MAGNETIC CURRENT IN THE SECOND STAGE OF LABOUR.

By Dr. A. E. M'Rae, Penicuik.

[The cases which form the substance of the following paper occurred in 1868 and 1869. The words electro-magnetic and electro-galvanic are used as practically well-nigh synonymous.]

It is well known that galvanism is a powerful agent in exciting muscular contraction. The uterus being a muscular body is susceptible of its influence. Some high authorities in gynæcological science, reasoning more from analogy than from actual experiments, have given their opinion against it. Later and more extended observations, however, have shown that galvanism, under certain conditions, has a powerful influence over uterine action. The following cases will, I think, in some measure support the theory:—

Case 1.—Mrs. S., aged 28, a well-made woman of about 5 feet 6 inches in height, was in labour with her second child; she had been in hourly expectation of the birth for the previous twelve hours. I was called about eight o'clock in the morning. I found the os well dilated, and quite flaccid, as were also the vaginal walls and perineum. The pelvis was well formed, and there existed no mechanical impediment to the progress of the labour. The bowels had been well opened on the previous day. The tongue was slightly whitish. The pulse was soft, and about 56. She had had no food for twenty-four hours. The summer was a very hot one, and she lived in a confined attic room. She was therefore in a very exhausted condition, and her muscular condition was far under par. I encouraged her to make a breakfast of bread, eggs, and tea.

On calling about an hour after, her condition was unchanged, and, thinking that she only now required some oxytoxic agent to rouse uterine action, I administered a drachm of tinctura ergotæ. This was several times repeated during the forenoon without any definite result. I was unwilling to leave, as the condition of the parts led me to expect a very speedy termination to the labour should even slight uterine action come on. Beef-tea, wine, spirits, and strong infusion of tea were administered with evident relish, but also to no purpose. Digital irritation, even pinching the os, and external friction gave no better result. Fearing post-partum hemorrhage with such a condition of things, I was unwilling to use the long forceps. Every ordinary means of rousing uterine action I knew of I tried, but each and all failed to fulfil my expectations, and as other engagements were pressing, I resolved to try galvanism. Accordingly, I sent for a galvanic machine, the appearance of which produced not a little amusement to the onlookers, although the patient eyed it with great apprehension. The first application was continued for two minutes. The uterus slowly responded, and after some seconds a moderate pain, the first for eleven hours, was induced, and continued to increase until the current was interrupted. The head descended to the brim. I now waited about ten minutes, and again applied the current for three minutes, which had the effect of bringing the head into the brim. After an intermission of five minutes, at the urgent request of the patient, who, to my astonishment, was pleased with the sensation produced, I again applied it, and in two minutes the head was rapidly born. By manual traction the birth of a healthy male child was completed in exactly twenty-three minutes from the commencement of the application. I now directed the patient to keep her hands firmly pressed against the uterine tumour, and in ten minutes more I found the placenta in the vagina. Convalescence went on uninterruptedly.

The success of this case, and the evident pleasure with which its application was enjoyed by the patient, made me resolve to add galvanism to my list of agents for expediting protracted labours, especially for those specifically known as cases of *inertia uteri*. And as I experienced some difficulty in getting the female attendant to work the machine steadily, I procured one of Maw and Son's self-acting electro-magnetic machines, with its complement of appliances or electrodes.

In the spring of the following year, 1869, other two cases occurred, where similar conditions presented themselves.

Case 2.—Mrs. M., aged 35, a tall, well-made woman, the wife of a farm labourer, had parturient pains for about thirty hours. I saw her first on the afternoon of the second day. This was her ninth child; all the previous births were natural, the two last only being somewhat protracted, but were accomplished without instrumental aid. I found her very exhausted, and the pains had left her some hours. The os was well dilated, and the parts were quite flaccid. The head could just be felt a good way above the brim. For several hours I tried the usual means of rousing uterine action, but failed to elicit any response. The patient was very lethargic and disposed to sleep. But as the case seemed so simple, I was unwilling to let it become further protracted, and sent for my battery. Meanwhile I freely administered Liebig's extract of meat, spirits, ergot, and friction, but failed to rouse action. The first application of the current lasted ninety seconds; powerful uterine action was induced almost immediately, and the head came through the brim. The current was now interrupted, in imitation of natural pains, and uterine action ceased, but commenced again spontaneously at the end of two minutes. This spontaneous action, however, was very feeble, and at the request of the patient the current was again applied. The action now became so very powerful that the head was born in a very few seconds, and the birth of a female child was completed without further aid, the placenta following in about the usual time. In this case the after-pains, probably induced by the large amount of ergot administered, were so severe that they had to be allayed with opiates. She made a good recovery.

Case 3.—Mrs. M'D., 32, about 5 feet 8 inches in height, slender but well-made, was in labour with her seventh child. The labour had commenced about midnight, and I saw her early next morning. Uterine action was weak, and occurring only at irregular and distant intervals. The os was about the size of a shilling, but dilatable. There was no great mechanical obstruction, though the coccyx was unyielding and more than usually turned in, but the pelvis was roomy. I had attended her in three previous confinements, which were natural, and required only digital assistance. I advised a good breakfast, which was somewhat reluctantly taken. In the course of the afternoon I returned, and found that there had been no action and no alteration in the condition of parts since the morning visit. I administered a large dose of ergot, and left, with orders to give another in the course of half an hour. On calling at the end of the hour, I found no change. Beef-tea, spirits, strong infusion of tea and coffee, porter, ergot, walking, digital irritation, and external friction were employed from time to time, and several times during the day she slept for half an hour. Evening arrived, but still no action could be elicited, nor did the os further dilate. I now sent for the battery. The first application lasted for one minute. Slight pains were induced, but the os was inclined to contract rather than dilate. I interrupted the current for five minutes, and kept up digital dilatation of the os; meanwhile, during the next several applications, I kept my fingers in the os, so as to maintain dilatation and allow the head the more easily and with less obstruction tó descend. The patient at first was very apprehensive of danger, and, thinking that there was no hope of her recovery, took a very affecting farewell of her husband, whose distress of mind during the operation was very audible from the next room. The patient now requested the application of the current more frequently than I considered advisable, and actually sent a message to her distracted husband "that she was enjoying it very much, and felt it doing her good." The descent of the head, however, was slower than I had experienced in the two previous cases. The tip of the coccyx offered some slight obstruction, but I persevered in the application, and in 35 minutes the head was born, and the birth of a large male child completed by manual traction. The placenta had to be removed, and there was a slight tendency to hemorrhage, which ceased, however, on the administration of spirits, and the application of the binder. She made a slow recovery, which I accounted for by the exhausted state of her system, her family having followed each other at about 14 months' interval. This woman always recovered fast after her confinements, thus showing that she had good recuperative powers.

These cases, though few in number, are sufficient to convince me that for some cases of labour we have in galvanism a potent power for good. That the results I obtained were due to galvanism I have not the least doubt. That the force evolved by the uterus was galvanic I doubt, but that galvanism roused and kept up a power inherent in the uterus, I think cannot be denied by the most sceptical. Ramsbotham, of London, in 1834, used galvanism, and was favourably impressed with its use in obstetrics and gynæcology. Five years later, Killian invented a galvanic forceps, and after six years' use gave it up, the results having fallen short of his expectations. In 1844, Hoeninger and Jacobi used it for the induction of premature labour. To Radford, of Manchester, however, belongs the merit of first making a practical acquaintance with its use in obstetrics, and he became favourably impressed with its usefulness. The late Sir J. Y. Simpson tried it in eight cases, but considered it useless as an oxytoxic agent, and of no utility in parturition. The method of application used by Radford and Sir J. Y. Simpson seems to have been suggested by Killian's galvanic forceps, viz., that of direct application of the electrode to the os and vaginal walls. Sir James held the opinion that if the galvanic machine had any oxytoxic influence at all, it was not by the galvanism *per se*, but by the irritating influence of the metallic instrument used—the catalytic action, if I might so use the term. He held the opinion advanced by Jordan that the uterus, like the dartos in the male, being of similar structure, is incapable of being excited to contraction by the agency of galvanism. That the presence of the instrument in the vagina had some influence on the action of the uterus I do not venture here to deny, but that galvanism has an influence on the action of the uterus in intensifying its *expulsive power*, either directly or indirectly, I think the cases recorded above *go far to prove*; and the more recent experiments of Oser and Schlesinger stamp the assertion with authority, for they have demonstrated that electrical excitation of the central end of a spinal nerve calls forth, in from five to fifteen seconds, energetic contractions of the uterus.

The method of application I have adopted still further proves that contractions of the uterus occur by the use of galvanism without introducing any instrument into the vagina. I might ask, if the contractions are caused by the presence in the vagina of the metallic electrode only, why make use of galvanism at all? Why not treat every case of *inertia uteri* by inserting into the vagina a piece of metal? If the catalytic theory be correct, then this treatment should be legitimate and successful. I am not aware that the theory has had any practical outcome.

My method of application is as follows:—One of the electrodes, a flat piece of metal the size of two hands opened, and joined in the line of their long axis, and curved to fit the transverse abdominal curve, is applied to the abdominal parietes and retained by the patient's hands, a layer of blanket being between the metal and the fingers to prevent the current entering by the fingers. The other pole is placed against the perineum, and retained by folds of blanket or a small pillow. The force can be moderated or increased at will, by the left hand of the accoucheur working the "regulator," while the right hand manipulates the child's head. The electrodes are now attached to the machine, and the circle being thus completed, the current is set on, and in obedience to a law in electro-physics, takes the shortest and most direct route from the abdominal to the perineal electrode, or *vice versa*. The uterus lying in this direct line shares in the influence; and in obedience to another law, its fibres contract, and in so doing aid in expelling the uterine contents, its action being limited by the electric force evolved, the quantity and condition of the muscular fibre of the uterus, and the calibre and patency of the passages. In this way the contracting or expelling force acts in the direction of the line of axis of the uterus. The induced contraction of the abdominal muscles must also exercise a very considerable influence over the expulsive process, this contraction lessening the size of the abdominal cavity and forcing outwards its contents. When the head descends and comes towards the perineum I shift the perineal electrode on to the coccygeal region, thus exercising a *vis a tergo*, and relieving the perineal muscles of their tonic contractions, which now become flaccid and permit dilatation. The head is thus born in the proper line of the axis of the outlet, with freedom. Possibly the action might be as powerful if the electrodes were placed anywhere on the abdominal parietes; but I prefer to place the one in front and the other over the coccyx, so that the action may be directed as nearly as possible in the long axis of the uterus and the outlet. If one of the poles of the battery were applied to the abdominal parietes, and the other to the os uteri, the whole uterus would be thrown into tonic contractions, the os would consequently be narrowed, and labour obstructed. In the cases recorded above, however, the current was not of sufficient strength to penetrate the whole body of the uterus, except in Case 3, where the os contracted slightly.

The electrodes I use for obstetrical purposes are two in number—the abdominal and the perineal. The abdominal is formed of two separate blades, each 6 inches long, 2 inches broad, and about ⅛th inch in thickness, slightly curved, transversely and longitudinally, to fit the curve of the enlarged abdomen, and joined together by a hinge, which permits of their being separated for facility of carrying. The perineal is a solid or hollow ovoid, 4 inches in longitudinal diameter, 1¾ inches transverse, and ½ inch in thickness. Instead of being a hollow ovoid, it may, for lightness' sake, be of a scaphoidal form.

If the waters have escaped too soon, the parts should be well lubricated, to facilitate the descent of the head. In cases of *inertia uteri* the child will usually be born in a very few minutes after the application has commenced. But should a longer time be required, then the application must be intermitted, so as far as possible to imitate the natural pains. Were it otherwise, the same consequences might occur as sometimes happens when ergot has been indiscreetly administered, viz., too violent and continuous action, with the risk of rupture or stoppage of the *platental* circulation and the death of the child.

Regarding the use of this agent, galvanism should be used when the os is dilated or dilatable; when the uterine action has ceased, and cannot be roused by stimulants, friction, or other oxytoxic agents; when there is evidence of uterine action becoming weak, irregular, or intermittent—time will thus be saved; when there is post-partum flaccidity of the uterus, or post-partum hemorrhage, not caused by hour-glass contractions. In the latter case, I should first administer a 15 or 20 gr. dose of chloral (an excellent co-ordinator (?) of nerve force), wait for some minutes, then apply the current. In short, it is necessary and useful when there is simple, slight, additional expulsive force required, that is, in cases of *inertia uteri*, with a well-formed pelvis.

It might also be used for the expulsion of clots or air, conditions very troublesome, and often giving rise to grave symptoms in stout women during post-partum convalescence. It might be used with advantage in cases of uterine hemorrhage caused by the pressure of fibrous or other tumours, or in their expulsion. Were an instrument made to encircle the tumour, and the current applied sufficiently long to stop the circulation, coagulation might take place in the vessels, and the tumour might die for want of nourishment. Ergot could not be given in doses sufficiently large to accomplish this without grave risk, but galvanism can be localized. It is also free from the risks attendant on the knife. Of course, these later remarks are entirely speculative, and my sanguine hopes of its practical application in these cases may be discouraged by the opinions of men of more extended experience; but my success with this agent in these cases of *inertia uteri* has emboldened me thus far to trace its probable application.

I would recommend galvanism, therefore, in cases of sluggish uterine action, for its use would save the time of the accoucheur, and shorten the labour of the mother. Its use in the latter case cannot be over-estimated, when we remember the frequent baneful effects of protracted labours. Its use at the time, and by the method I have indicated, is not attended by any danger to the mother or the child. It is pleasant to the mother, and hope is revived within her when she feels that its application is doing good. They all expressed themselves agreeably surprised.—*Edin. Medical Journal, Sept.* 1873, *p.* 243.

89.—ON THE TREATMENT OF PUERPERAL CONVULSIONS.

By Dr. Robert Barnes.

[The following is an abstract of that part of Dr. Barnes's Lumleian lectures which relates to the treatment of convulsive diseases.]

Upon the treatment of convulsive diseases I have nothing new to offer in the way of remedies, or even of methods of applying them. The utmost I pretend to do is, to strengthen the rational basis, the principles, upon which the treatment should be conducted; and to show why certain agents, still of conventional reputation, should be rejected.

The principles of treatment flow logically from the view we have taken of the etiology and nature of the diseases we have to deal with. Four cardinal principles may be laid down for our guidance:—

1. To moderate central nervous irritability.
2. To cut off emotional irritants or excitants.
3. To cut off peripheral irritants or excitants.
4. To eliminate all complicating morbid conditions.

To carry out all, or as many of these indications as we can, should be our aim in the management of all convulsive diseases. But the varying circumstances of different diseases and of different cases will prompt us to vary the order in which these indications shall be taken. For example, in all the convulsions of pregnancy the question surges up, Shall we try to subdue the *central* nervous irritability by putting an end to the pregnancy which produced *it?* The affirmative presents itself with cogent force. Yet it is not always possible or wise to resort to this measure, at least in the first instance. The indication is most clear when eclampsia breaks out after the sixth month of gestation. Here we have not only intense exaltation of nervous irritability, but an active blood-poison constantly keeping up this irritability, and which we know from experience will rarely disappear until the pregnancy which produced it is brought to an end. Often, indeed, the disease itself will determine the labour. In this case we have but to follow the lead, and accelerate it judiciously. The urgency is pressing; every fit is a source of fresh danger to the brain. Prompt action, by averting one fit, may save life. Still we may err by precipitation. If we hurry on the labour too quickly, if, in short, we carry out what the French call the *accouchement forcé*, we may do irreparable mischief. The violence of the necessary manœuvres will be a source of so much irritation as to add to the severity of the fits, and to add the depressing influence of shock. There cannot, I am convinced by observation even more than by theory, be a more fatal error than to follow the precept still inculcated and practised by many, to deliver as quickly as possible. The simplest measure which involves the least possible irritation is the best. That is, to puncture the membranes and leave the rest to Nature, at least until we see she fails to carry on the process. And since even the gentlest examination is often enough to provoke a fit, I would advise the previous induction of anæsthesia by chloroform. Under the cover of this state, the catheter should first be passed to secure an empty bladder, and to procure a good specimen of urine for testing. Then, at the same sitting, the membranes should be punctured by a quill, stilet, or other suitable instrument. The diminution of the volume of the uterus by the draining off of the liquor amnii, lessening the pressure upon the vessels and the vascular tension, gives sensible relief. But another good effect generally follows: one it is true not without occasional drawbacks, but still a risk that must be encountered. The good effect is this; the moment labour is started a call is made upon the nervous centres for nerve-force to be expended upon the uterus. It is, indeed, a matter of observation that uterine action will often excite a convulsion. But, upon the whole, I am disposed to think that it acts beneficially; and we shall be the less afraid of calling it into operation if we reflect—first, that labour must take place, and that it cannot be effected without this uterine action; and, secondly, that we can greatly diminish the excess of irritability by the use of chloroform.

The expediency of inducing labour when there is albuminuria without convulsion is more doubtful. As we have seen, it is not certain that convulsions will break out.

The induction of labour is the means of carrying off, or of discharging, the excess of nervous tension. But something more is commonly required. We cannot complete labour all at once. Sometimes we must wait, and, not seldom, when the nerve-force has once got into a wrong channel we shall fail to turn it all to the proper direction. Measures for moderating the excess of central irritability are almost always useful. The most available of these is the induction of anæsthesia by chloroform. It should be carried to the surgical degree —that is, to the extent of rendering the spinal cord irresponsive to irritation of the sphincters. The beneficial effect of this is sometimes very striking. The convulsive fit is rendered less violent; it is shortened. When anæsthesia is induced in anticipation of a fit, this may be almost entirely averted, and is sure to be moderated. The evidence in favour of chloroform has been greatly accumulating since Simpson's time. Chloroform blots out memory, one source of emotion; it cuts off perception, another source of emotion; it lessens reflex irritability. When chloroform or equivalent anæsthetics cannot be given, we must act on the same indications. We must procure absolute rest; exclude noise, light; avoid all suggestion of disagreeable ideas; avoid all irritation of the skin. All this is especially necessary at the acme of spinal and cerebral irritability when a fit is on or impending. But an essential condition for obtaining from chloroform the full benefit it is capable of giving, is to keep close watch over the patient, so as to be ready to administer it on the slightest warning of a fit.

How it is that chloroform acts in averting or shortening a fit may perhaps be explained by the following observation of Achille Foville. The cessation of an attack is the consequence of the asphyxia which itself produced. The quicker the asphyxia, the more quickly is its action felt upon the cord, rendering it incapable of reacting. Thus the danger is averted by its very excess. Chloroform, by inducing asphyxia, acts in a similar way.

But whatever the explanation, the use of chloroform now rests upon a solid foundation of clinical facts. When a fit overtakes a patient, the action of chloroform is too slow. It is therefore my intention on the next occasion to administer the nitrite of amyl.

In fulfilment of the same indication we may derive great assistance from opium, belladonna, and bromide of potassium.

When the patient can swallow, in the intervals of the fits, the bromide of potassium or of ammonium may be given in scruple or half-drachm doses every three or four hours. But in the first instance it is more useful to give a scruple or half-drachm dose of chloral. This, like chloroform, removes all emotional sources of irritation, and lessens the sensitiveness of the nervous centres to peripheral irritation. The sleep it procures is eminently beneficial. I entirely concur in the praises which many practitioners have bestowed upon this most precious remedy in eclampsia.

We cannot yet discuss the treatment of eclampsia without referring to the practice of bleeding. To advocate anæsthesia is practically to condemn venesection. Not that there is any necessary antagonism, but that, as a matter of fact or of fashion, chloroform is superseding venesection. I am one of those who think there is more of fashion than of wisdom in the almost absolute oblivion of the lancet. But in this particular case I do not regret the disuse into which it is falling. It is very easy to tell of cases in which bleeding has been followed by recovery, and of other cases in which other treatment has been followed by death. I believe I have seen distinct relief ensue upon moderate abstraction of blood from the arm or by the application of leeches to the temples. And where there is distinct evidence of plethora with marked engorgement of the vessels of the face, it is judicious, I think, to apply eight or twelve leeches to the temples, but not to the exclusion of anæsthesia. In delicate women with a feeble circulation bleeding in any form should be rigorously condemned. And we must not forget that the process of labour is usually attended by a loss of blood quite as great as is good for the patient.

In climacteric epilepsy the abstraction of a moderate quantity of blood by leeches to the temples, or by cupping at the back of the neck, just before the expected return of the attack, is often eminently useful.

The second indication, to cut off emotional excitants, is one that is generally studied.

The third indication, that to cut off physical peripheral excitants, demands more attention. The first impulse of many when they see a person in a fit is to dash cold water in the face. In eclampsia it is decidedly injurious. I have seen it provoke a fit. Another not uncommon error in eclampsia is to apply blisters to the nucha, or mustard poultices to the calves. These applications do exactly what ought not to be done. The irritation they produce when the whole surface is in a state of hyperesthesia, is doubly prejudicial. There is no fact in medicine of which a stronger conviction has been forced upon me by observation than this, that all peripheral irritation is injurious in eclampsia. It is a sin against physiology. Hence the rule, when the situation dictates manipulation of any kind, to lull the system in the artificial sleep of anæsthesia before passing the catheter, before making a uterine examination, or proceeding to induce labour.

These principles of action apply with almost equal cogency to the treatment of epilepsy in the pregnant state; they apply with quite equal cogency to the treatment of obstinate vomiting in pregnancy. When vomiting, in severe and uncontrollable degree, sets in about the third month, the question as to inducing labour presents itself with peculiar anxiety. On the one hand; if it be resorted to at once, there may be room for the misgiving that a grave step has been taken unnecessarily. On the other hand, the fatal progress is apt to steal on insidiously but rapidly, so that the time for hopeful action may quickly pass away. If the pulse have risen to 120 or 130, if it be small, indicating prostration; if there be marked Hippocratic countenance, considerable emaciation, continuous difficulty in keeping down food, sleeplessness, and especially any degree of delirium, it is highly probable that the induction of abortion will be too late. It may even provoke distress which will accelerate the fatal issue. The same observations apply to obstinate vomiting in the latter months. The indication to interpose early is all the stronger if there be albuminuria; for the concomitant blood-disease will almost infallibly keep up the irritability of the nervous centres and the vomiting. But in several fatal cases which I have seen there was no albumen in the urine, so that, whatever poison there were in the blood, the condition was probably different from that which we call uremia. Comparing these cases with the phenomena of acute atrophy of the liver, and with other cases of rapid sinking in pregnancy, I cannot help suspecting that there is developed some graver systemic or organic disorder than has been yet recognised. In some cases I have no doubt the delirium witnessed towards the close, the irritative fever, are the result mainly of starvation.

It is wiser then to err on the side of safety to the mother, and rather to induce labour too soon than to temporize until it is too late. Unless we can, within a short time, get wholesome nutriment into the system, the system will feed upon itself; and the nervous centres, partaking of the general exhaustion, may soon be paralysed.

To counteract this progressive starvation is one of the greatest practical difficulties. Enemata of beef-tea containing brandy or port wine, and sometimes half a drachm of chloral, are often of the greatest benefit. But in cases of extreme anxiety there is another remedy which has not yet been sufficiently, if at all tried, and which is full of promise. I mean the transfusion of blood. One successful case has been recorded of transfusion in exhaustion from puerperal convulsions.

In the case of exhaustion from obstinate vomiting, if used betimes, the prospect is better still, unless, indeed, the patient be struck with that deadly disease at the probable existence of which I have hinted.

In the chronic state of the menstrual or climacteric epilepsy, bromide of potassium, belladonna, and various metallic preparations, and a carefully ordered hygiene, are our chief allies.

I have not yet fairly tried Voisin's plan of giving the bromide. But it appears to me eminently deserving of being rigorously tested. It consists, as described in his admirable article on Epilepsy in the Nouveau Dictionnaire de Médecine, in giving from two to twelve grammes daily a few minutes before meals, and in persisting unintermittingly for a year or more, testing the action of the bromide by applying a spoon to the epiglottis. When reflex nausea is no longer excited by this test, we know that the drug is acting on the rachidian bulb diminishing its excito-motor force. As Voisin truly says, "in a chronic disease we must have a chronic medication. Bromide of potassium must be an aliment." Diuretics must be given with it; and occasionally iron to obviate anemia.

Trousseau's plan of giving belladonna continuously for one, two, or more years, is another way of carrying out the same indication. This, he says, is more especially useful in the epileptic vertigo. I am inclined to suspect that the disappointment at times experienced with these remedies is in some measure due to the want of perseverance in keeping up the full doses over a long space of time, and in many cases—my remark applies especially to women—to the neglect to treat the frequently attending ovario-uterine complications.

The value of the metals in the treatment of convulsive diseases, has always been recognised. It would be impertinence on my part to dwell upon the subject. I would simply ask leave to mention that many years ago I proposed, and extensively tried, the combination of zinc with phosphoric acid. The state of the urine should be observed; and, in any case, the secretions should be carefully regulated by alkaline salines, aloetic and mercurial pills, or Püllna or Friedrichshall waters. To meet the fit itself, chloroform should be kept at hand, and a bit of india-rubber to slip between the teeth.

The indication to cut off all complicating morbid conditions is one that rarely admits of being fulfilled in the urgent cases of eclampsia and of the vomiting of pregnancy. Purgatives, and those not the gentlest, are commonly given in eclampsia, the motive being to remove any possible irritating matters from the intestinal canal. It is a routine practice, which is occasionally useful, as in those cases where a fit has come on soon after a heavy meal. An emetic would act better still. But in the majority of cases I do not think I have seen any good from the practice. Some violence is often done in forcing open the jaws to place calomel or croton oil on the tongue, and this is bad.

The elimination of all complicating morbid conditions is especially necessary in the prevention and treatment of the convulsive and other nervous diseases of non-pregnant women. In a large proportion of cases, indeed, this constitutes the greater part of the treatment. This applies particularly to neuralgia, to hysteria, and, in a lesser degree, to the epilepsy of the climacteric period.

The particular form of dysmenorrhea which is most commonly the attendant and forerunner of nervous disorder is that which depends upon obstruction or partial retention of the menstrual fluid. I may state that the following sequence of facts is established by an overwhelming mass of clinical observations.

1. Beginning with the menstrual function, there is the pain which is one of the features of dysmenorrhea.

2. There is gradual wear and tear of the nervous system, attended by degradation of the blood.

3. Increased susceptibility to physical and mental impressions.

4. Where marriage ensues, it is generally unfruitful; proving again, by another test, that there exists an impediment to the due performance of the ovario-uterine functions.

5. Where the obstruction is removed, as in most cases it can be, we find the preceding conditions gradually disappear.

If, on the other hand, the dysmenorrhea be cured early—that is, before hysteria, neuralgia, or epilepsy have shown themselves,—these disorders will, in high probability, not appear at all.

In single women dysmenorrhea is the most frequent attendant or exciting cause of hysteria or epilepsy. In a certain proportion of cases, however, no abnormality of structure or function of the ovaries or uterus is apparent. The exciting cause may spring up elsewhere; but in all there is a special proclivity developed by the normal ovario-uterine stimulus.

In married women, and in those who have borne children, not only may dysmenorrhea arise, but metritis, congestion, displacement, and other affections are more frequent. They hardly ever fail to induce that general debility and nervous prostration which predispose to nervous disorders. It is generally a hopeless task to cure these nervous disorders, unless we begin by relieving the local disorders upon which they so greatly depend.

Before closing, I must beg leave to retrace my steps rapidly, and to sum up in a few propositions the principal points of my theme.

1. Pregnancy and labour require for their due fulfilment an extraordinary supply of nerve-force.

2. This extraordinary supply of nerve-force implies a corresponding organic development of the spinal cord.

3. The provision of an extraordinary supply of nerve-force implies a greatly augmented irritability of the nervous centres, rendering them more susceptible to emotional and peripheral impressions.

4. The disturbances in nutrition occasioned by pregnancy almost always entail some alteration of the blood, which increases the irritability of the nervous centres, and favours the evocation of any latent convulsive or rather nervous diathesis, as chorea, epilepsy, or vomiting.

5. When the blood-change wrought by pregnancy is marked by albuminuria, a poisonous action of peculiar intensity is exerted upon the nervous centres tending to produce eclampsia.

6. Obstinate vomiting in pregnancy probably sometimes proves fatal by the development of an unknown organic or systemic morbid process.

7. Menstruation resembles pregnancy in giving rise to an exalted central nervous erethism, and ovulation is a primary exciting cause of epileptical, vomitive, and hysterical convulsion.

8. At the climacteric age, again, there is renewed susceptibility to convulsive disease.

9. Pregnancy, by evoking or producing convulsive diseases under certain

known and passing conditions, puts to the test the various theories of the pathogeny of these diseases.

10. The rational treatment of convulsive diseases in women must take into account the two great factors in the production of these diseases—namely, exalted nervous irritability under the stimulus of the reproductive function, and lowered or empoisoned conditions of the blood.

If it be objected that the views I have ventured to lay before you seem to be false in colour and form, wanting in breadth and perspective, I might reply that to deny this absolutely would be to arrogate to oneself freedom from human infirmity. The mind, like a mirror, can only reflect the impressions it receives. Our care must be to keep the mirror bright and even, so that it may reflect truly. This I have endeavoured to do. With what success must be determined by comparing my reflections with those coming from mirrors that are brighter. One thing only do I ask. It is that these mirrors may be so set as to take in the objects which have been reflected from mine.—*Obstetrical Journal*, *July* 1873, *p.* 267.

90.—ENEMATA OF CHLORAL HYDRATE IN PUERPERAL CONVULSIONS.

By Dr. JOHN BARCLAY, Leicester.

The notice in a contemporary of a somewhat similar case, induces me to mention an instance of the successful administration of chloral in a most severe case that I recently saw in conjunction with my friend Dr. Hulme, of Wigston Magna.

Mrs. T., aged 22, primipara, who had had slight anasarca before her confinement, was delivered at 5 p.m. on February 9th. The labour was satisfactory, the placenta following naturally. Shortly afterwards convulsions set in, and continued almost without intermission. I saw her first at 2 p.m. on February 10th, the convulsions having lasted twenty hours. She was totally insensible, cold, and clammy; the pupils were largely dilated; pulse 180. She was unable to swallow; the fæces and urine passed unconsciously; she had had turpentine enemata with no benefit. While I was there she had the most horrible set of convulsions I ever witnessed. In fact, I thought she was dead, respiration ceased for so long. Unable to give any remedy by the mouth, I suggested, as a forlorn hope, half a drachm of chloral in a small starch enema, to be repeated once. I saw her again at 6 p.m., and found her in a quiet natural slumber that I did not disturb. She had fallen into it, and had had no more convulsions after the second enema.

Passing through the village next day, the 11th, I found her only a little stupid; the pupils were natural; pulse 108; the milk and lochia appearing. She made a good recovery, and is now well and nursing her baby.—*British Medical Journal*, *May* 31, 1873, *p.* 611.

91.—CASE OF PUERPERAL CONVULSIONS AFTER DELIVERY.

By Dr. ALLAN D. MACKAY, of Stony Stratford.

[The patient, who had been attended by a midwife in the morning, had had three convulsions before being seen by Dr. Mackay, in the evening, which were described as "slight, bad, and worse" by the friends. There had been anasarca of the lower limbs, and various head symptoms indicative of uræmic poisoning.]

I drew off more than a pint of urine with the catheter, and found it of specific gravity 1018, and containing much albumen. I prescribed perfect quietude, broth and milk for her diet, and a draught containing 25 grains of hydrate of chloral to be given as soon as she could swallow, in case another convulsion should come on; if none recurred, that she should be left alone, as far as medicine was concerned, till I saw her the next day.

On the following day I found her with a tongue loaded with a creamy fur, very swollen, and showing traces of having been bitten. Her pulse was 100. She had headache, but could "see a little." No urine had been passed, so I again drew it off with the catheter, and it was still albuminous. The legs still pitted a little. But now for the practical part of my paper, and it is solely for this that I bring the matter before you to-day. Soon after I left her on the previous evening, about 7 p.m., she had "a severe convulsion." There had not been then time for the medicine to reach her. At 11 p.m. she had "a still more severe convulsion;" when this ceased, she was able to swallow the draught (25 grains), and was soon afterwards asleep. She slept for four hours, then woke, saying she "could see a little," and slept again and again. At night the chloral was repeated, and she had an aperient dose to take during the day.

On the 10th the tongue was cleaning; pulse 80. The urine was passed last night and this morning. She could see, though still "a little misty." She had to-day no headache, and there had been no more convulsions.

On the 11th the milk had come, and the urine was passed well. I need not detain you with the progress of the case. She got better each day, and is now perfectly well, the albumen quickly passing away from the urine.

In chloral, then, it would appear we have a grand assistant in these cases. For some time I doubted with myself (hating to believe too soon that the *post hoc* is *propter hoc*) whether the cessation of the convulsions could be due to the chloral or not; but, considering that they were increasing in severity, it seemed that such might be the case.—*British Medical Journal, June* 21, 1873, *p.* 701.

92.—THE ANTICIPATION OF POST-PARTUM HEMORRHAGE.

By Dr. Ewing Whittle, Lecturer on Medical Jurisprudence in the Royal Infirmary School of Medicine, Liverpool.

[More than twenty years ago Dr. Whittle read a paper before the Medical Society of Liverpool, showing that the occurrence of post-partum hemorrhage can be diagnosed beforehand, and being diagnosed can be prevented.]

Very early in my career in the practice of obstetric medicine, I observed that cases of parturition in which post-partum hemorrhage occurred were always marked by the pains during labour being of a peculiar character. Having my attention directed to this point, I further observed that post-partum hemorrhage *never* occurred in any case of labour in which this peculiar character of the pains was absent. The pains are of this kind: they are strong and quick; they do not gradually culminate into a strong pain and subside again, but they are sharp, quick, and cease almost suddenly; and the intervals between the pains are long in proportion to the length of the pains. In an ordinary case, for one or two hours before the completion of labour, the intervals will average about three times the length of the pains; *i.e.*, if the pains last each from fifty to sixty seconds, the intervals will average a little less than three minutes. Now, if the pains last each only from forty to fifty seconds, and are of the sharp character I have described, with intervals lasting five or six minutes, though the labour may proceed steadily, and the head advance a little with every pain, you will be sure to have hemorrhage after delivery is completed, unless you anticipate it by altering the character of the pains, in making the pains longer and the intervals shorter. It is very easy to understand how this comes to be the case; the uterus is contracting sharply, and then becoming fully relaxed; after the child is born, a relaxation follows; one or two sharp pains expel the placenta with a gush of blood, and the uterus *again relaxes*, continuing the same tendency which existed before the delivery *of the child.*

Some of my friends will say that this may be very well, but that all this may be prevented by carefully attending to the maintenance of a steady contraction of the uterus by following down the expulsion of the child and placenta, and keeping the uterus firmly grasped until all danger of relaxation has passed away; that, by attending to this point carefully, during an experience of many years, they have never had a serious case of hemorrhage. But I would warn them that, no matter how long, through good fortune or good care, they may have escaped this painful trial, they never know when they may happen to fall in with a patient of weak, flabby fibre, and low nervous power, in whom they may find it impossible almost to induce active uterine contraction in time to save her from the effects of the hemorrhage.

Having recognised the necessity for interference in any case as soon as the os is fully dilated, I give a full dose of ergot. If this do not improve the character of the pains at the end of an hour, or thereabouts, I repeat it; I scarcely ever find a second dose necessary. In dealing with primiparæ, caution is required, first, not to administer ergot until the soft parts are pretty well dilated as well as the os uteri; and the drug should be administered, in much smaller doses, as it sometimes acts with unusual energy in primiparæ. Generally, in about twenty minutes or half an hour after the ergot has been administered, the pains increase in length and frequency, and when the labour is over, the uterus maintains a good contraction. The ergot which I use is a liquid extract twice the strength of that of the *Pharmacopœia*, of which I give a teaspoonful when I think a full dose is indicated.

I have pursued this practice now for more than twenty years. During this time I have attended 3750 labours, and among them I have had one case of *post partum* hemorrhage; that case occurred about three o'clock one winter's morning, when I happened to have no ergot with me. I foresaw that it would happen, and was able to prevent its being very serious by adopting the ordinary means.—*British Medical Journal*, *Sept.* 27, 1873, *p.* 370.

93.—ON THE INJECTION OF PERCHLORIDE OF IRON IN POST-PARTUM HEMORRHAGE.

By Dr. W. S. PLAYFAIR, Professor of Obstetric Medicine in King's College, and Examiner in Midwifery at the Royal College of Physicians, London.

[In a discussion which recently took place at the Obstetrical Society of London, upon the subject which forms the title of this paper, Dr. Playfair stated that he had in no case seen any injurious effects from the injection of a solution of perchloride of iron into the uterus, although admitting that an agent so potent should not be carelessly and indiscriminately used.]

By a somewhat curious coincidence a few days after the debate I had a case under my care in which I used it, and, as I firmly believe, saved by it the life of my patient. Yet very grave and even alarming symptoms followed—due, it can hardly be doubted, to its employment, and I think that the case is sufficiently instructive to be worthy of record. It shows one class of dangers which may arise from it, and possibly the history will teach us how, under similar circumstances, these are to be avoided.

Two and a half years ago I saw, with Mr. Aikin, of Clifton Place, Sussex Square, a lady who was apparently at the point of death from post-partum hemorrhage. She had been confined of her fifth child rather more than two hours before I saw her, after a somewhat tedious labour, the breech presenting. All her other labours had been natural. She was a stout woman, thirty years of age. After delivery the uterus had contracted firmly, with no more discharge than usual. Mr. Aikin had stayed with her more than an hour, and had left her seemingly well and comfortable. Half an hour afterwards she had a tremendous gush of hemorrhage. Mr. Aikin was immediately summoned, and speedily arrived, accompanied by Mr. Rushforth, of Oxford Terrace. The patient was then collapsed and insensible, and to all appearance dead. Some brandy was introduced into the mouth through an aperture formed by the absence of one or two teeth, and a solution of perchloride of iron, which Mr. Aikin had fortunately with him, was at once injected into the uterus, and all further loss was checked. When I saw her shortly afterwards she was still collapsed and pulseless, and I immediately sent for the necessary apparatus for transfusion, which seemed to afford the only hope of saving her life. Before the instruments arrived, however, she had slightly rallied, and eventually made a good recovery, though she long remained blanched and anemic. Such was the formidable history of the patient previous to her present confinement. On this occasion Mr. Aikin was unable to take charge of her, being confined to his home by illness, and I was asked to attend her in company with Mr. Rushforth. In no case is "forewarned, forearmed" a truer proverb than in relation to post-partum hemorrhage, and as we adopted every possible precaution to prevent it, we were in hopes that no repetition of the former flooding would occur. The head presented, and the labour was natural and easy. As the head descended a drachm of the liquid extract of ergot was administered. Firm pressure on the uterus was kept up as the child was expelled, and continued without intermission afterwards. A second dose of ergot was given shortly after delivery, immediately after the expulsion of the placenta. One or other of us kept kneading the uterus for three-quarters of an hour after the birth of the child. It contracted fairly, but not tightly, and it showed a tendency to relax. Two or three times small pieces of ice were introduced into the uterus to promote contraction. All this time there was no unusual loss, and we considered any danger of hemorrhage to be over. Suddenly, and while the uterus was still grasped by the hand, an appalling flow of blood occurred. I immediately emptied the vagina of a mass of clots, and, as all means of promoting contraction had been already vigorously employed, I at once proceeded to inject a solution of the perchloride of iron of the usual strength; and not a moment too soon, as the patient was already tossing about, sighing deeply, and showing the well-known formidable signs of collapse. As I injected I felt the uterus contracting round my hand, and not a drop more blood was lost. Nothing could be more rapid and satisfactory than the action of the remedy, and I honestly believe nothing else would have checked the flooding or enabled us to save the patient's life. For two days all went well. On the third day the pulse was 100, and the temperature 102°. The day following the pulse was 120, small and thready, the temperature 104° in the morning, and 105° in the evening, the tongue dry and black, and the general condition very alarming. There was no abdominal tenderness whatever. The uterus was somewhat large, reaching nearly to the level of the umbilicus. There was little or no discharge, and what there was was highly offensive. Eight ounces of brandy per diem were administered, and 30 minims of turpentine every sixth hour, and a teaspoonful of Brande's beef jelly every hour. On internal examination, the whole vagina was found to be filled with small, hard, black clots, formed by the corrugating effects of the iron, and believing that the symptoms were probably due to the retention in utero and decomposition of similar clots, giving rise to septic absorption, the cavity of the uterus was freely washed out with Condy's fluid and water, by which several portions of broken-down coagula were removed. Next day things were worse rather than better, the temperature being 105½°, pulse 130. There was some cough, with sibilant râles over the right chest. Still there was no local tenderness or other symptoms. We then had the advantage of meeting Sir William Jenner in consultation. The general treatment was continued, the quantity of brandy being increased. With the view of reducing the hyperpyrexia gr. v. of sulphate of quinine in pill were administered every third hour. The intra-uterine injections of Condy were continued three times a day, and in the evening a large and highly offensive clot was ejected. Next morning the temperature had sunk to 102½°, and the pulse to 100. Treatment as before. Quinine now given every fifth hour. In the evening the temperature had again risen to 104°. Another large coagulum was expelled after injection. Next morning the temperature had fallen to 101½, the pulse to 86, and all fetor had disappeared from the discharge. No more coagula were passed. It is needless to continue a record of the case, as the improvement from this date continued to be steady, and in a few days the patient was convalescent.

There can I think be little doubt as to the sequence of events which gave rise to these alarming symptoms. When the iron was injected, although the hand was in the uterus, and the clots within it had been as much as possible removed, blood was still pouring out abundantly. The powerful astringent at once corrugated all the blood and coagula it came in contact with, and these hardened clots filled up the uterus and the canal of the vagina. In due course these began to decompose, and septic absorption took place. By the finger and the intra-uterine injection they were gradually broken down and removed. The improvement unquestionably dated from the expulsion of the two large and decomposing coagula on the sixth and seventh days after delivery. Immediately after this happened, the temperature and pulse fell remarkably, and recovery commenced and continued uninterruptedly.

What then is the lesson to be learnt from this case? Is it that the risk is too great, and that the injection of the perchloride of iron should be banished from practice? I think most unquestionably not. I have little doubt, knowing what I did of the patient's former labour, and having already tried in vain all the anti-hemorrhagic treatment at our command, that without the perchloride the flooding would have proved fatal. It is indeed precisely in these inveterate cases, where every means of inducing uterine contraction proves unavailing, that it forms so invaluable a resource. Rather, I think, it should teach us to limit its use to these only—as, I believe, Dr. Barnes has all along taught. It shows also that the retention in utero of hardened coagula, liable to decomposition, may prove a source of danger hitherto unsuspected. With a knowledge of this fact it would be our duty to secure the expulsion of the coagula as soon as possible after all risk of hemorrhage had ceased, and make sure that there was a free exit for the discharge.

This would best be done by satisfying ourselves on the second or third day after delivery that the vagina is not filled with clots, and removing them if present, and by using antiseptic intra-uterine injections freely, as in the above case, should suspicious symptoms arise. With a knowledge of this source of danger, it might probably be avoided in most cases. Whether any other astringent fluid, such as the tincture of matico, the use of which was suggested at the Obstetrical Society, would answer equally well in constringing the vessels from which the blood flows, and be less apt to produce hardened coagula, is well worthy of consideration. I question very much, however, if anything less than the most powerful and direct astringent is to be depended on.

Important as are the lessons this case has taught me, it has left me not a whit less a believer, but rather a firmer one, in this most invaluable remedy.—*Obstetrical Journal*, *May* 1873, *p.* 89.

94.—A CASE OF POST-PARTUM HEMORRHAGE TREATED BY THE INJECTION OF THE PERCHLORIDE OF IRON.

By Dr. THOMAS CHAMBERS, Physician to the Chelsea Hospital for Women, &c.

[Nearly every case of post-partum hemorrhage can be treated successfully by external pressure and cold, but there are cases which quite resist such treatment, and it is for these that the perchloride of iron is said to be so successfully employed.]

Mrs P., aged 30, a thin, delicate-fibred, nervous woman; third pregnancy, full time; last pregnant five years ago. I saw this lady for the first time at 11 p.m., July 10. The pains were short, and recurring once in fifteen or twenty minutes. The os uteri being high up and not larger than a shilling, I prescribed twenty drops of opium in a little brandy-and-water, and returned home. At six o'clock on the following morning (July 11th) I was again sent for. From this time labour progressed quietly until 11.30, when a sudden and long-continued pain came on, which expelled the child. After the separation of the child, the right hand was placed upon the abdomen, when the uterus was found to be al*most as large as before the* expulsion of the child; it was very soft and flabby, having a doughy feel. When the hand was pressed upon it, it seemed to sink *into it* as if pressed into a soft body. As there was no external hemorrhage, and as the cord was soft and lax, it was clear the placenta was detached, and, in all probability, blocking up the os uteri, and that the enlarged uterus was filled with blood. When the placenta was withdrawn it was followed immediately by a large clot and most appalling hemorrhage. Free pressure was made outside by the right hand, and inside by the left, but notwithstanding every effort, the blood still flowed apace, streaming down my left arm, and running off the elbow on to the carpet. In a few minutes the patient became completely blanched, the pulse all but imperceptible, the respirations sighing and spasmodic, with general jactitation. Still the uterus remained soft and flabby, and there was every prospect of an early death. Here was a case admitting of no delay by way of consultation or otherwise. I sent to the nearest chemist for tinc. ferri perchloridi ℥ vj. Half of this I directed the nurse to mix with a pint of water, still keeping up both external and internal pressure. When all was ready, I passed the long tube up the palm of my left hand to the fundus uteri. In two minutes from the first injection I could feel the uterus gradually contracting around my hand, and with this contraction the bleeding ceased, the coagula passing out of the uterus with the hand as the uterus contracted. In five minutes from the first injection the uterus had freely contracted, and all hemorrhage had ceased; and, for the first time, there seemed a chance for the patient's life. Nothing could exceed the rapidity of action of this remedy. The satisfaction felt at such success under circumstances so urgent requires to be experienced in order to be duly appreciated. I honestly believe that no other remedy with which we are at present acquainted could have rescued this poor lady from immediate death. The fact cannot be too often or too deeply impressed on the professional mind that the treatment of cases such as this must be *prompt* and effective, or the patient is lost.

It may be remarked that this was a capital case for ergot. Just so. I gave a dose of ergot in a little brandy before removing the placenta and immediately afterwards. It must not be forgotten that the ergot must be absorbed into the circulation before it can induce uterine contraction. Now, in acute hemorrhage, such as has been described above, one of the first and most important symptoms is the almost total loss of pulse. It must follow as a matter of course that when the circulation is reduced to a minimum the absorbing powers must be reduced in a like degree; this being the case, it will follow that ergot can be but of little if of any value in such cases. And what took place in this case will doubtless take place in others under similar circumstances—viz., the ergot and brandy remained in the stomach for a time, and then were ejected by vomiting. Very free vomiting took place in this case which complicated matters considerably.

The immediate contraction which followed the injection was very remarkable. The right hand was kept on the uterus for half an hour after the hemorrhage ceased, as a matter of precaution. The uterus could be distinctly felt to contract into a hardened body, and again relax at frequent intervals, but no bleeding accompanied or followed these contractions.

The patient vomited violently immediately after the injection, nothing being ejected but the brandy and ergot, which had been freely taken at the early stage of the case. As far as one could judge, it was much in the same state as when taken into the stomach, indicating pretty clearly that little or no absorption had taken place. This lady made a rapid and satisfactory recovery without a single bad symptom. July 31st. She is to-day in her drawing-room, her only complaint being that she is much weaker this time than on former occasions. This is just the kind of case which Dr. Barnes has indicated as suitable for the perchloride of iron treatment—one which, in all human probability, would have been lost but for the timely aid afforded by this powerful remedy.—*Obstetrical Journal*, *Oct.* 1873, *p.* 446.

95.—THE INJECTION OF PERCHLORIDE OF IRON IN PUERPERAL HEMORRHAGE.

By Dr. A. B. Steele, Physician to the Lying-in Hospital, Liverpool.

[It was only owing to the very strong opinion expressed by Dr. Barnes that the writer of this paper first resolved to put to the test of practical experience, the intra-uterine injection of perchloride of iron to arrest post-partum hemorrhage.]

As I am now in a position to speak from bedside observation, and finding from recent discussion that the treatment in question is still *sub judice*, I feel bound to contribute my quota to the settlement of so important a question in obstetric practice.

A circumstance which more immediately determined my adoption of this mode of treatment was the accident of my listening to a graphic description of a case by Dr. Williams of Wrexham, given at a meeting of the North Wales Branch of the British Medical Association, held last summer at Bala, at which I had the good fortune to be present. A lady who had been attended by Dr. Williams in several labours invariably suffered from post-partum hemorrhage to a degree which caused much anxiety for her immediate safety, and rendered each approaching confinement a source of dread to herself and her friends. On the occasion of her last confinement, Dr. Williams determined to try the effect of the iron injection as recommended by Dr. Barnes, and accordingly this was done immediately after the expulsion of the placenta and before hemorrhage had commenced. No sooner was the operation completed than the patient, notwithstanding her usual dread of impending hemorrhage, at once exclaimed, "I am better now, and I know I shall have no bleeding this time;" and such proved to be the case, and her recovery was excellent. This case impressed me so strongly that I resolved to adopt the plan on the first suitable occasion, which shortly after presented itself in the following case:—

A patient of my own, nearly forty years of age, of tender, delicate frame, deficient muscular tone and energy, large dilated veins, and the subject of hemorrhoids, which in the latter months of pregnancy became so aggravated as to necessitate their removal by the clamp and cautery, was taken in labour for the second time. Her first confinement, a year previous, was protracted and difficult from uterine and general inertia, rendering forceps delivery necessary, extraction not being effected without long and forcible traction; hemorrhage ensued, which was controlled by the ordinary measures, but a subsequent draining of blood continued, which, although not excessive in quantity, was nevertheless a source of much anxiety in her already exhausted condition. She however ultimately recovered after a tedious and troublesome puerperal period. The child was born alive, but died in a few weeks from diarrhœa and atrophy consequent upon loss of breast milk and general debility.

The second labour was almost as difficult and protracted as the first; she was delivered after long traction with the forceps of a fine living female child. Hemorrhage again set in immediately after the completion of labour. I at once injected a solution of iron, one part of liq. ferri perchloridi fortior. to four of water, which in a few minutes completely controlled all bleeding, and caused firm and general contraction of the uterus, contrasting favourably with the imperfect and unreliable contraction so common under these circumstances; and which on the former occasion rendered her condition critical for a considerable time. Her recovery, although complicated by constitutional delicacy and feebleness, was nevertheless much more favourable than in her first confinement, and the child lived and thrived well.

The following case, which occurred shortly after that just related, is even more specially illustrative of the value of the iron injection, not only in puerperal hemorrhage, but also in the hemorrhages of abortion.

A patient about twenty-five years of age, a fair delicate-looking woman, the mother of one child, first consulted me for a constant, and at times profuse loss of blood, which had lasted for many weeks, after an abortion at the fourth month. I opened up the cervix with tents and swabbed the uterine cavity freely with the undiluted liq. ferri perch. fortior.; after two applications all bleeding ceased, and *in a short time she was quite* well. About a year later I was called to see her in consultation with her medical attendent, in consequence of post-partum hemorrhage of a formidable character, which had come on about an hour after the completion of labour, and had already caused great depression, approaching to collapse. The bleeding was promptly checked by compression, cold cloths, and the other ordinary means, but reaction was slow in taking place; the patient remained for some hours in feeble, excited state, with delirium and other symptoms of constitutional disturbance, which required close watching for two or three days. She recovered slowly, but completely. In about twelve months after this she was again taken in labour, and I saw her as soon as the pains set in. Her labour was easy and somewhat rapid. Every precaution in anticipation of flooding was adopted: a full dose of ergot just before the expulsion of the head, careful compression of the uterus with the hand throughout and subsequent to the expulsive stage, compress and binder, and so on. For a short time after delivery all went on well, and I left the room, but was soon recalled by the nurse, as the patient told her "there was a good deal coming away." I at once recognised the effects of hemorrhage in her pallid lips and faint condition, and found a large quantity of coagula in the bed and in the vagina, from whence fluid blood was still flowing. The uterus, although not entirely flaccid, was doughy, and did not readily respond to compression. I hastily prepared a strong solution of the solid perchloride (which fortunately I had brought with me), and having cleared the uterus and vagina from clots, during which process I could feel the warm stream still flowing, I threw up about a quart of the fluid, which at once checked the bleeding, and in a few minutes the uterus, and especially the os, was firmly contracted. No further bleeding nor any untoward symptoms followed, and the patient made a better and quicker recovery than she had ever done on former occasions. In each of these cases the patients themselves appeared to appreciate the beneficial effects of the iron injection, and to acquire a feeling of confidence in its power to control the bleeding in a few minutes after its application; a sense of security which, while encouraging to the accoucheur, is not without its beneficial emotional effect upon the patient. I have used the iron in a few other instances, not so typical nor so interesting as to deserve record here; but in all the result has been satisfactory, and unattended by any appreciable after consequences of a disagreeable nature.

I therefore assume from my own experience as well as from that of others that this mode of treating puerperal hemorrhage is both safe and reliable, and under certain circumstances not only justifiable but strongly indicated as one of the most effectual means of rescuing a patient from imminent death.

I am disposed to believe that the action of the iron injection depends not so much upon its direct styptic or hemostatic effect, as upon its influence as a reflex excitor of the incident nerves of the walls of the uterus, and also by directly arousing the peristaltic action of that organ upon which the more powerful muscular actions are as it were based.

One class of cases to which the use of this powerful astringent appears specially adapted are those not uncommon and most troublesome forms of flooding which might be called recurring hemorrhage, where the uterus alternately contracts and relaxes, and where it is difficult to determine when the patient can be pronounced free from risk of further bleeding. Instead of being obliged to grasp the uterus for an hour or two, and feeling afraid to leave the patient perhaps for many hours, the use of the iron at once removes all doubt and difficulty, by inducing firm and permanent contraction.

With regard to the objections which have been raised to the use of the iron injection in flooding, I am not yet convinced that these are to be conceded as sufficiently established by observation to give them weight against the proved safety and efficacy of the treatment when judiciously applied. It has been said that the perchloride acts so powerfully upon the walls of the uterus as to leave a layer of dead tissue of some thickness, which is liable to give rise to septicemia. I am not aware that this has been proved by actual observation; it appears to me improbable when the antiseptic property of the solution is considered.

The few reported fatal cases in which the injection had been employed, are not to my mind conclusive evidence of its supposed ill effects.

Deaths from septicemia and other puerperal complications following profuse flooding were sufficiently frequent before the treatment in question was adopted, to render it at least doubtful whether the mortality has not been due to other causes than the use of the injection. Granting, however, that in this, as in some other powerful remedies employed in circumstances of great and immediate danger to life, there may be a certain possible contingent risk of subsequent mischief, it then becomes a question whether we shall allow a patient to bleed to death before our eyes rather than employ means which we feel confident will rescue her from impending death, although they may subject her to possible future risk.

The mode of applying the remedy has been so fully and accurately described by Dr. Barnes as to render it unnecessary to add anything on that point. It may be well, however, to repeat one condition insisted upon by him, which if neglected will probably cause failure. Before injecting the fluid into the uterus, all coagula or remaining portions of placenta structure must be carefully removed.

A woman was brought into the hospital literally bleeding to death after an abortion at the fourth month. I opened up the cervix with tents and freely swabbed (I never inject the non-pregnant or immature parturient uterus) out the cavity with the undiluted liq. ferri perchloridi, but the bleeding, so far from ceasing, appeared to flow more freely than ever. As a last resource I introduced a finger up to the fundus and with infinite difficulty scraped off a minute particle of placenta structure, after which the hemorrhage ceased and the patient slowly but completely recovered.—*Obstetrical Journal, June* 1873, *p.* 168.

96.—GALVANISM IN POST-PARTUM HEMORRHAGE.

By Robert Storrs, Esq., Doncaster.

On June 24th, 1873, I was sent for to see a patient in labour, suffering from puerperal convulsions. The convulsions came on during the first stage of labour, and could only be checked by keeping her under the influence of chloroform for some time. Failure of uterine action occurred before the os uteri was fully dilated; but, as it was sufficiently dilatable, the forceps was introduced, and delivery accomplished. Still the uterus did not contract, and, after the placenta was removed, hemorrhage could only be restrained by keeping the hand within the uterus. Grasping and kneading the uterus, cold affusion externally, and injections of cold water per vaginam produced no effect. A dilute solution of perchloride of iron was freely injected into the uterus, but proved ineffectual. My assistant, Dr. Macintosh, then procured for me at my request one of Stöhrer's portable coil machines. An interrupted current of considerable intensity was directed through the uterus, one pole of the battery being applied over the abdominal walls directly over the fundus by means of a curved plate ot copper, and the other placed in the cervix. Almost immediately firm contraction took place, and when the current was discontinued the uterus remained securely contracted. The patient made a good recovery.

To those who have witnessed as I have done the confusion and other disagreeable accessories of a case of obstinate hemorrhage after labour, any means that tend to diminish these inconveniences must be most acceptable. The use of galvanism as a means of treatment in these cases, besides its remarkable efficiency as shown by this case, possesses many other advantages over the means usually employed; for instance, its cleanliness, the simplicity of its mode of application, and the avoidance of those evil consequences in the puerperal state which are so apt to result from irritation of the uterus by injections, such as that of the perchloride of iron.—*Lancet, Sept.* 6, 1873, *p.* 363.

97.—A CASE OF EXTRA-UTERINE FŒTATION SIMULATING OVARIAN DROPSY.

By Jonathan Hutchinson, Esq., Senior Surgeon to the London Hospital.

[The appearance and physical characters of this case exactly resembled those of ovarian disease except in this particular, that it did not pass into either loin, but occupied the middle of the abdomen. The woman's menstruation had been regular for the last six months, and prior to that there had been a period of nine months' suspension, during which she had supposed herself pregnant.]

On Monday last, the patient having been transferred to my care for that purpose, I tapped the tumour. The patient was at the time on her left side in the recumbent position. The trocar was introduced about two inches below the umbilicus. A dark brown fluid, like linseed tea, escaped, and at the sight of it I at once abandoned all hesitation as to the diagnosis of an ovarian cyst. I have seen just such fluid numberless times from ovarian tumours, and it has rarely happened to me to see anything just like it and in large quantity from other sources. It was the feeling of certainty produced by the appearance and quantity of the fluid which prevented me from making any further examination with a view to diagnosis. After the fluid had been flowing some little time, we observed that the end of the trocar was drawn down towards the pubes, and it became necessary to introduce a probe in order to obtain a full stream. The former fact was explained, as we supposed, by the subsidence of the tumour into the pelvis, and the latter by its multilocular character. The stream became irregular and greatly diminished before we had materially reduced the bulk of the tumour; and, believing that we had established the fact of its being a multilocular ovarian cyst, and therefore a suitable case for ovariotomy, I ceased to be solicitous about getting any larger quantity of fluid away, and withdrew the canula. The total quantity removed was, I believe, between four and five pints. I did not manipulate the tumour much afterwards, but just sufficient to ascertain that it still fluctuated freely in some parts, and felt firmer in others. My intention was to examine it more carefully in a few days, when the trocar puncture in the cyst would be closed. This opportunity was, however, not afforded us. The patient began to suffer from symptoms of peritonitis almost immediately after the operation, and on the following day she had a high temperature and great abdominal tenderness. The tapping was on Thursday at four o'clock, and she died at half-past four in the afternoon of the following Sunday, exactly seventy-two hours after it. Her symptoms had been distressing sickness, abdominal pain, and rapid pulse. On the Saturday and Sunday her pain had ceased, and on Saturday she had suffered me to handle her abdomen, and assured me that, excepting close to the wound, she had no tenderness. This absence of tenderness is not uncommon in the later stages of peritonitis.

At the post-mortem examination we found the tumour adherent everywhere to the abdominal wall and to the omentum. It had also firm adhesions to the intestine in several places. Everywhere in the abdomen there were evidences of recent acute peritonitis, and in the depending positions there was a considerable quantity of a thin, brown fluid, much like what we had removed at the exploration, only greatly diluted. The tumour itself contained also a large quantity of fluid. At its base, to the right side, was a spot which was almost gangrenous, and which showed a perforation, but I am not quite certain that the perforation was present before the adhesions were torn through. On the opposite side there was another patch in a similar condition. I think there can be little or no doubt that fluid from the cyst had escaped into the peritoneal cavity, but whether through one of these perforations or through the vaginal puncture, or both, it was impossible to say. At the time of the woman's death the trocar wound in the cyst was completely sealed up by adhesions, but it is of course possible, and I think probable, that immediately after the operation it allowed fluid to escape. The walls of the cyst were tolerably thick, but were much softened by inflammation. On the outside, almost everywhere, it was roughened by shreddy patches of cellular adhesion which we had torn through. My first suspicion *that it was not an* ovarian tumour recommenced when we found that it was in *the middle line*, that it had no pedicle, and that it concealed the uterus. On cutting into it, a large quantity of fluid of the same character as that removed by paracentesis, only much thinner, escaped, and the body of a macerated fœtus was found at its bottom. The umbilical cord of the fœtus was still continuous with the placenta, but it was rotten, and tore through during removal. The placenta and fœtal membranes, in a much degenerate condition, adhered firmly to the walls of the cyst, which they served to strengthen. The fœtus, considering that, according to the history, it had been dead for six months, showed less signs of decomposition than might have been expected. It was a female, and at full term. It was not shrivelled, but sodden and flabby. Its eyeballs were quite collapsed, and we noticed, as one peculiarity, that its joints were exceedingly loose, as if it had no ligaments. Its anus was wide open, and would have admitted the finger, and contained a small quantity of meconium. I mention these peculiarities because there was really nothing about the fœtus in general appearance which would have made one think it had been dead more than a fortnight. On further dissection of the parts, the case proved to be one of gestation in the left Fallopian tube. The uterus was pushed over to the right side, and was in close connexion with the walls of the cyst. The left Fallopian tube could be traced for a short distance on the front of the cyst, where it became thinned out and lost. The left broad ligament passed downwards from the front of the cyst, and between its extremity and that of the Fallopian tube was a thickened mass, which might perhaps be the remains of the ovary, but it was not practicable accurately to identify it.

Thus, you will see, we have had to deal with a case of tubal gestation, with the not unusual history of death of the fœtus at the full term, and its retention afterwards. The case differed from the usual course in the distension of the cyst with fluid, which had steadily increased in quantity, and thus simulated ovarian dropsy. I believe that this is very exceptional. I have had to deal with several cases of extra-uterine fœtation, but never with any in which, with a full-sized fœtus, there was any difficulty in discovering the latter. Usually the quantity of fluid is but small, and after the death of the fœtus the cyst contracts on its contents. In this instance, however, the woman was steadily increasing in size, and hence the error in diagnosis. As regards the cause of the patient's death, there can be no doubt that it was due directly to the exploratory tapping, which in all probability allowed of the escape of fluid into the peritoneal cavity. The existence, however, of perforating ulceration of the back of the cyst in two places suggests the probability that, even if the exploration had not been done, the cyst would not have long retained its contents.

Although, probably, as I have just said, it is rare for the cysts of extra-uterine fœtation to contain enough fluid to cause resemblance to ovarian dropsy, yet now and then it does so happen. I have found a case very similar to this in respect to the difficulty of diagnosis from ovarian dropsy, and also in the fact that an exploratory tapping was performed, in a report on the treatment of extra-uterine fœtation which I published in one of the medical journals in August 1860. Curiously enough, it was under the care of a namesake—my friend Mr. Francis Hutchinson, of Blackfriars. The woman had believed herself pregnant, and, excepting that she had suffered from violent attacks of pain, her pregnancy had passed to its full term without any unusual occurrence. At the end of the nine months labour pains set in, and they lasted more or less for about three weeks. At length the pains ceased, and after this the woman never again felt what she supposed to be the movements of the child. Six weeks later Dr. Ramsbotham, who was, without doubt, the highest authority on this subject, was called in consultation, the woman being ill, emaciated, with an irritable stomach, rapid pulse, and dry tongue. As the tumour was large, and fluctuated freely, the diagnosis of ovarian dropsy was made, and paracentesis performed. About six pints of thick fluid were removed. The trocar puncture never healed, but became a fistula. A fortnight later a lock of fœtal hair presented itself, and thus cleared up the diagnosis. Two months after this, the patient having become still worse in general health, and there being a continuous drain from the cyst, a free incision was made, and the fœtus extracted. The placenta was left to come away afterwards. The patient made a good recovery, and regained perfect health.

Thus *you will see that Dr. Ramsbotham* was more lucky than myself in that his patient did not die of the exploratory tapping. The difference in our cases probably was this, that in his inflammation had occurred and the cyst become adherent, whereas in mine, there being no adhesions, fluid escaped into the peritoneal cavity. It would be very unsafe to infer from what followed in his case that paracentesis is the best mode of treatment, or even of exploration, in these cases. In some, as has been proved in my case, it is in itself attended by great danger; nor shall I deal honestly with you or with myself if I do not here candidly admit that with due care and patience I do not think that paracentesis ought to be necessary in a case of extra-uterine fœtation simulating ovarian dropsy. If another case of like obscurity should come under my care in which, on examination with the patient on her back, no solid substance could be detected, I would at once make the woman support herself on her hands and knees, and in that position I have no doubt that the fœtus, if present, would be felt. I need not tell you that I exceedingly regret that I did not try this simple manœuvre in the case before us.

We will now turn to consider what our treatment ought to have been had my diagnosis been correct and had I felt certain as to the nature of the case.

In the year 1860 I published the paper from which I have already quoted on this subject, and collected all the evidence I could find, in order to establish principles of treatment. To this paper I must ask you to allow me to refer. It will be well that we should restrict our attention to cases really similar to the one before us—that is, to those in which the fœtus dies at the full period of gestation, and in which the woman continues to carry it, and, for a time at least, without suffering much in health. Most of you are probably aware that a large majority of cases of extra-uterine fœtation never go on to the full period, but end by rupture of the cyst and hemorrhage into the peritoneal cavity at an early period. There are a few in which this does not take place, and to these I now ask your attention. Several different events were possible in our patient's case. First, the tumour might have become quiet, the fœtus might have shrivelled, and the woman might have lived on to the term of her natural life. Such things have happened more than once, but they are exceptional to the rule. Cases have indeed occurred in which a woman still bearing a dead encapsuled extra-uterine fœtus again became pregnant and bore a living child, or even became pregnant several times in succession. This, however, is less likely to happen if the dead fœtus be of full size than when its death happens early. A second possible event is that the fœtus might have caused inflammation and an abscess, and that thus either the mother's death might be brought about or an opportunity for surgical interference under advantageous circumstances might occur. Thirdly, an operation for the removal of the fœtus might have been performed without waiting. In the paper to which I have referred I have tabulated (chiefly from the excellent monograph of Dr. Campbell) twenty-one cases in which the fœtus remained quiescent; thirteen in which the mother died without any attempt, either natural or by art, having been made for the removal of the fœtus; sixteen in which ulceration occurred and the remains were spontaneously extruded (three deaths); thirteen in which, after an abdominal fistula had formed, the removal of the fœtus was assisted by the surgeon (two deaths); and, lastly, sixteen in which, without waiting for suppuration to occur, the surgeon boldly cut into the abdomen and extracted the retained fœtus. To these, however, I must yet add another group in which the fœtus was got rid of not through the abdominal wall, but by ulceration into the vagina or rectum. The total number mentioned in the table is 102, and of these no fewer than 72 of the patients survived. Probably this ratio of good results is far beyond the real truth, since the cases which recover are precisely those the most likely to excite the interest and perhaps the astonishment of the surgeon, and thus secure themselves record in print. Still, making allowance for this, there is abundant proof that the retention of a fœtus in an extra-uterine cavity, although very dangerous, is by no means an absolutely fatal condition. As the result of a careful investigation of the evidence, I formed a strong opinion in favour of leaving such cases to nature up to a certain stage. I would not on any account in such a case undertake what is *called a primary* abdominal section—that is, I would not operate unless an *abscess had already* formed. The statistics seemed to show that whilst, out of the total of 102 patients, only 29 died, out of 16 in which primary abdominal section was performed, more than half died (9). This will give us a mortality of more than one in two when interference occurs before suppuration has taken place, and of less than one in four in those in which nature was allowed time for her own processes.

Let me state clearly that it is not intended to discourage surgical interference at all stages, for I believe that it is very valuable, and comparatively safe, after inflammation has occurred and adhesions formed. The danger comes when incisions are made into a peritoneal cavity as yet unobliterated with the risk of escape of fluids into it. If, then, in the case which I have been detailing, we had been able to form a confident opinion that it was one of extra-uterine fœtation, I should have advised the patient to wait, and should certainly have declined for the present to interfere. Bearing in mind that the tumour was getting larger from increase of fluid contents, and that its walls had in some parts thinned and softened, I think it not at all improbable that it would have given way into the peritoneal cavity, and thus have produced death; but, on the other hand, we must admit that it is possible it might have suppurated, given way externally, and thus allowed opportunity for the removal of the fœtus, or that the tendency to distension might have come to an end, and that the patient might have lived for some time without serious inconvenience. I do not think that the latter is probable, because the patient was already suffering a good deal of pain and inconvenience from the enlargement of the tumour.

At the risk of somewhat repeating what I have said, I will read to you the concluding sentences of the report to which I have alluded, since they convey in somewhat more detail a statement of my opinions on this not unimportant point in practice. (For extracts, not given here, see Medical Times and Gazette, Aug. 1860.) Those opinions I may, however, briefly sum up in the following practical rule—*that extra-uterine fœtation cysts ought not to be meddled with in any way, either by puncture or incision, until suppuration has occurred and an abscess fistula has been formed.—Lancet, July* 19, 1873, *p.* 71.

98.—GASTROTOMY IN A CASE OF EXTRA-UTERINE GESTATION.

By F. H. MABERLY, Esq., Surgeon Chloroformist to the Dental Hospital, Birmingham.

Mr. Bailey, surgeon of this town, requested me, on June 21st, to see Mrs. ——, a young woman aged 27, mother of two children, the youngest being two years old. I found her suffering from excrutiating pains in the abdomen, and constant sickness. Her face was pinched, and expressive of great anxiety; the pulse rapid and feeble. On examining the abdomen, I found its walls exquisitely sensitive, so that no pressure could be borne; yet, on the right side, could be seen and felt a solid substance, springing evidently from within the pelvis, rising to a level with the umbilicus, and slightly crossing the median line to the left. The history showed that this substance had been forming for about six months, during which time she had constantly suffered with severe intermitting pains of a bearing-down nature, together with frequent nausea and vomiting; the bowels had been very costive, with tenesmus coming on at intervals, and sometimes she had had complete retention of urine for some hours; all these symptoms had become very much aggravated about ten days before I saw her. There were no breast-signs of pregnancy, and she had never felt any movements. A vaginal examination with the finger revealed the uterus carried high up, and greatly anteverted. The menstrual discharge had been very irregular from the commencement of the symptoms. She was ordered full doses of opium, and a turpentine stupe to be applied over the abdomen. Next day she appeared somewhat better, and I did not see her again till the evening of June 26th, when I found the symptoms, if anything, increased in severity, and she had had some rigors. As no efficient examination could be made without the use of chloroform, I determined to place her under its influence on the following day; and having done so I was enabled, by the additional evidence thrown on the case, to express a decided opinion that it was one of extra-uterine pregnancy; for not only did the history and symptoms entirely favour this view, but nearly every other possible disease could be excluded.

Owing to the extremely precarious state of the patient, it was thought advisable, before proceeding to operate, that another opinion should be had, although the patient and her friends were most anxious that what was necessary should be done at once, notwithstanding only a very slight chance of recovery was held out to them. I was extremely fortunate in obtaining the opinion of my esteemed friend, Dr. Savage, the same afternoon, when he agreed that immediate operation was the proper course. I therefore, in the presence of Mr. Bailey, Dr. Savage, Mr. Priestley Smith, who kindly gave the chloroform, and two others, performed gastrotomy.

An incision about four inches in length was made in the median line between the umbilicus and the pubes. Dissecting down through the linea alba, the peritoneum was reached, and opened upon a director, when a quantity of dark blood began to flow from the abdominal cavity. The tumour was now exposed; and, as it was somewhat elastic to the touch, an ovarian trocar, for safety's sake, was thrust into it; but, as no fluid escaped, I withdrew the instrument, and introduced my finger through the opening it had made, when, to my extreme satisfaction, I found my diagnosis to be correct, as I at once felt the limbs of the fœtus. Opening the cyst, I removed it by the feet; and, in doing so, found the head lodged deeply in the pelvis. The fœtus weighed about a pound and a half. All blood was removed as completely as possible from the abdominal cavity. Its effusion had been caused by a rupture in the cyst, which no doubt occurred at the time when the symptoms increased in severity—viz., about ten days before I saw her. The cyst-wall was then stitched to the abdominal parietes, the placenta being left *in situ*. A pad of wet lint placed over the wound, covered with tenax, and a bandage, completed the operation. The patient was then removed to bed; and a small quantity of brandy was administered, as she appeared faint. She soon, however, became very restless, and in about three hours breathed her last. The next day, I was permitted to examine the parts. I therefore enlarged the abdominal wound, and exposed the cyst, &c. The peritoneum in the right lumbar region was covered with blood, which had evidently been effused for some days. Adhesive inflammation had been set up about the part of the cyst where the rupture had occurred, glueing it to the ascending colon, and slightly to the parietal peritoneum. The omentum was likewise covered in patches with discoloured blood, and was of a greenish cast in many places. The cyst was found to be connected with the right Fallopian tube, and no trace of an ovary could be found on that side. The uterus was rather enlarged. The ovary, round ligament, and Fallopian tube on the left side, were all healthy.—*British Medical Journal, July* 26, 1873, *p*. 81.

99.—ON THE OPERATION FOR RUPTURE OF THE PERINEUM AND PROLAPSE OF THE WOMB.

By James Fowler, Esq., Surgeon to the Wakefield Hospital and Dispensary.

To say nothing of the comparative difficulty of even ordinary labour; when the vaginal orifice is small and unyielding, the head of the child large or extensively ossified, or the presentation and birth otherwise abnormal, even with the greatest skill and attention, laceration of the perineum is a common accident—in some cases it is inevitable; and lacerations of the whole length of the perineum, even though not involving the rectum, if not, as frequently, a source of distress and misery at once, are at least liable at any time to become so. Besides the inconvenience and discomfort incident to the patient herself, and the prolapsus, rectocele, or cystocele, which are almost sure sooner or later to follow, and which furnish so large a proportion of our hospital cases, the attachments of the constrictor vaginæ and other muscles of the perineum being torn through, the normal conditions for proper intercourse no longer exist. Accordingly, it is now generally admitted that ruptures of the perineum should be dealt with at once, on the completion of labour, or at least as soon after as practicable. For several years this has been my own practice, and with the best results. I have brought the edges of the wound together by means of deep quill and superficial sutures; kept the bowels confined as long as necessary; drawn off the urine regularly with a catheter; kept the patient on a water-pillow; had the vagina daily washed out with water and disinfectants; and have been rewarded by always having had union follow—a union the strength of which has in several cases been tried by subsequent labour, and remained firm and elastic. A perineum will, I know, sometimes give way in subsequent labour, however carefully repaired. A lady, not at that time a patient of mine, whose perineum was ruptured in her first confinement, was operated upon in London ten weeks afterwards, in consequence of the great suffering which ensued, by one of the most distinguished surgeon-accoucheurs then living; but four years afterwards, notwithstanding every precaution, it gave way during her next labour. I brought the wound together in the usual manner at once, and secured capital union, avoiding in that manner all the inconvenience and trouble she had previously experienced; but there has been no labour since to test the strength of the parts.

A perineum, again, though apparently well repaired and remaining good through after labours, will not always prevent subsequent prolapse of the womb; relaxation of the vagina and ligaments of the womb, perhaps, ensuing long after the operation has been performed. In some cases, possibly, the removal of mucous membrane may not have been as free as it well might have been; in others the union may not have been carried sufficiently high. But from whatever cause the failure of the operation may have arisen, the fact that there has been a failure at all has repeatedly been urged as a practical objection against its performance. It is in order to show, on the other hand, what complete relief may be afforded by a judicious operation, that I give notes of the following case.

J. R., aged fifty-six, was confined for the fourth and last time, eighteen years ago. She had a slow labour. The child was very large, and instruments were sent for; but before they arrived, all was over naturally. She felt afterwards unusual pain and tenderness of the perineum, and had much scalding in making water. At the end of a fortnight, however, she got up, and did not at once feel much inconvenience. Fourteen years ago she ceased to menstruate. Eight years afterwards she began to have great pain in the back on standing, with occasional bearing-down; and four years later, on lifting a recently filled water-filter, "felt something give way in her inside," and the womb appeared externally. Since then she has been unable to keep it up either with pessaries or any other contrivance. She has had to give up washing; has been unable even to iron a shirt; has not been able to stand during the singing of a hymn in a place of worship; has been unable to walk more than a quarter of a mile at once at the furthest; for weeks together has often been unable to get out of the house at all; has suffered almost constantly from leucorrhœa; has often been faint, and sick, and off her food; and, more than all, has had to suffer the domestic misery and distress arising from such a state of things.

Such was her history when she came into our hospital on the 3rd of April, 1871. Her countenance was blanched and expressive of great anxiety. On making an examination, the womb was found extensively prolapsed; there was an ulcer, about as large as a shilling, on the os; there was a great deal of leucorrhœa; and the perineum was ruptured backwards as far as the verge of the anus.

Having been placed under the influence of chloroform, in the position for lithotomy, the sides of the fissure were completely denuded from the point where the posterior commissure of the vulva should naturally exist, backwards and upwards towards the rectum, which was next freely denuded in like manner. Immediately in front of the rectum the denudation was carried somewhat higher than usual, in order to produce a certain amount of subsequent contraction of the vagina, and thus prevent the redescent of the womb. Having waited patiently now for some time, and applied ice until the bleeding had entirely ceased, two strong waxed-silk sutures were inserted by means of a long curved needle, and firmly tied on quills on either side the wound, for deep union; and three of stout silver wire, for superficial union of the edges of the skin along the middle line of the perineum. On passing the forefinger into the vagina, apposition of the two sides of the fissure was found to be complete.

Placed on her left side on a water-cushion, with the knees tied together and flexed on the abdomen, the treatment was the same as after operations generally, except that, in addition, the bowels were confined with opium, the urine regularly drawn off every six hours with a catheter, and the vagina washed out every day with a stream of warm water and Condy's fluid. I have no regular time for removing the sutures, but in this particular case the deep ones were taken out ninety hours, and the superficial ones a hundred hours after the operation. Union was found to be complete and firm. On the seventh day the bowels were relieved by castor oil and an enema, so as to cause as little disturbance of the new perineum as possible. In order to avoid any possible strain, and to allow the swollen and ulcerated womb to recover its normal condition, and its relaxed ligaments to strengthen, bed was enjoined for three weeks longer; and at the end of the month she drove home in a cab.

For a long time she could not believe that she was really cured, but gradually felt her way about her household duties. The womb has never been down since, nor has she ever had the slightest feeling of pain or inconvenience. She can wash a fortnight's wash and iron and clean for a family of four regularly now without assistance, fetching and carrying water, emptying the tubs, &c., all by herself. She also walks to Wakefield every week in the middle of the day to market—a distance of four miles, does her shopping, and then walks back again in the evening, without the slightest feeling of local weakness; and gets along faster than one of her years and figure (rather stout) might be expected to do. She can also stand quite comfortably for any length of time. Her general health is as good as possible, and she has never had any leucorrhœa, since she left the hospital, now more than two years ago.

By an operation of this kind it must be admitted therefore that the evils of a torn perineum may be completely remedied. If not raised to an ideally perfect condition, the perineum thus treated is at least brought back to the state in which it was before the accident, which is sufficient—at least in the great majority of cases—for all practical purposes.—*Lancet, July* 19, 1873, *p.* 79.

100.—ON INJURY TO THE RECTUM IN PARTURITION.

By Christopher Heath, Esq., Surgeon to University College Hospital.

Every now and then in primiparæ, and especially in women advanced in years, when the head of the child is on the perineum, the perineum gives way. The tear may simply be in the perineum, or it may extend through the sphincter ani and lay open the rectum. This is of course very serious both for the patient and also sometimes for the doctor; but it is to be remedied, and usually remedied with success if the case is treated at once. You have a tear through the anterior part of the sphincter, and you have to bring the parts into apposition so that they may heal while fresh. The best plan is the ordinary method pursued for ruptured perineum with deep sutures. It is not necessary to have recourse to the old-fashioned quill suture, which is apt to give rise to ulceration by the pressure exerted upon the edges of the wound. Take an ordinary good, large, curved needle, and pass it in deeply on each side of the ruptured perineum, going well down to the bowel, or even into it. Use a good piece of thick wire, twist the ends together over the front, and get the parts drawn well together. You can leave these sutures in much longer than you can leave quilled sutures in, and with less damage to the skin and surrounding parts.

But if it is a case of rupture *into* the rectum you must do something more. It is no doubt owing to contraction of the sphincter ani that some of these cases do so badly. In former times, before the operation on the perineum, Mr. Copeland Hutchinson, a rather celebrated surgeon in his day, proposed to treat the cases simply by dividing the sphincter ani on each side. He was perfectly right in his view, and I believe he carried it out in one or two cases; and so also did Mr. Mayo. The patient lay afterwards with the legs bound together, and all fæces passed involuntarily out of the sphincter without the sphincter contracting; and they found that they did occasionally get the parts to unite. We have nowadays the method they practised, but, in addition, we put in deep sutures; and therefore, having done that, I advise you to pass the finger into the anus, and, with a blunt-pointed straight bistoury, to divide the spincter on each side. It is not sufficient to divide it on one side, because the sphincter is really a double muscle. Keep the patient quiet and lock the bowels up with opium, and you will probably have a good result; at all events, it is the best chance for the patient.

It unfortunately happens, however, that you do not always obtain a complete result. The patient recovers with an apparently sound perineum, but discovers that she passes fæces and flatus per vaginam. It is a very painful thing for a woman to find that flatus escapes without her will, for she is thereby shut out from society. Of course there must be a small fistula if simple flatus passes; if it be larger you have in addition fæcal matter passing through into the vagina, and thus escaping. We have of course here the condition known as recto-vaginal fistula, and it is easy enough to see, if large, by putting the patient into the lithotomy position. If minute, however, giving passage only to flatus, it is not always easy to find it. You may have to hunt for some time for it, as in the case of fistula in ano. Having found the fistula, the object is to close it. If minute, you can often effect this by using the wire made hot by a galvanic battery, but if large you must pare the edges and sew up the opening. My own personal experience is unsatisfactory, and surgeons who have had a more extended experience of recto-vaginal fistula find it a difficult thing to cure. The reason is this, that the tissues upon which you have to operate to get the raw surface to unite are so exceedingly thin that to pare them so as to get to a broad raw surface to unite is very difficult to manage. I have notes of two cases that were operated on which were relieved, but I am sorry to say they were not cured. You will generally find that these operations need to be repeated three or four times or more before you can completely close the opening. Briefly, one case was this. The patient was a woman, aged twenty-three, having two children. Her first confinement was difficult; she was delivered by instruments, and ever since she has been able to get about she has found her motions passing by the vagina. I may remark here concerning the use of instruments, that both the surgeon and the instruments are often blamed unnecessarily. It is the common story, "Mr. So-and-so used instruments, and the woman has never been well since." It is not the fault of the surgeon, or of the instruments, but it is commonly the fault of an ignorant midwife who has allowed the head to rest so long upon the parts as to make them slough, as is seen also in vesico-vaginal fistula, a result which is more frequent than recto-vaginal. The woman came into the hospital, was operated on here, and was improved, but was not cured.

The other case was that of a woman aged thirty-one. In her first confinement her attendant put on the forceps, and in delivering the child the perineum was torn. He immediately put the parts together, and put deep sutures in, and treated the case *secundem artem*. But she had an opening into the rectum just above the sphincter, through which both fæces and flatus passed. I performed an operation on it with great care, making the edges of the fistula raw, and not merely paring the edges themselves, but taking a good broad piece off, and then bringing the surfaces together with sutures. It is very difficult to do it effectually, for there is a great tendency for it to break down again the first time the patient has a motion. This unfortunately happened in this patient. We locked up the bowels for some time, but they acted rather too soon, and we had all our work undone, and she is coming in again to have another operation performed.—*Lancet, May* 24, 1873, *p.* 726.

101.—ON AN IMPROVED METHOD OF EXAMINING AND SURGICALLY TREATING THE FEMALE RECTUM.

By Dr. Horatio R. Storer, Surgeon to St. Elizabeth's Hospital for Women, Boston, U.S.

[This paper was elicited by a lecture of Mr. C. Heath's, in the Lancet for January 18th. Mr. Heath, after speaking of the advantage of administering an enema previous an examination, and having the patient force out the lower gut by subsequent straining, mentions the ordinary digital method, employment of the common anal specula, and that of the endoscope.]

He says also that it is advisable in examinations of women to pass the finger into the vagina as well as the rectum, since "a rectal stricture, for instance, can often be much more thoroughly examined through the vagina than is possible if the examination is confined to the rectum itself." Here, however, he stops, just short, it will be perceived, of accomplishing the manœuvre now to be described.

For many years I have been in the habit of attaching importance to the condition of the rectum in uterine cases. My attention was first drawn to their correlation by Sir James Simpson during my residence in Edinburgh in 1854–56, and it was subsequently increased by Mr. Baker Brown's admirable chapters upon the effect of morbid conditions of the rectum upon uterine disease and of those of the uterus upon rectal affections. Finding the ordinary methods of exploration, such as Mr. Heath has described, tiresome, imperfect, and in every way unsatisfactory, it occurred to me that *eversion of the anus and lower rectum by digital pressure from within the vagina* might give better results, and such has proved to be the case.

As I have described the process at some length in a series of papers upon the relations of the rectum to uterine disease, published several years since in the American Journal of Obstetrics and Diseases of Women and Children (New York), and have also spoken of it in the Journal of the Gynæcological Society of Boston, it will be unnecessary here to do more than state some of the results that it enables us to obtain. As to the manner of effecting the eversion, it is only requisite, the patient being upon her side, to introduce the finger into the vagina a sufficient distance, and then to press steadily backwards and downwards upon the sphincter ani; this muscle will relax in consequence, and allow the rectal wall for a large portion, and in some cases the whole of its circumference, to turn itself outward through the anal orifice, very much like the everted finger of a glove. Should the sphincter ani be hypertrophied or irritable and resistant, as in some cases of fistula, &c., and digital pressure from within the vagina prove insufficient to produce eversion to the extent that is desired, it may at once be accomplished by previously rupturing the sphincter by rapid dilatation with the thumbs, as suggested by Récamier for the treatment of anal fissure. This is a very simple procedure, unattended by any subsequent ill-effects, and I have now resorted to it for many other indications and in a very great number of cases.

1. *Artificial anal eversion for purposes of diagnosis.*—Every one familiar with rectal surgery knows how important it is to get at the precise character and extent of the existing disease, whatever that may be, and that it cannot be accurately effected by the touch alone. Were the rectal mucous membrane unbroken by folds this remark would still be true, but rugous as it is, the most skilled touch will often fail to detect quite extensive lesions. The larnygoscopist who should undertake the discovery, much more the differential diagnosis, of his special class of affections by the finger would be justly laughed at, and attempts to treat them through specula as awkward and unsatisfactory as are the best forms employed for the rectum would be considered even more blameworthy. It will probably not be long before the usual methods of rectal manipulations are considered just as unsatisfactory by surgeons.

By the method now described, one is able to ascertain the exact position of the internal orifice of fistulæ—upon the necessity of discovering which Mr. Heath lays much stress in a subsequent lecture (Lancet, Feb. 22nd, 1873), although he can only suggest "hooking" for it by a bent probe,—the site and extent of ulcers, whether specific or otherwise, the attachment and character of internal hemorrhoids, the presence of minute foreign bodies that may have become imbedded in the mucous membrane during their passage from above, &c., to an almost indefinite extent.

2. *Artificial anal eversion for purposes of treatment.*—The extent of the new field here opened to the surgeon is even greater than that already stated. In addition to the fact that he is enabled to perform the usual operations with greater exactitude, such as cauterisation by acid nitrate of mercury or the hot iron, without injuring contiguous healthy membrane, as is else almost necessarily done; the excision of internal hemorrhoids, which is so preferable in every respect to ligation, now that even the slightest risk of subsequent concealed hemorrhage can be so surely prevented, &c., it becomes easy to accomplish others hitherto almost impossible, at least so far as the obtaining a perfectly successful result. Take, for instance, imperforate anus in the female. In these cases, the pouch of meconium at the extremity of the gut almost always projects against and beneath Douglas's fossa. It will be found that the infant vagina admits a finger without difficulty, and that by a little discreet pressure thereby from within the pouch can be so flattened and carried downwards that anal incision into it can be made infinitely more accurately than has yet been possible; and also that the most important step of all in the operation, the stitching of the extremity of the mucous membrane to the external integument, can be effected without difficulty. In cases where a congenital vaginal *cloaca* exists, the operation for its effacement and the completion of the natural canal can almost as easily be performed.

It is hardly necessary to refer to the cases, not so very rare, especially in the insane, where foreign bodies of large size have been introduced into the rectum, or to the very common ones of its impaction by hardened fæces. In both these classes outward pressure from within the vagina accomplishes removal in a fraction of the time otherwise required.

It may be well to add, in the present paper, that visual exploration of the upper rectum, even for some distance within the sigmoid flexure, is perfectly practicable without the endescope, and to a much greater extent than can be accomplished by that instrument, where the sphincter ani has been relaxed by forcible dilatation in the manner above referred to. A full-sized vaginal bivalve speculum—not the little anal affair referred to by Mr. Heath—can then be introduced without difficulty, and the upper portion of the canal is rendered as accessible for all purposes of diagnosis and operative treatment as the vaginal cul-de-sac itself.

This last hint will not be lost upon those engaged in the treatment of the rectum, and even prostate and bladder, in the male, for I have found that by forced anal dilatation, and the use of a large-sized vaginal bivalve—that of Cusco, for instance—it has become possible also to revolutionise practice in this direction, as well as in the treatment of women.

With regard to the priority of the suggestion of anal eversion by pressure from within the vagina, I may state that not only has it been new to the many physicians who have seen me perform it in hospital and private practice during the last ten years, but that no trace of it could be found among leading British and Continental gynæcologists by one of my pupils, Dr. Stone, now of St. Paul, Minnesota, whom I had requested to make special inquiries as to this point during a late trip to Europe.—*Lancet, May* 31, 1873, *p.* 766.

102.—OPERATION FOR RECTO-VAGINAL FISTULA.

(Under the care of Dr. DEMARQUAY, Paris.)

In the course of a recent visit to the wards of Dr. Demarquay, at the Hospice Municipal, we witnessed the above operation, some of the circumstances of which are of interest. The operation was very ably performed on a woman aged thirty-four years, in whom an incision had been accidentally made by a surgeon in operating for polypus through the recto-vaginal wall in about the situation of the mesial line.

Operation.—The patient was laid on the left side and put under the influence of chloroform. The duck-bill speculum was introduced into the rectum, so as to explore the region. This is quite an original feature in Dr. Demarquay's operations on the rectum; he always uses a small duck-bill speculum as being infinitely superior to the common speculum ani in affording light and space. The incision was then disclosed to the view, measuring about two centimetres in length, and easily admitting the operator's fingers. Dr. Demarquay then slit the sphincter ani and rectum towards the coccyx with a bistoury. This was done with a twofold object—to give room for performing the operation, and to facilitate later the passage of the fæces, so that they might not bear too directly on the situation of the repaired fistula. Slight venous hemorrhage being stopped, the patient was then placed on her back, with the pelvis overhanging the edge of the pad, and the legs well held up by two assistants. A duck-bill speculum was introduced, and placed against each side of the vagina, so that the lateral walls were widely stretched open. The lips of the fistula were then largely pared, Dr. Demarquay insisting on the importance of touching up the edges of the fistula over an extended surface. Arterial hemorrhage was stopped by means of Kœberlé's pincers (with notches in the handle, so that the pincers hold on by themselves) and two ligatures. Seven sutures, with silver wires, were then applied; and, on the vagina being carefully washed out by means of an injection, the lips of the incision were seen to be nicely and exactly adapted. Whilst the sutures were being applied, a large duck-bill speculum, passed under the pubes, held the labia widely open, whilst a small sponge at the end of a pair of pincers was introduced into the rectum in order to hold up the lips of the incision, and to prevent the posterior wall of the rectum from being taken up by the needle.

The treatment consisted of warmth, absolute quiet, opium, and sounding for urine every six hours.—*Lancet, May* 17, 1873, *p.* 698.

103.—ON THE MECHANISM OF THE PRODUCTION OF CERTAIN DISPLACEMENTS OF THE UTERUS.

By Dr. John Williams, Assistant Obstetric Physician to University College Hospital, London.

In a paper recently published in the Lancet, the limits of the changes occurring normally in the unimpregnated uterus were defined. In no one of those positions were any of the so-called uterine ligaments put on the stretch by reason of the position assumed by the uterus. The utero-sacral and utero-vesical ligaments were, however, put on the stretch when the rectum and the bladder were both distended. The stretched condition of the ligaments was not due to the position of the uterus, but to the distended state of the organs with which they are connected. Of course, when the rectum alone is distended, the utero-sacral ligaments only are stretched. It follows, then, that, whatever may be the use of the so-called uterine ligaments, it is not to limit the antero-posterior movements, ascent and descent, of the unimpregnated uterus.

Having premised this much, we are in a position to investigate the mode of production of abnormal positions of the uterus. In the present paper the mode of production of antero-posterior displacements—anteflexion, anteversion, retroflexion, and retroversion—will alone be considered.

A change in the position of any body can be produced by mechanical force only, and therefore the mode of production of displacements of the uterus is a question of mechanics.

The uterus is a semi-rigid tubular organ, having walls traversed by canals which anastomose freely, and which are filled with fluid. In what ways is it possible to produce a flexion of such an organ? A bend in an organ so constructed may be brought about in any one of the following ways:—(*a*) By a force acting at one end in the direction of the flexion, the other end being fixed; (*b*) by a force acting at each end, the middle being fixed; (*c*) by a force acting at a point situated between the two ends, the latter being fixed; (*d*) by gravitation, one end being fixed, when the organ has been lengthened without proportionate increase in the thickness of its walls, or when the canal has been enlarged, the thickness of the walls remaining the same, or when the thickness of the walls has been diminished, the canal remaining the same, or when the fluid contained in the canals anastomosing in the walls of the organ has been removed or greatly lessened, or when the consistence of the material forming the walls of the organ has been so changed as to become soft.

What are the mechanical forces which can act on the uterus? The force of gravitation is one which is quite sufficient to produce a flexion of the unimpregnated uterus in any condition of the organ which I have met with. As a cause of flexion, indeed, it may be utterly ignored, but as a cause of version it is the most prevalent.

The force generated by the distending rectum acts invariably on the under and posterior surface of the uterus in such manner as to raise and push the organ forwards.

The distending bladder produces a force which always acts on the anterior surface of the organ, and pushes backwards at the same time that it slightly elevates the uterus.

The force of the diaphragm communicated from above through the abdominal viscera may strike the fundus directly, or may strike the anterior or posterior surface of the fundus—depending on the position which the organ occupies at the time. This force is directed downwards and forwards in the direction of the continuation of the anterior surface of the spinal column; so that when the fundus is raised above or in front of the plane continuing the anterior surface of the spinal column the posterior surface of the fundus will be struck by the diaphragmatic force, and anteflexion becomes possible. If, however, the fundus of the uterus be below or behind the before-mentioned plane, the force of the diaphragm will act on its anterior surface, driving it further back into the hollow of the sacrum.

The force produced by the abdominal muscles is directed backwards, and can usually act on the anterior surface of the uterus only, pushing it back into the sacral hollow. When the uterus is elevated, however, the force produced by the action of the abdominal muscles strikes the superior surface of the fundus, driving the uterus lower into the pelvis.

The other forces which may act on the unimpregnated uterus are: the weight of the superincumbent viscera, the force of inertia in falls, jolts, &c.

Adhesion of the fundus to the peritoneum in front of or behind the uterus is not very uncommon. Occasionally the organ is closely bound down in a retroflexed position, but usually the bands forming the adhesions are of considerable length, and they allow of considerable movement of the fundus. I think it is very improbable that adhesions ever cause flexion; in most if not all cases where the uterus is closely bound down the adhesions and the malposition were simultaneous in their origin; the adhesions, however, served to maintain the malposition.

Anteflexion.—When the uterus lies in the pelvis below and behind a line drawn in continuation of the anterior surface of the spinal column, the only forces that can act on its posterior surface are the distending rectum and growing tumours. Growing tumours may cause anteflexion, but the distending rectum cannot. When, however, the uterus is elevated above or in front of the line drawn in continuation of the anterior surface of the vertebral column, other and most powerful forces may act on the posterior surface of the organ, and push the fundus forward. These forces are, the force of the diaphragm, the weight of the intestines, and inertia from falls, &c. When the rectum is full and the bladder is full the uterus occupies a higher position in the pelvis than in any other condition of these organs. It is raised above or in front of the imaginary continuation of the spinal column. In this position the fundus is free to move, and is quite unsupported anteriorly, for there is a space between the anterior wall of the uterus and the posterior wall of the distended bladder. The cervix, however, is fixed, and cannot be moved in any direction. This is brought about by several agencies: the rectum is distended, and resists any movement of the cervix in a posterior direction; the anterior wall of the vagina and the base of the bladder are stretched to their full tether and prevent any backward movement; the utero-vesical ligaments are stretched, and the connective tissue medium uniting the cervix of the uterus to the posterior angle of the bladder is put to the full stretch, so that the neck of the uterus is firmly bound to the bladder, and all movement is prevented. This is one position of the uterus in which anteflexion can take place; indeed, it is the only one, for in all other positions of the organ in which a force can be exerted on the posterior surface of the fundus of the uterus, so as to drive it forwards, the cervix is freely movable. A strain, or severe exertion, or a fall, when the uterus is in the above position, will produce anteflexion.

A fibrous tumour growing in the posterior wall of the uterus, involving the cervix and a great part of the posterior wall, but leaving a free portion near the fundus, may enlarge, fill the pelvis, elevate the uterus, and fix the lower part of it, so as to bring the organ into a condition in which anteflexion is possible.

Adhesions between the anterior wall of the uterus and the bladder are not very common. In the cases of adhesion which I have seen the connecting bands were of such length that it would have been impossible for them to have caused flexion. In one such case anteflexion existed. The adhesions were over an inch long, and probably came on from an attack of pelvic peritonitis after the flexion had occurred.

Anteversion.—There is a form of anteversion described, but said to be very rare, which is called "complete anteversion." I have never seen such malposition. There are two ways in which it is possible for it to occur. I pointed out in a former paper that when the bladder is empty and the rectum full the uterus is anteverted—the whole length of the organ lying on the collapsed bladder and the anterior wall of the vagina, the fundus being immediately behind the symphysis pubis. In the so-called complete anteversion the uterus lies on the bladder and anterior wall of the vagina, but the fundus is at some distance behind the symphysis. This may be congenital or acquired. In the former case the anterior wall of the vagina is naturally too long. In the second case the anterior wall of the vagina has been artificially elongated. Dr. Marion Sims relates a case in his "Uterine Surgery" which favours this view. He performed an operation in order to shorten the anterior wall of the vagina, and the malposition was cured. The acquired elongation of the anterior wall may possibly occur after pregnancy, but more probably from great exertion when the uterus is already anteverted—that is, when the bladder is empty and the rectum is full.

Retroflexion and Retroversion.—When the uterus occupies a position below or behind the plane which would form a continuation of the anterior surface of the spinal column, the action of the diaphragm, of the abdominal muscles, weight of viscera, inertia from falls, &c., is to push the fundus backwards and downwards. When the rectum is empty the uterus occupies such a position; the bladder may be full or empty. The condition of the rectum then, is the important matter in the consideration of retro displacements. The rectum may be empty under a variety of circumstances. The bowels may be regular, and then the rectum is occasionally full and occasionally empty. When the rectum is empty the uterus is somewhat retroverted. Any sudden exertion, strain, or fall under these circumstances will increase the retroversion, or may even produce retroflexion. But the bowels may be constipated—that is, they may be open every three or four days,—and there is much straining at stool; and what is passed consists of a few small hard fæcal masses. The condition of the rectum in these cases is one of comparative emptiness; it is not at all distended; indeed it contains only a few small, hard masses of fæces lodged in its folds, and great force is necessary to dislodge them. (This is the usual condition of the rectum in patients suffering from constipation and passing such stools as described; at the same time there are cases of constipation in which the rectum is much distended, but the state of the stools in these cases is totally different from the cases stated above. Of cases of constipation, some of the most obstinate are those in which the rectum is immensely distended.)

The physiological actions performed at a healthy stool (the rectum and bladder are always full) are, first of all, evacuation of the bladder; as the bladder collapses the uterus becomes anteverted; the rectum is then emptied, and in consequence, the bladder and the uterus sink somewhat lower in the pelvis; and as soon, probably, as the woman begins to walk the uterus falls to its lowest normal position—the cervix comes to rest on the top of the inclined plane of the vagina. When, however, there is constipation with much straining of stool this does not happen. The rectum is from the first comparatively empty, the bladder is full and the uterus is somewhat retroverted. The first physiological act performed at stool is evacuation of the bladder. As the rectum is almost empty the uterus does not become anteverted as the bladder collapses, as it does in health. It was from the first somewhat retroverted, and it only descends slightly by the evacuation of the bladder: it still remains retroverted. Having emptied the bladder, the person proceeds to empty the rectum, which is by no means easy. She strains, and the uterus becomes still more retroverted. She strains again until a few small, hard masses are passed. The uterus by this means is driven still lower, especially the fundus. The uterus lies now with the fundus directed backwards and somewhat downwards; the cervix is directed forwards and slightly upwards; and the uterus, at the junction of its neck and body, rests on the posterior wall of the vagina, so that the organ cannot descend lower than as a whole. Here we have the middle point of the uterus fixed, with both extremities movable; for the fundus may descend until it reaches the bottom of Douglas's pouch, and the cervix may descend, or it may ascend until the anterior vaginal wall is fully stretched. This position of the uterus is one of great retroversion.

During the act of straining (the uterus having been driven into the above position) the mass of the intestine acted upon in front and above by the diaphragm and the abdominal muscles is driven into the pelvis, and acts like a wedge. One of two things may happen. Should the anterior wall of the vagina be sufficiently long to allow the fundus of the uterus to be pushed to the bottom of Douglas's pouch, that may take place. The fundus of the uterus by this means becomes fixed, and the middle point is also fixed; any further straining will bend the cervix downwards into the vagina, and retroflexion is the consequence. But the anterior wall of the vagina is, as a rule, not so long as to allow the cervix to ascend to such a degree as to permit the fundus to descend and become fixed in Douglas's pouch. In this case, as soon as the anterior wall of the vagina has been fully stretched, the cervix becomes a fixed point, the fundus is by further straining driven lower down into Douglas's pouch, and retroflexion is the result. The fundus of the uterus with its broad ligaments having been driven into Douglas's pouch, the veins of the latter become constricted by the utero-sacral ligaments, which form the upper margin of the pouch, and in this manner is caused the congestion of the body of the uterus so commonly associated with retroflexion of the organ. In cases of simple anteflexion of the most acute kind occurring in virgins, the enlargement of the body of the uterus is not marked. In women who have borne children, however, there is often considerable enlargement; but this enlargement is due to complications accompanying the flexion.

In many cases of retroflexion in which there is difficulty in replacing the body of the retroflexed uterus, the cause of the obstruction is to be looked for, not in adhesions, but in the state of the uterus itself and of the utero-sacral ligaments. The fundus and part of the broad ligaments in these cases have been for a long time lodged in Douglas's pouch; the utero-sacral ligaments compress the broad ligaments and the lower part of the body of the uterus; the portions of the broad ligaments immediately adjacent to the uterus and the uterine body itself are mechanically congested and swollen, so as really to be too large to pass up between the somewhat stretched sacro-uterine ligaments. If in some of these cases the cervix be pushed back, the utero-sacral ligaments are somewhat relaxed, and the fundus of the uterus can be pushed up between them, and the displacement reduced.

Diminution in the amount of blood in the vessels of the uterus is little, if at all, known, though congestion—a condition most unfavourable to the production of flexion—is said to be not uncommon. Probably those curious cases of flexion which are sometimes directed forwards and sometimes backwards are due to a *deficiency of the circulating* fluid in the uterine vessels. This view is favoured by the fact that such cases occur almost invariably in very anæmic women.

Softening of the tissue of the uterus is a condition of the organ, the pathology of which has yet to be elucidated. I have seen it only in cases of pregnancy, some cases of fibrous tumours, and some cases where the uterus had contained foreign bodies. This would point to its being due to rapid growth or to rapid change of tissue. I have never seen a uterus whose tissues were so soft that it could be said that the soft condition was capable of causing flexion; nor have I seen a flexed uterus so soft that the flexion could in any way be ascribed to that condition.

Anteflexion of the uterus, then, is possible only when the bladder and rectum are both full enough to raise the fundus of the uterus above and in front of an imaginary plane continuous with the anterior surface of the spinal column.

Anteversion is only possible when the rectum is full and the bladder empty, except when the uterus is considerably elongated, in which case it is the permanent condition. The so-called "complete anteversion" is only possible when the anterior wall of the vagina is of unusual length.

Retroversion is only a step towards retroflexion. Both conditions may be produced when the rectum is empty, and that is a necessary condition for their production.

Elongation of the uterus, enlargement of the uterine canal, and diminution of the thickness of the uterine walls, are questions which can be discussed only in connection with pregnancy and fibroid tumours.

On the influence of the above conditions in the production of displacements, as well as on the pathology of flexions generally, I hope to publish on a future occasion.—*Lancet, Aug.* 30, 1873, *p.* 298.

104.—ON FIBROID TUMOURS OF THE UTERUS.

By Dr. Alfred Meadows, Physician-Accoucheur to St. Mary's Hospital, and Physician to the Hospital for Women.

These tumours are not mere hypertrophies but distinct growths. In simple hypertrophy there is, as you know, only an exaggeration of the normal tissue of the part, there is no foreign or adventitious element or structure; and I have been surprised to find in the writings of some authorities that all these uterine fibroids are regarded as made up only of the proper tissues of the uterus, variously combined. This, however, is not the case; in most if not in all of them, there are besides the ordinary elements of the uterus—namely, muscular fibre-cells in varying proportion, all bound together with firm connective tissue—a considerable proportion of cellular elements; in some places little else is observable, and the whole appearance there is very much the reverse of what one would be led to expect in a fibrous tumour. I have noticed also that the proportion of these cellular elements varies directly with the rate of growth in the tumour—that is to say, that the more rapidly a tumour has grown, the less fibrous and the more cellular will it be found to be. This point is well seen in some of the specimens on the table, and their clinical histories bear out this view.

In number and size these tumours vary very greatly; sometimes only one is found, but this, according to my experience, is rather the exception than the rule. In one case I have seen the uterus completely studded with them, both within and without, so that on making a section of the uterine wall the surface looked almost like the skin of a person suffering from small-pox; so numerous were the tumours that it was literally impossible to count them. They varied in size from that of a millet-seed, or even less, to that of a small walnut. I need not say how hopeless such a case would be from a therapeutical point of view, and how utterly impossible to diagnose it. A careful inspection of such a specimen as the one I show you here suggests the inquiry—How do these tumours originate? That in some way or other they are a consequence of mal-nutrition, must, I think, be conceded. There seems no other way of accounting for them in the case to which I have referred just now, for where the disease is so general no other explanation meets the case. At each one of the innumerable points where the tumour exists in that specimen some departure from the ordinary cycle of nutritive changes must have been in operation. I think, too, we may go a step farther, and say that the change in question was once characterised rather by deficiency than excess of formative power; that, in fact, it is a degenerative process, and not a process of exaltation. The whole history of these growths supports this hypothesis, and the way in which they sometimes terminate, by decay and absorption, by calcareous degeneration, or by abscess, all these point to the same conclusion. Especially is this the case, I think, in the second of these processes, for the change in question consists "in an amorphous and disorderly deposit of the salts of lime and other bases, in combination with, or in the place of, the fibrous tissue." (Paget.)

It may be thought, at first sight, that the clinical history of uterine fibroids tends rather to contradict this statement, because there is no doubt that these tumours occur much more frequently at those periods of life when the uterine functions are in greatest activity. In this respect my experience coincides pretty closely with that of most other observers. For instance, of the 180 cases which have come under my care in the past seven years, 6 occurred between the ages of twenty and twenty-five, 14 from twenty-five to thirty, 42 from thirty to thirty-five, 33 from thirty-five to forty, 41 from forty to forty-five, 33 from forty-five to fifty, 9 from fifty to fifty-five, and 2 from fifty-five to sixty-five. Or to put them in decades, we find that 20 only occurred between the ages of twenty and thirty, 75 between thirty and forty, 74 between forty and fifty, and only 10 between fifty and sixty. Of course, the time at which these cases came under observation must not be taken as the date of occurrence of the growth; that must be put back at least for several years. But this only brings the fact of the origin of the tumour still more closely into relation with the period of greatest uterine activity. For instance, allowing for a period of five years as that within which the tumour was growing from its very commencement as a mere speck, no larger than a pin's head, to the time when it first came under observation, the greatest number of cases (149 out of 180) must by this reckoning have originated between the ages of twenty-five and forty-five—a period which is universally admitted to include the highest functional activity. How, then, it may be asked, is this to be reconciled with the idea that these growths are the result of some form of mal-nutrition, and that the change in the formative power of the part effected is characterised rather by deficiency than excess?

A little reflection will, I think, satisfy us that the facts and the hypothesis are quite compatible; nay rather, that the latter *best* accords with the former. Let us look a little more closely into the facts. Of the total number of cases (180), 152 were married, 25 were single, and in 3 no note of the social condition in this respect is stated. Of the 152 who were married, 45 were sterile, 94 fertile. Of the latter 46 aborted, the total abortions being 105. The number of children born was 454, and the total number of pregnancies was 559, giving an average of very nearly 6 pregnancies to each. Further, I find of the 152 who were married, the number of years of married life is stated in 128, and the total number of years amounts to 1937, giving an average of 15 years and a fraction to each. All these facts are, I think, sufficient to prove that the period of greatest functional activity in the organ concerned is that within which the occurrence of fibroid tumours is most likely to happen. But functional activity—in other words, active nutritional change—implies corresponding waste, disintegration, decay; the two are commensurate one with the other, and it might, I suppose, be formulated as an axiom in pathology that excluding blood poisons, such as those of gout, syphilis, the poisons of eruptive and other fevers, all of which have special affinities for particular tissues, parts which are subject to the greatest functional activity, and where the nutritive changes are consequently *most actively* carried on, are just those which are *especially liable to disease.* The greater the activity the greater is the waste, and the greater the waste the greater is the liability to disease. The uterus during procreative life is surely no exception to this rule; it is, in fact, the very basis of our practice, without which, I fear, our office of gynæcologists would be in danger of becoming a sinecure. There is no fear of that at present at any rate, as the experience of this hospital testifies. But, further, if you will look with your mental eye into the microscopical changes which we may suppose to be taking place in the minute spot where the germs, if I may so say, of a uterine fibroid tumor are being developed, you will see, I think, indications of the kind of action I have pointed out. Suppose that, instead of this deficiency in the formative power of the local nutritional changes, there was an excess, surely what we should have would be, rapid growth, great pain, and a tumour characterised rather by softness than hardness. Such, at least, is the universal result in cases where excessive formative power is certainly going on, as is seen, for instance, in the case of certain forms of cancerous growths.

You must not suppose that all this is mere fanciful speculation with no practical interest or bearing. On the contrary, it is related in a very special manner to the treatment of these cases, at least in its medical aspect; and certainly no treatment can be successful which is not based upon sound pathological data. One illustration will suffice in proof of this statement, for I need only point to the difference in treatment between a case of uterine fibroid and one in cancer to demonstrate my meaning. In one word, local stimulation is the process upon which we shall mainly rely in the medical management of a case of fibroid tumour, where a cure is aimed at without the intervention of surgery, while all thought of such a process is most carefully excluded in the conduct of a case of cancer. Why? Because in the one case there is already an excess of formative activity which we are most anxious to curtail, while in the other an opposite condition exists, and local stimulation, with the slender hope of securing a removal of the disease by a process of absorption, is the key-note of our therapeutical system.

So much, then, on the pathology of these tumours. There are many other points in connexion with this branch of our subject upon which I should have been glad to remark had time permitted; some of these I shall perhaps allude to in considering the question of treatment. Before proceeding to discuss the *Symptoms*, a few words are necessary in regard to the classification of these growths which has been adopted by writers on the subject. The point is important, too, with reference especially to surgical measures, and hence the diagnosis of the several varieties is a matter of prime consideration. I do not propose, nor, indeed, is it necessary for me, to dwell at any length upon this part of our subject; suffice it to say, that for all practical purposes fibroid tumours of the uterus may be arranged in three classes, according to their situation and relation to the uterine walls. Thus we have, first, the *submucous* group, in which the tumour is originally developed immediately under the mucous surface. As these grow, they project more and more towards the uterine cavity, which they invade and distort to a greater or less extent, according to the size of the tumour. At first, and when extremely small, they resemble very much another variety—viz., the interstitial, and they differ from them only in being nearer to the uterine cavity. Sometimes it is thought that, by the active efforts of the muscular fibres behind or beneath them, they are gradually projected from the bed in which they were originally developed; and as this process goes on, the contraction of the fibres behind gradually extrudes the growth, narrows its point of attachment, so that ultimately it becomes polypoidal in form, having a more or less slender stalk, and being covered all over by the uterine mucous membrane. In regard to this submucous group, the chief variations—and they are of great importance from a surgical point of view—have reference to their size, to the amount of their projection into the uterine cavity, and to the extent of their attachment.

Another variety of uterine fibroid is that in which the original development occurs, not near the mucous, but the peritoneal surface—the *subperitoneal* group. These are commonly believed—though upon what grounds I do not know, and it certainly does not agree with my experience—to be of more frequent occurrence than the former group; they give rise to very distinct symptoms, which usually mark them off very clearly from those we have just considered. In their growth it is thought that very much the same kind of action goes on with reference to the muscular fibres behind or beneath them as occurs in the case of the submucous group—that is to say, that a process of extrusion takes place by muscular effort, formed in both cases by the deficient resistance which exists upon either surface; so that here again the point of attachment may become gradually narrowed, and the tumour at last assume more or less of a pedunculate character. In a few rare cases, indeed, this process has been carried to such an extent that the tumour has at length become detached altogether, and, still maintaining its independent existence, has for a time lived free in the peritoneal cavity, and ultimately, in some cases, has taken root again, and grown in another part. Such a medical curiosity has, however, never fallen within my experience.

The third group of cases comprises those which belong to neither of the two classes just considered, but which lie as it were in the intervening territory, having originated in the substance of the uterine wall, not appreciably nearer to the one surface or the other. To these the term "interstitial," or intramural, fibroid has been given. This comprises a very large group of cases, and, according to my experience, they on the average attain to a greater size than either of the other two; though, of course, there are exceptions to this. The greater freedom of blood-supply is probably a sufficient explanation of the fact. In the growth of these tumours they may incline a little more to one or other surface of the uterus, and in proportion as this occurs, so will the uterine cavity be more or less influenced, distorted, or enlarged—a point which obviously is important in a diagnostic point of view, and which is happily within our cognizance.

In reference to all these several varieties, it may be said that they occur much more frequently in the fundus and body of the uterus than in the cervix; indeed, it is very rarely that we meet with them in this latter situation, unless they have invaded that part in their growth downwards from the body. On the whole, I believe that the fundus is most frequently the seat of these growths, though I am unable to give you numerical data. In regard to their relative frequency in the anterior and posterior walls, I find that of 82 in which its position could be accurately defined, the tumour was situate in or upon the anterior wall 44 times, and in the posterior wall 38 times. In 22 other cases, it was situate five times on the right side, five times on the left, once only in the cervix; and in 11 cases it was distinctly subperitoneal.

Treatment.—Before resorting to any plan of treatment, it is of course, above all things, necessary to determine, if possible, the exact position of the tumour; and, in reference to this point, you will bear in mind what I stated in my last lecture as being, on the whole, fairly deducible from a consideration of the leading symptoms—viz., that excessive hemorrhage, or other irregular discharge, is indicative of a polypoid, submucous, or interstitial fibroid, while great pain, on the other hand, results much more frequently from tumours which are subperitoneal, or which, if interstitial, are so placed as to cause obstruction to the menstrual flow or pressure on adjoining sensitive parts. As, however, the fitness of the case for medical or surgical treatment depends entirely, at least so far as our advice is concerned, upon a correct appreciation of the exact position of the tumour and its relation to the uterine walls, it is desirable that I should consider a little more fully the means at our command for determining this point. Of course if a case comes before us in which excessive menorrhagia, irregular attacks of hemorrhage, and profuse watery discharges are the leading symptoms, so that the patient has been reduced to a marked anæmic condition, and we find on examination that the uterus is occupied by a tumour, that the cervix is expanded over it equally all round, that its tissue is soft, the os a little patulous, its edges thin, and through it, though the opening be very small so as hardly to admit the tip of the finger, a tumour is felt projecting into the uterine cavity, no doubt whatever exists as to what ought to be done in such a case. Medicines, even of the palliative kind, are here quite out of place, and the case is a perfect one for surgical interference by one or other of the methods I shall presently describe. Our advice should be given accordingly, and in no faltering tone.

Again, suppose a case presents itself similar to the foregoing as regards the general symptoms, with perhaps this difference, that, though menorrhagia is excessive, there is no history of irregular attacks of hemorrhage, and no profuse watery discharges, but that there is rather more pain in this than in the former case, and more disturbance of the functions of the neighbouring viscera—the bladder or rectum. The inference before vaginal examination will tend rather to the opinion that an interstitial fibroid exists. And if, on examining the uterus, we find its tissue hard, the cervix displaced from its central position in the pelvis, the os closed, or at least not patulous, and that a hard swelling exists in the pelvis, immediately connected with the uterus and opposite to the position of the cervix, most likely in the anterior or posterior wall of the uterus; if, further, on cautiously introducing the sound, we find that the uterine cavity is enlarged in all directions, that the sound seems to pass over a rounded surface, and that the slightest movement of the tumour at once affects the sound in utero, then there can be little doubt that we have to deal with a fibroid tumour of the interstitial kind, and the question of medical *versus* surgical treatment becomes one of pressing importance, and is at the same time full of great intricacy, difficulty, and danger. Such a case requires unquestionably the greatest skill, judgment, and discretion. It should be examined again and again before a definite course of action is decided on, at least if surgical interference is contemplated. And here I would urge very strongly the great importance of careful examination under chloroform before any surgical measures are attempted in any doubtful case. I do not think it is possible to gain a correct idea of the relations of an interstitial fibroid unless the patient is completely under the influence of chloroform, so that by combined internal and external examination, with and without the use of the sound, the whole position of the tumour can be mapped out, as in most cases it can with almost absolute certainty and accuracy. When this is done, then and not until then shall we be in a position fairly to decide for our patient's welfare as to the course that should be adopted. I shall presently describe the surgical bearings of such a case, and the character of the operation suited to it.

Lastly, if a case occurs in which the leading symptom is pain in and about the pelvic region, with accompanying disturbance of the functions of the adjoining viscera—the bladder or rectum; with a certain but comparatively slight excess in the menstrual discharge; with no hemorrhage at irregular times, and no amount of watery discharge; if, on examining such a case, we find much displacement of the cervix uteri, with but little alteration of its size, shape, or consistence, and that a tumour exists somewhere in the opposite direction to the cervix, causing its displacement; if, further, we find that the tumour is very mobile, and does not affect the cervix to the same extent as does an interstitial fibroid, showing that its connexion with the uterus is not so intimate in the one case as in the other; if, moreover, the uterine sound gives a measurement showing that the uterus is little if at all increased in size,—then the tumour, if uterine at all, is most probably of the subperitoneal form.

Having, then, determined accurately the position of the tumour, the question of its medical or surgical treatment is thereby settled; and, supposing that palliative treatment is required, our attention will probably be directed chiefly to the relief of one or other or all of the three leading symptoms—namely, menorrhagia, irregular discharge of mucus or of blood, and pain. Of these, undoubtedly, excessive discharge, either at or between the menstrual periods, will be the most common, and probably the most trying and troublesome symptom to treat. Of course I need not say that astringents will under such circumstances be called for; and, among the many that at different times enjoy popularity, it is difficult to select one which shall prove most effectual; indeed, I do not hesitate to declare that there is no single remedy that I know of which can be relied upon with anything like certainty to check the hemorrhage or arrest the menstrual flow in cases of this sort. I have tried, in turn, most, if not all, the so-called hæmostatics, and I cannot say of any one of them that it has been uniformly successful, or that it has not at times most signally failed. Gallic acid in one case, astringent chalybeates in another—and, of the latter, I know of none that is so effective as the ethereal peracetate of iron in half-drachm doses—acetate of lead in a third, taking care always, in giving the latter, to do so in solution with the dilute acetic acid; ergot of rye in a fourth, in half-drachm doses, giving this in the recent powder in preference to any other form;—these are, according to my experience, among the most efficient remedies of this class. But there is one that I have used during the last twelvemonth which certainly seems to me by far the most effective remedy of the kind that we possess: I mean the watery extract or liquor of the common periwinkle—the extractum vincæ major liquidum, as it is called. This, taken in drachm doses, properly diluted every four hours, I have seldom known to fail; it is certainly by far the most efficient remedy that I know of for the purpose in question. Why one remedy answers at one time or in one case better than another I am quite unable to say; constitutional or local peculiarity may have to do with it; but in any case I can lay down no rule for your guidance, for the whole thing seems to be purely empirical. Of those agents which, while possessing no astringency, have nevertheless in some hands proved effective as hæmostatics, such as digitalis, Indian hemp, turpentine, and mercury, I have no satisfactory opinion to offer, for the reason that they have all in my hands proved utter failures. Of course, where great anæmia exists, a chalybeate astringent would seem the most fitting, but I have on many occasions found it rather increase than diminish the bleeding. The same may be said also of ergot. Dr. M'Clintock speaks very highly of the hæmostatic properties of mercury, confirming in this respect the experience of Dr. Tanner, who found it a most useful agent for this purpose. I must own that I have never seen such a result. If I were to place the medicines in the order in which I have found them of greatest value, I should assign them as follows: the liquid extract of the common garden periwinkle, the ethereal peracetate of iron, ergot in the form presently to be described, gallic and sulphuric acids with the compound infusion of roses, the acetate of lead with dilute acetic acid, and, in a few rare cases of the congestive kind, the local abstraction of blood by means of leeches to the cervix; these have all, in turns, proved efficient in my hands. As a general rule, I have observed that when hemorrhage does occur, which is very seldom, in cases of subperitoneal fibroids, it is more easily arrested than in either of the other varieties, and for such the periwinkle, or the chloride of calcium, given perseveringly for months, has been the most successful. In interstitial fibroids the bleeding is more difficult to control, and ergot of rye or borax has proved the most efficient agent in such cases. In the submucous, and still more in the polypoidal forms, the difficulty of arresting hemorrhage is greater than in either of the others; here the vinca major and the more common astringents, such as gallic acid, sulphuric acid, iron, alum, or the peracetate of iron, have appeared to me to answer best.

There is one other remedy which I have tried quite recently in three cases, and the result in one of these was very marked; I allude to the subcutaneous injection of ergotine. The solution which I employed contained ten grains of ergotine to a drachm, and from five to twenty minims were injected each day. In one case severe cellular inflammation followed in the site of the puncture, so that the treatment had to be discontinued; in another it excited so much pain and distress about the præcordia that I was obliged to desist, and in neither did any good result; while in a third the remedy was continued daily for ten days, and not only did it produce a decidedly beneficial result in regard to the next menstrual period, but the hemorrhage was restrained for two months afterwards, when the case passed from my observation. I believe that we have in this agent a powerful hæmostatic remedy, for I am informed that very good results have followed its practice in other hands.

In addition to the above, the local application of astringents will often prove most successful, and these we may apply either in the shape of vaginal suppositories of tannin, or the glycerine of tannin, or matico, or alum, or we may inject the uterus with perchloride of iron, though I very much prefer the use of the anhydrous zinc points as suggested by Dr. Braxton Hicks. In the more severe cases, where a fatal issue seems almost to threaten, it will be necessary to resort to the use of the tampon.

Besides all these remedies there is a measure which has been greatly extolled by some as most efficient in arresting hemorrhage—viz., the division of the cervix. Various *explanations have been* given to account for this result, but none of them, I think, are satisfactory, nor has the proceeding itself proved successful in my hands. Occasionally, however, where the cervix has been dilated with sponge tents, and some inflammation has followed in consequence, the disposition to hemorrhage has been afterwards completely arrested. It seems likely, therefore, that the good result which occasionally flows from dividing the cervix is to be explained in the same way. A similar method to this has been recommended by Dr. Atlee, of Philadelphia, which he describes as most successful. His plan is "to pass a bistoury along the vagina into the cavity of the uterus, and make a very free incision into the most exposed portion of the tumour." The operation is to be done during the occurrence of hemorrhage, and he declares that it is "invariably arrested instantaneously." I have had no experience of this method, but anything coming with the approval of Dr. Atlee is deserving of every attention. So much, then, for the question of hemorrhage, or excessive discharge of any kind, in cases of uterine fibroid.

In regard to the relief of pain, there are several ways open to us by which we may hope to accomplish this end; and we shall be probably guided to the right method according to the correctness of our appreciation of its causes. Sometimes the pain is produced mechanically, by pressure of the tumour on neighbouring parts. This is especially likely to occur in the subperitoneal and pedunculate variety; and in such cases great relief has occasionally followed the removal of the tumour by the hand from one part to another. In one case that came under my care where the tumour was situate posteriorly, the introduction of an air-pessary behind the cervix proved very successful in supporting the tumour, and gave great relief to the patient. In a movable tumour, therefore, the possibility of a mechanical cause of pain should be borne in mind. The pain which is due to a congested or inflamed condition of the tumour must be met by the remedies already described when treating of this condition. And in cases where neither of these two causes is in operation, but where the pain is, nevertheless, constant and wearing, though its cause may not be very clearly defined, there we must resort to the employment of anodynes; and these we may use either subcutaneously or by the mouth, or better still, I think, by the vagina. I will only speak of the latter mode of treatment, because, of course, the two former are well understood. The advantages of this method are—first, that the anodyne is applied directly to the affected nerves; and, secondly, that the stomach and digestive organs are not so injuriously affected as when the same drug is given by the mouth. On the other hand, the disadvantages are—first, what I may call the moral or sentimental objection to the practice of daily introducing a foreign substance into the vagina; and, secondly, the discharge which must necessarily take place of those portions of the pessary which are not absorbed. This latter objection formerly applied with much greater force when the greasy substance—cocoa-butter—was used, than it does now, because, as I showed long ago, in a paper which appeared in the Practitioner, "On the Use of Medicated Vaginal Pessaries," it is no part of the function of the vagina to digest fats; and the presence of such a greasy substance in the vagina tends to prevent the absorption of the active ingredients. Moreover, the discharge of all the unabsorbable part, including the whole basis of the pessary, is, in private practice, very objectionable. I have, therefore, long since discarded the use of the cocoa-butter, at least in all cases where an astringent is not required; and in its place I now use, as the basis of the pessary, a mixture of gelatine and glycerine, in the proportion of one part of the former to four of the latter. This makes an admirable mass; it readily melts at the temperature of the vagina, and any ingredient that is not astringent can be added to it; but as all astringents combine with the gelatine to form an insoluble compound, it cannot be used in these cases. There is one other point I may mention in connection with these pessaries—namely, their size. Very commonly they are used as large as a Minie ball, or even larger. There is no necessity for this, and I think it is on every account desirable that they should be as small as possible. I therefore always use them the size of a suppository, which is quite large enough to carry any drug you may require. Another advantage of using the gelatine and glycerine is, that the latter promotes the secretion of the vaginal mucous *membrane*, and so favours the absorption of the active ingredient. The anodynes *which* I have found most useful are the alkaloids coneia, atropia, and morphia—one to two grains of coneia, one-eighteenth to one-twelfth of a grain of atropia, and half a grain to a grain of morphia. These may be used once or twice a day, according to circumstances. On the whole I am inclined to believe that coneia is by far the best and most efficient anodyne we possess for vaginal purposes. I am satisfied that it is in all ovarian cases, but am not quite so sure of it in the class of cases we are considering.

So much, then, for what we must call only the palliative treatment of these tumours. Happily we are not restricted to this, for we possess many means by which we can effectually remove these growths. The mode of procedure to be adopted in any given case must of course depend upon the nature of the case, and especially upon the situation of the tumour. Hence the great importance of being able to diagnose accurately the position of the tumour, and its relation to the uterine walls. In the polypoid and in the submucous varieties our course of action is clearly marked out; in both cases the first step is to secure free dilatation of the cervix, so as at least to admit easily two fingers within the uterine cavity, in a case where the tumour is small, and has a distinct pedicle; while in others where the tumour is large, and has an extensive attachment, though only to the mucous surface, there we must dilate the cervix more freely by means first of the sponge or sea-tangle tents, and afterwards with the india-rubber air- or water-bags, so as to admit not merely two or three fingers, but even in some cases the entire hand. I have succeeded in doing this on several occasions, detaching thereby tumours considerably larger than a fœtal head at full term, and separating them completely from the uterus by means of the écraseur. In one case, after cutting off such a tumour I had the greatest difficulty in extracting it from the uterus, and was obliged to use a pair of long obstetric forceps for this purpose. We shall generally find, at least I have hitherto found, little or no difficulty in separating a submucous tumour from its attachment; as a rule, one or more of the fingers is quite sufficient for this purpose, except at one part where it will be found to have a firmer attachment than elsewhere, and there the écraseur must be used. The old-fashioned canula of Gooch is now, I think, pretty generally abandoned, and deservedly so, for there is nothing to be said in its favour, and very much may be fairly urged against it. It is, I think, most objectionable practice to retain, as with the use of this instrument we are obliged often to do, a sloughing putrid mass in the vagina, or even in the uterus, sometimes for days together, until the ligature, which is here used for the purpose of strangulation, has succeeded in cutting its way through the tumour. I am in the habit always of using, and greatly prefer, the single wire écraseur. The wire in question is the same as is used in pianofortes; it is extremely tough, but is most conveniently stiff and at the same time springy, so that the loop may be nipped tightly and narrowed for facility of passage through the vagina and os uteri, but as soon as it gets beyond this into the cavity of the uterus it springs open so as to encircle even a large tumour, and its stiffness makes it very easy of application because it can be passed, of course with all due care and gentleness, along the uterine wall, even without the guide of the finger, if the operator is accustomed to such manipulations, and so up to the stalk or base of the tumour. While this is being done, and also when the screw is being worked so as to fix the wire round the tumour, the instrument should be constantly semi-rotated, so as to free the wire from the uterus, at the same time that the point is pressed home as far as it will go. Occasionally when there is much difficulty in passing the wire up to the fundus, I have used a small hook as a carrier of the wire to the point sought. The application of the wire is, I think, easier if we use a new wire for every operation; it is inexpensive, and it is much easier to use than when the wire has before been drawn through the instrument, and so spoilt its round shape and springiness. I usually cut through the tumour quickly. There is seldom or never any bleeding of consequence; if there be, I plug with cotton wool and tincture of matico and a little carbolic acid.

With reference to the subperitoneal fibroids, I can conceive no justification for surgical interference, except it be in cases where the suffering caused by the tumour is so great, or the condition so alarming, that life is either insupportable from pain and general distress or is threatened by the growth of the tumour. Even in these cases there ought to be at least a fair presumption that the tumour is removable by the operation of gastrotomy. But while I would deprecate any hasty resort to operative interference in these cases, I must at the same time express again my opinion, as I have done on more than one occasion in writing, that "many of these cases might be attacked which are now left alone to the great distress of the patient; and I cannot help thinking that, with our rapidly diminishing mortality after ovariotomy, due in great measure to the knowledge we are gaining from daily accumulating experience, the time will come, and is fast approaching, when we shall be justified in attempting the removal of many of these growths by abdominal section." I regret that I have not more time to discuss this question, but the subject has been fairly and ably handled by Professor Thomas, of New York, in the last edition of his admirable "Treatise on the Diseases of Women."

Between the two varieties of tumour the treatment of which we have considered—viz., the submucous and the subperitoneal—there is a large group, indeed, as I believe, by far the largest group, of cases—namely, the interstitial; and it is to these that the attention of the profession will, I am sure, be more and more directed in future. Since I began to attack these growths by the process of enucleation I have been surprised often at the facility with which they may sometimes be removed, and the comparatively slight constitutional disturbance which their removal has involved. At the same time I fully recognise the danger that may be run, and the serious and even fatal consequences that may ensue, unless, on the one hand we are very precise and clear in our diagnosis of the exact position of the tumour, and in its relation to the uterine walls, especially on its peritoneal aspect; and, on the other hand, are extremely careful, and, as far as possible, gentle in the somewhat rough handling to which the uterus must necessarily be subjected in the process of enucleation. Hitherto the results in my hands have proved so successful that I do not hesitate to recommend its more frequent adoption, even though so great an authority as Dr. M'Clintock expresses an adverse opinion upon it. It was, indeed, to a great extent due to the strong objections urged by him and other writers that I was for a long time deterred from resorting to this operation. Dr. M'Clintock's opinion is thus expressed: "The results of this operation are very far from encouraging, and it may be doubted whether it will ever become an established mode of practice. It is attended by grave dangers of an immediate and remote kind—viz., hemorrhage and pyæmia. The idea of the operation suggested itself to Velpeau from observing the looseness of attachment between some of these tumours and the uterus. But in my experience it is quite exceptional to find the connexions of the tumour with the uterus so lax as to admit of its ready enucleation or dislodgment; while in most cases very great and direct force would have been required to effect this; and in some instances, as Dr. Rigby remarks, the tumour seems to be almost *continuous* with the uterine substance, so that its detachment would be next to an impossibility. However," he adds, "this operation should not be wholly proscribed." Dr. West, again, expresses himself still more strongly to the same effect. He states "that twenty-seven operations yield fourteen deaths to thirteen recoveries, while in four of the latter the operation was incomplete, and a portion of the tumour was left behind." Later on he expresses the opinion that, if we could get correct statistics of the operation, it would be shown that most of the cases have ended fatally; and, accordingly, he classes the operation "among the most hazardous in surgery," and concludes by saying that the "hazard attendant on operations for the enucleation of interstitial fibrous tumours of the uterus, when still imbedded in the walls of the organ, is so extreme as to render them generally inadmissible, and to remove them to that class of exceptional proceedings which the special condition of the patient and the unusual dexterity of the surgeon alone justify." Now, I cannot agree with either of these two authorities, for I am sure from my own experience that they have greatly exaggerated the dangers that are incurred, if only due care is taken in the selection of cases suitable for the operation. You can readily understand that an interstitial fibroid, not perhaps of very great size, may yet be so placed that only *the very thinnest* layer of uterine tissue covers it on its peritoneal side; and, *of course*, in such a case very great danger will be run in attempting its enucleation. On the other hand, you may have a fibroid equally interstitial, of large or small size, completely covered with uterine tissue, but with a good thick layer upon its peritoneal surface, and a comparatively thin layer on its mucous surface. Obviously the danger in these two cases is widely different, and in the latter it is comparatively slight. How, then, is the differential diagnosis of these two conditions to be made, at least with some approach to accuracy? For, of course, accuracy in such a matter is of the very utmost importance. I have already considered this subject in part when speaking of the differential diagnosis of the several varieties of uterine fibroids. It must be judged of partly by symptoms and partly by physical signs, and, as a general rule, it will be found that the more subperitoneal a tumour is, the less will the size and form of the uterine cavity be interfered with; while on the other hand, the more submucous it is, the more will those relations be distorted; so that where the hemorrhage or other uterine discharge is very free, and where the size of the uterine cavity, as measured by the uterine sound, bears a fair proportion to the size of the tumour, there we may reasonably expect to find a fair amount of uterine tissue on the peritoneal surface of the tumour, a thinner layer on its mucous surface, and, consequently, the case is a favourable one for enucleation. I should add, too, that the lower the tumour is in the pelvis, the more the body of the uterus is involved; and the nearer the tumour is to the cervix, still more if the cervix itself is invaded by it, the greater are the chances of successful interference. A long cervix, especially if it be little or not at all altered from its natural size and shape, must be regarded as unfavourable for surgical treatment in cases of uterine fibroids.

I regret that I have not more time for the consideration of this part of our subject; but I have left myself only a few minutes for a description of the operation, and cannot, I fear, detail the cases I hoped to bring before you where I have resorted to this proceeding. Now the operation of enucleation should be conducted upon a definite plan, and according to some fixed principle, and both may, I think, be summed up as follows:—First, that these uterine tumours should be regarded as essentially foreign bodies. Secondly, that nature's method of dealing with these foreign bodies is to expel them; we see this often in the case of polypi, and occasionally even in large submucous fibroids. Thirdly, that for her success in this matter a dilated or dilatable os, and uterine contraction, are the only essential requisites. Fourthly, that it is the duty of the physician, when these conditions are absent, to step in with his art and imitate the process. Fifthly, that for this purpose the cervix must be opened up, and the uterus be stimulated to contraction. Lastly, that both these objects will be greatly facilitated, in the case of interstitial tumours, by their forcible detachment from the position in which they are embedded, thus making them more and more foreign to the organ where they are lodged.

Now as to details. The conditions of success are, as I have said, an open os, uterine contraction, and detachment of the tumour. As to the first, I am strongly opposed to the method of dilatation, and in favour of free division. I have seen infinitely greater evils result from dilatation than from division. Moreover, the former is inadequate to the subsequent steps of the operation, because—and this applies especially to cases where the tumour is large—I think it is most important that the process of detachment should be conducted gradually, and in successive portions, so as not at one time to expose a very large surface to the risk of inflammation and purulent absorption. If the cervix be divided, it is done once for all, and is ready for the successive steps of the operation; whereas in dilatation the same process has to be repeated every time that we resort to detachment.

My plan, then, is, first to divide the cervix freely in two or three places, taking care to prevent reunion of the divided surface, and always to plug the vagina for a few hours after the division is made. In about a week, or at most a fortnight, from this time, supposing that all has gone on well, I introduce the finger up to the tumour, and, with whatever force is necessary (sometimes a good deal is required, sometimes but very little), I break through the tissue covering the tumour at the point where it joins the healthy uterus. Once within what I call the intracapsular space—that is, the space between the tumour *and the uterus,—there is*, according to my experience, very little difficulty indeed in passing the finger freely round the tumour in all directions, breaking down its loose cellular attachments, and, in short, shelling it out from its uterine bed. As I have said, I greatly prefer not doing all this at once, but in successive steps. When the tumour is in good part detached, but not before then, we may advance to the next step in the process, and endeavour to procure its expulsion. This we do by the administration of any of the various oxytoxic agents, of which ergot and borax are certainly the best. Sometimes I have used galvanism, with evident success; but in this matter I do not think it is of much importance what we use, for, if the tumour is only well separated, the uterus will usually contract most efficiently, and all the more readily, no doubt, because of the irritation to which it has been subjected by the previous operations. The last step in the operation is the removal of the tumour by means of the écraseur, in the same way as has been already recommended in the case of polypoid growths. I have once succeeded in entirely enucleating such a tumour without the use of the écraseur; and I do not at all see why in some cases we should not be able completely to detach it with the finger, just as I have done again and again in post-mortem cases. Where we are dealing with a very large tumour, it may occasionally be necessary to remove it in successive portions. I had a case of this kind under my care some time ago; the uterus was occupied by one of these interstitial fibroids, and was of such a size that the fundus reached fully up to the umbilicus. In that case, after I had detached a good part of the tumour, the uterus contracted so vigorously upon it that in four days a large mass was forced into the vagina, and I was compelled to remove it before waiting for the subsequent separation and removal of the remaining portion. Ultimately, however, I got it all away, and the patient came to me the other day perfectly well.

Such, then, is the plan of enucleation which I am in the habit of practising, which I recommend strongly as by far the best way of treating these cases of interstitial fibroid, and which, if conducted with all due care and in properly selected cases, is, I am convinced, not nearly so dangerous an operation as it has been represented.

If you will pardon my detaining you a little longer, there is just one other method of treating these cases about which I should like to say a few words. It is a method which was discovered, I believe quite accidentally, by my friend Dr. Greenhalgh; and it consists in the application of the actual cautery directly to the tumour itself. Dr. Greenhalgh was engaged one day in some cutting operation upon a rather large interstitial fibroid, when the hemorrhage became so severe that he was obliged to desist; and as all styptics failed to arrest the bleeding, he applied the actual cautery so freely to the bleeding surface that he burnt quite a hole in the tumour. The bleeding stopped, and, to his great surprise, he found in the course of a few days that the tumour was protruding forcibly through the opening made by the cautery. Little by little the tumour descended, until the whole mass was eventually extruded, having been but slightly assisted by art. Encouraged by this, he in a subsequent case determined to repeat the experiment. I was requested to meet him in consultation, and was present when the operation was performed. The case was one of a large interstitial fibroid, growing entirely in the posterior wall of the uterus, and invading the cervix so completely that its posterior lip formed part of the tumour, and was, in fact, expanded over it; the tumour itself occupied three-fourths of the pelvis posteriorly, the os and anterior wall of the uterus being thrust forward against the symphysis pubis. A small conically bulbous cautery was thrust through the vaginal roof and into the centre of the tumour, about an inch or so behind the os uteri, to a depth of about an inch or an inch and a half. No bleeding followed the operation. In the course of a few days the opening thus made began to dilate, and the finger, when passed through it, readily detected a space between the tumour and the posterior wall of the uterus, where detachment could be easily effected. Dr. Greenhalgh being obliged to leave London for the Continent, I was requested to take charge of the case. At that time, which was about a week after the application of the cautery, the opening was fully as large as a crown-piece, and through it the tumour was beginning to protrude. All this, too, by the natural effort, and without the help of any drug. In four or five days more, during which time I had aided the process of expulsion by daily detaching it more and more, the mass came to occupy the whole of the vaginal cavity, and began to assume a sloughy condition, from which serious constitutional disturbance threatened. I therefore applied the écraseur, and removed all that was then practicable. A week after this Dr. Greenhalgh returned, and by that time more of the tumour had been extruded. The patient was accordingly placed under the influence of chloroform; and then, without the slightest difficulty, the entire mass was turned out with the finger, no other instrument being used. The patient made a completely satisfactory recovery.

Such a case is highly instructive from several points of view; it seems to open up an entirely new plan of treating these growths, and one which, I think, has very much to commend it. It avoids the danger of hemorrhage in the cutting operations, and may possibly prove preventive of purulent infection; while it seems also in some way to promote uterine contraction and expulsion of the tumour. It is, however, of course very limited in its application; for it can only be resorted to in cases where the tumour is close down upon the vaginal roof and has entirely invaded the cervix.—*Lancet, May* 10 *and July* 12, 1873, *pp.* 657, 35.

105.—CASES ILLUSTRATIVE OF PERSISTENCE OF THE HYMEN.

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The following cases, while presenting to the obstetrician much matter of interest, are of still greater importance to the medico-legalist, as they afford incontestable proof that persistence of the hymen is compatible with the wedded state, that its destruction does not necessarily follow even the calling of a prostitute, and hence, that its persistence after attempted rape must not be relied upon solely as evidence sufficient to disprove the charge.

Case 1.—A lady, aged 43 years, who had spent twenty-four years in the matrimonial condition, suffered from a swelling in the right iliac fossa, which, on careful examination by external palpation, appeared to be connected with the ovary of that side. In order, then, to ascertain exactly its relation to this and the other pelvic viscera, a vaginal examination was attempted, but at first no vaginal orifice could be discovered, the meatus being guarded by a perfect hymen, having anteriorly a small aperture which permitted the escape of the menstrual flow. Careful examination further revealed a slight depression involving the anterior portion of the perinæum and the posterior portion of the hymen. This depression, evidently the result of frequent attempts at coitus, was, under pressure by the finger, capable only of affording a cul-de-sac not more than three quarters of an inch in depth, while its normal depth was but one-quarter of an inch.

Case 2.—In this case the condition of parts observed in Case 1 was also observed, but was greater in degree. A lady, aged 30 years, who had been married ten years, but was childless. She complained of all the symptoms indicative of prolapsus uteri. On a vaginal examination being made, the os uteri was found situated about an inch from the meatus. The os uteri presented to the finger a very ill-defined outline; and this, on careful examination, was found to be due to the intervention of a thin membrane between that organ and the finger. This membrane was then discovered to be but a persistent hymen, which, from frequent attempts at sexual intercourse, had become pouched, so as to give rise to the formation of a perfect cul-de-sac, which, in its normal state, was from three quarters of an inch to one inch in depth, but under pressure, either by the finger or the speculum, was capable of being distended to the extent of about one inch and a half. This cul-de-sac, as in the former case, involved the posterior two-thirds or rather more of the hymen and the anterior portion of the perinæum, and its persistence was evidently due to the presence in the tissue of strong bands of fibrous or fibro-elastic tissue, which rendered the structure as a whole very resilient.

In Case 1 no steps were taken to destroy the hymen, owing to the comparatively advanced age of the patient, as also to the fact that should pregnancy have followed (which, while improbable, was yet possible), the ovarian tumour might have led to a complication of no little gravity. Bromide of potassium was given in large doses, with a view to decrease the size of the ovarian tumour, or, at least, to prevent its further development. This treatment was continued for several months, the tumour being at the expiry of that time rather smaller than formerly. When last seen, patient was still much in the same condition. Her husband was a robust man, weighing 14 stone, and was in every respect well formed.

In Case 2 the patient was well placed under the influence of chloroform, and the hymen was then forcibly ruptured by means of a speculum. The uterus was then found to be not only prolapsed, but also retroverted and slightly retroflexed. The uterus was replaced in its normal situation, and there secured by means of a Hodge's pessary. In a few days, all inconvenience arising from the operation had subsided, and the symptoms due to the prolapsus had entirely disappeared.

These two cases are then, I think, sufficient to prove that, even under circumstances the most favourable for its rupture, the hymen may remain intact, and if we remember that in cases of attempted rape the circumstances are less favourable—any struggling on the part of the female having a tendency to prevent perfect penetration—we are, I think, warranted in inferring that in such cases the proportion of those in which the hymen is not destroyed must be considerable.

Still further, however, to strengthen this position, I shall now shortly relate the particulars of three cases (3, 4, and 5) in which the hymen was found persistent in prostitutes. These three cases were found among about 1500, who within the last few years, applied for admission to the Glasgow Magdalene Asylum, Lochburn, Maryhill, and should subsequent experience establish this as the precise proportion, viz., 1 in 500, it will considerably affect the significance of persistence of the hymen as an evidence or a sign of virginity.

Case 3.—A. B., aged about 25 years, and for seven years a prostitute, applied for admission to the Magdalene Asylum. With a view to ascertain whether or not she suffered from any venereal or other disease of an infectious or contagious nature, she was subjected to the usual examination. In every respect she was healthy; but the hymen was perfectly entire, save anteriorly where there was a small aperture, barely sufficient to permit the passage of a crow-quill. She stated that she had never suffered any inconvenience during her menstrual periods, that she has never contracted any venereal disease, and that she was perfectly unaware of there being anything abnormal or unusual in her conformation. In this case the hymen was firm but resilient, containing evidently numerous elastic and fibro-elastic bands.

Case 4.—C. B., aged about 24 years, also a prostitute, had adopted this path of life at the age of 16 years. Having likewise presented herself for admission to the G. M. A., was, on examination, found perfectly free from all disease; but presented a persistent hymen of a soft, but strongly resilient consistence, indeed, so much so was this the case that on making pressure with the finger, it afforded the sensation of small reduplications of mucous membrane being gradually thrust aside, but careful examination established the case as one of persistent hymen, in which the usually thin crescentic duplicature of membrane was greatly modified by the deposit in its substance of numerous muscular bands. She, like her predecessor, had never contracted any venereal disease, suffered no inconvenience during menstruation, and was perfectly unaware of there being any unusual condition of parts.

Case 5.—Like the preceding cases, this person, D. M., aged 30, had been a prostitute for 11 years, and applied for admission to the G. M. A., wishing to alter her mode of life. She had never suffered from any venereal affection, and presented a perfectly well-defined crescentic hymen of the usual size, but of very dense consistence—indeed, to the touch it seemed almost cartilaginous.

These cases are full of significance pointing most distinctly to the fact that *the persistence* of the hymen, *per se* as a sign of virginity, must be viewed *with great care*, while it must, at the same time, be taken as one of the class of signs rather collateral than direct in influencing the decision as to whether connection has, or has not taken place, or at least been attempted.

As adding still further proof in favour of the occasional occurrence of this rather abnormal condition, I have but to refer to one case which has come under observation, and which adds another to those already on record in which conception has taken place without rupture of the hymen.

These cases may be divided into those occurring in unmarried and those occurring in married females. In the majority of cases included in the first class, it appears that an explanation may be found in the fact that popular opinion holds it impossible for a conception to occur without full penetration; and hence a certain degree only of penetration is in these cases admitted, and thus, if the hymen be at all resilient, it may resist the slight pressure made upon it, while by means of the small anterior aperture the fructifying element may find its way to the cavity of the uterus, and thence to the ovum. But in the case of married females, constituting the second and smaller section, no such explanation will hold, and we are hence shut up to cases of what might, for want of a better appellation, be termed *genuine persistent hymen*, due to abnormal development of tissue in its structure. It is to this latter class that the following case belongs:—

Case 6.—Mrs. A., aged 29 years, had been married seven years before this her first confinement. When first seen she had been in labour about twelve hours. Vaginal examination being attempted failed to reveal any external orifice, and this rather peculiar phenomenon at once received explanation when a persistent hymen was discovered of such size and form as completely to occlude the meatus, save in its central part, where a small aperture was discovered sufficient in size to admit an ordinary sized crow-quill. She had no idea that such was the condition of parts, and seemed to have suffered no inconvenience during menstruation. Labour was tedious, but in the course of forty-eight hours the head of the child was found to be fully distending the perinæum. A crucial incision was then made through the hymen, and delivery effected by the use of the forceps, this being necessitated by the exhausted state in which the patient now was. Recovery was rapid, and the child, though born asphyxiated, was resuscitated. Mother and child are at present alive and well.

Such are examples of persistent hymen in which the persistence is due to an adventitious development of elastic, fibro-elastic, or muscular tissue. But there is another cause which may lead to the presence in the married female of a perfect hymen. The cause to which I refer has, by Dr. Marion Sims, been termed *vaginismus*, and this affection may be defined as a peculiar state of parts whereby, either from excessive nervous supply, or a peculiar irritable condition of the normal supply, any attempt to consummate the sexual act, or indeed, any pressure made in the vicinity, causes the infliction of intolerable pain upon the female. This is an affection certainly much more frequently met with in females in whom the hymen has been destroyed, and in whom the seat of the affection is rather the carunculæ myrtiformes, but the cases which I have here to record are examples of the affection conjoined with perfect development of the hymen.

Case 7.—Mrs. B., aged 38 years, had been married thirteen years. Her husband stated that whenever he attempted performance of the sexual act, she suffered excessive pain and implored him to desist. She was in every other respect healthy. When first seen, patient seemed anxious to be cured, and had no hesitation in submitting to a vaginal examination, but no sooner did the finger touch the vulva than she cried out loudly, and sprang to the opposite side of the bed. After various unsuccessful attempts, it was arranged to visit again in about eight days, when chloroform should be administered, and the necessary proceedings adopted. On calling the following week, she objected to be placed under the influence of chloroform, and said she would much rather endure the pain, as she was firmly convinced she would not recover from the effects of the anæsthetic. An attempt was accordingly made to proceed without it, but beyond ascertaining that the hymen was perfect, nothing further could be learned, and no operation performed, on account of the intense suffering which the least manipulation of the parts induced. Since then she has been frequently seen, but she is still in the same condition as previous to her *formal* marriage, an event of fully eighteen years ago. Query. According to Scotch Law, was marriage in this case consummated, and might a decree of nullity of marriage be obtained?

The two following cases are perhaps more interesting, as in both the obstacle was removed, and the operative interference was rapidly followed by conception.

Case 8.—Mrs. N., aged about 25 years, had been married four years. She stated that since her marriage sexual intercourse had been entirely precluded by the extreme degree of suffering induced by any attempt at its performance. General health perfect. On examination the parts were found very tender and sensitive, the very slightest pressure occasioning intense suffering. Patient being placed under the influence of chloroform, there was found in the centre of a persistent hymen, which was tense and unyielding, an aperture about the size of a crow quill. A bougie was thrust through the hymen, then one of larger size, then a small thick-glass speculum, and ultimately one of medium diameter. After a lapse of eight days, chloroform was again administered, and the dilatation continued till the largest sized speculum was introduced. Patient complained a little of pain, but this was perfectly relieved by the use of the atropine pessary. Within a fortnight recovery was perfect; she returned to her husband in Marseilles, and has since given birth to four children.

Case 9.—Mrs. M'N. has been married seven years. In all respects has enjoyed good health, but complains that any attempt at sexual connection is accompanied by pain so excessive as at once to necessitate desistance therefrom. On examination the hymen was found entire, with a small fissure about half-an-inch long in a slightly oblique direction. Here, also, as in most of the preceding cases, there was a small aperture anteriorly, by means of which the menstrual discharge obtained egress. Chloroform was administered, and an incision made through the hack or fissure, after which specula of various diameters were introduced, thereby perfectly destroying the hymen. In a few days recovery was perfect, and she is now the mother of three children.

These, then, may be taken as typical cases of vaginismus, an affection which may be said to be of recent introduction into the literature of obstetrics, but one which deserves the close attention of the obstetrician. To these I might have added examples of cases of the same affection, but affecting persons in whom rupture of the hymen had already occurred, but, as such cases do not illustrate the subject in hand, I reserve them for future matter of remark.—*Glasgow Medical Journal*, *May*, 1873, *p*. 346.

SYNOPSIS,

(ARRANGED ALPHABETICALLY,) CONTAINING

A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THIS VOLUME, SHOWING AT A GLANCE THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE HALF-YEAR.

AFFECTIONS OF THE SYSTEM GENERALLY.

CANCER.—*Electrolytic Treatment of.*—Virchow believes that cancer is at first purely local; and Waldeyer believes it always originates from epithelium. Cases are related in which the following treatment dispersed the tumours in the early stage, and when the parts affected were not adherent to the deeper parts. Even in such cases however the plan may be tried. The method of applying the agent is described as follows:—"Suppose, in the first place, that the tumour is of moderate size, hard, and of slow growth. With or without chloroform, as circumstances may determine, two, three, or four needles are introduced at equal distances into the tumour, and connected with the negative pole or cathode of the battery. The positive pole or anode is connected with a broad plate, which is applied with a wet compress to the skin at a certain distance from the tumour, and moved from place to place. The current is gradually increased up to thirty-five or forty elements, and after twenty to thirty minutes, gradually lessened, and needle after needle withdrawn. This operation is repeated for a limited number of times at intervals of three or four days, the needles being introduced at different parts of the tumour. After this energetic treatment, a weak current of four to eight elements is to be used daily for months. The negative pole, formed of a flat plate and a wet compress, is placed over the needle punctures, and the positive at a distance from the tumour as before. The sloughs produced by the needles come away after a time, and their track serves as a conductor for the current. If, in the second place, the tumour be a large one, then the only modification is that, during the operation, needles should be extracted and placed in different situations. The number of the needles should not, however, be increased above four at one time. If, in the third place, the tumour be large and ulcerating, then an attempt is made to produce sloughing of it. In this case, the anode (or positive) needle is thrust into the centre of the tumour, and its base transfixed with a number of needles connected with the negative pole. These are left for about twenty minutes, and then inserted in a different position. In from seven to ten days the whole or part of the tumour generally sloughs away, and if it is not entirely removed, the operation may be repeated. After this rather severe treatment, there is never any fever or special constitutional disturbance. In this case, also, after the tumour has sloughed, the treatment with weak currents should be continued for months. Of late, the author has sometimes introduced a slight difference in the treatment of the first and second class of cases. Instead of inserting cathode needles throughout, he has occasionally begun with an anode needle, which he has *only kept in for a short time*, and he thinks this an improvement on the original plan. The anode needle should only be kept in for a short time, as otherwise it is apt to cause sloughing; after this single application of the positive needle, he continues as before. It is to be noted that the author attaches great importance to the efficient carrying out of the treatment with weak currents, after the energetic currents have been used preliminarily; and he generally entrusts this latter treatment to the patient. The author has also used a similar mode of treatment, as others have done, in aneurisms, varix, &c. His method is in this respect different from the usual, that he does not insulate the stems of the needles which he introduces." (M. Neftel, p. 54.)

CHOLERA.—*Chloralum.*—Dr. Blanc recommends the internal administration of chloralum (chloride of aluminium) in cholera. It is a powerful disinfectant, and, unless the success which certainly appeared to follow its use in really desperate cases in India was merely coincidence, it is a remedy of great value, and acts by destroying the poison in the system. It is free from smell and taste. A mixture of one part of liquid chloralum must be made with twenty-five of water, and an ounce of this should be given every half-hour, and an enema containing sixteen ounces of the mixture should be administered every hour. Undiluted chloralum should be sprinkled upon and around the bed. (Dr. H. Blanc, p. 55.)

FEVERS.—In all fevers attended by the action of weak hearts, try digitalis as a heart tonic. Dr. T. W. Grimshaw has especially attracted our attention to this remedy. He sometimes gives as much as one ounce and more of the *infusion* of digitalis every three hours for five or six days together. The heart seems to have been even invigorated by it. Dr. Anstie thinks that it sustains the exhausted heart, checks the too rapid pulse, and in short enables the heart to hold on till the course of the fever changes. (Dr. F. E. Anstie, p. 17.)

GOUT AND GRAVEL.—The uric acid diathesis is a disease of sub-oxidation. Uric acid can be converted into urea by the action of oxygen, and this process is always going on in the body. When, however, the oxidation is imperfect, part of the uric acid, a larger part than natural, is unoxidised, and gout or gravel result. The ethereal solution of peroxide of hydrogen, commonly known as ozonic ether, affords us a mode of supplying oxygen, and it is found that this drug gives us a mode of successfully treating gout and allied diseases. The dose recommended is half a drachm to a drachm of the ether in a wineglassful of water three times a day or oftener. (Dr. Day, Med. Times and Gazette, May 24, p. 554.)

INTERMITTENT FEVER.—*Treatment of by other Cinchona Alkaloids than Quinia.*—It is found, as the result of extensive experiment, that the cinchona alkaloids—cinchonia, quinidia, and cinchonidia—possess considerable therapeutic value; and, as they are obtained very largely in the manufacture of quinia from some common barks, their practical utility is of great consequence. Quinidia is the most energetic. It is found that it is most serviceable when administered during the paroxysm only. The least active is cinchonia, but even this is energetic, and, in an adequate dose, a sure remedy. The average amount of quinia necessary to cure a case of intermittent fever varies between 31 and 70 grains, of cinchonia from 54 to 76, and of cinchonidia from 43 to 90. (Dr. Dougall, Edin. Med. Jour. Sept., p. 193.)

MILK AND DISEASE.—The outbreaks of fever, the sources of which have been traced to milk infected with typhoid poison, have drawn a good deal of attention to the peculiar properties milk has of absorbing surrounding atmospheric poisons. A little milk exposed in a saucer all night near a sink will be found in the morning to possess a mawkish and unnatural odour. It is peculiarly liable to putrefy when there is a thundery atmosphere, and it is singular that veal—a meat fed on milk—will sometimes become quite decomposed under the same atmospheric or electric conditions in the course of a few hours. (Mr. H. Willington, p. 52.)

Sewage Grass.—The following conclusions seem pretty well established:—1. Sewage grass is quite as healthy and good for cattle as ordinary grass. 2. If the sewage is run on in too large quantities, it is possible that evil may follow, as the liquid matter may adhere to the grass for several inches from the ground. Even in this case no communicable disease—such as typhoid—would be given to the drinker of the milk. 3. Supposing the sewage matter to be contaminated with typhoid poison, there is no evidence that typhoid or any other form of disease can be thereby produced in animals. 4. Numbers of scientific witnesses are unanimous as to the excellent character both of milk and butter produced from sewage grass. (Editor of Medical Times and Gazette, p. 22.)

Typhoid Fever.—Two important facts appear to have been established comparatively recently:—1. That typhoid fever may be generated *de novo* from habitual drinking of water contaminated with sewage, or from exposure to gaseous emanations from sewers. Dr. Murchison cites numerous instances of sporadic and limited epidemic outbreaks of enteric fever, the spontaneous origin of which in sewer emanation, and in contaminated drinking water, was proved to a certainty, there being no proof of previous pollution with enteric excreta. 2. That milk possesses such absorbent properties that if merely exposed in an open vessel in the neighbourhood of a typhoid case, it may become a vehicle for the poison. (Dr. J. Dougall, p. 44.)

Try the sulpho-carbolate of sodium, on the principle of Dr. Sansom's antiseptic treatment of small-pox. Give 20 grains every four hours to an adult, and gradually increase during the next few days to 30 grains. Beyond this you may produce cerebral symptoms. Children require much smaller doses, and may have very small doses of Batley's liq. opii sed. to assist in checking the diarrhœa. If possible, give the remedy during the period of incubation. (Dr. S. Skinner, p. 35.)

AFFECTIONS OF THE NERVOUS SYSTEM.

Facial Neuralgia.—*Aconite.*—Aconite is a powerful local remedy, acting not only as a controller of morbid sensation, but as a retarder of vascular action, and by consequence as a modifier of muscular action. Small quantities of the drug are readily absorbed through the skin, attack the nerves and muscles in their neighbourhood, but show little disposition to be absorbed into the general current of the circulation. In cases of facial neuralgia it is of the greatest use. (Dr. F. Bagshawe, p. 60.)

Hysterical Hyperæsthesia.—Nothing is more useful in cases of hysterical hyperæsthesia than faradising the part pretty strongly. It should be applied unsparingly. Supposing, for instance, that the hyperæsthetic area is on the skin, dry conductors should be used, one being placed five or six inches distant on an indifferent spot; the other (which should be in the form of the wire brush) should be so used as to apply a kind of electrical flagellation to the whole of the affected area. This must be kept up several minutes with a powerful current; and if the painfulness of the proceeding becomes intolerable, it is better to give the patient a little chloroform and go on, than to do the thing by halves. Faradisation is of no use in true neuralgia. (Dr. Anstie, p. 74.)

Lead-Poisoning.—People may be poisoned by water passing through a lead-pipe from a well to the house. We suspect this may be the case in towns, and when the water is particularly soft and pure the danger may even be increased. It is suggested by Dr. Medlock in such cases to have block-tin pipes. (Dr. J. W. Von Tunzelmann, Dr. Medlock, p. 26.)

Collateral evidence and argument in every way demonstrate the superiority of hard against soft water. Soft water is very capable of absorbing lead, and at one time in Glasgow there was a good deal of lead poisoning and colic from this source. (Editor of Med. Press and Circular, p. 25.)

Migraine.—Migraine may be artificially induced, as the result of injury, and when so originated may recur for years, just as does ordinary migraine. In a case of this nature in the person of a medical man great benefit was always experienced from taking a cup of strong tea at the commencement of the attack, also from a tumbler of sherry wine; but the remedy *par excellence*, the patient found in guarana. Four doses of thirty grains each taken in two hours afforded such relief that the attack which foreboded to be of unusual severity turned out to be a very small affair indeed. (Dr. D. Morton, p. 65.)

Give three grains of guarana whenever the attack comes on, and in one or two hours, if not relieved, take another dose. (Dr. Macdowall, p. 66.)

NEURALGIA AND PARALYSIS.—*Electricity.*—Electricity may be of great use. Faradisation may do good occasionally, but the constant voltaic current is the great thing in cases of pain. Both the electrodes ought to be firmly pressed upon the skin and held there, neither of them being moved about; "that is, the current must be voltaic, and must be *constant*, not interrupted; and the electrodes must be applied so as to include in the circuit the affected nerves." The number of cells used must be according to the feelings of the patient; they should be as many as can be borne without pain. In the face five cells will be enough to begin with, but fifteen or twenty cells may be required; elsewhere more cells may be used. One of the electrodes should be placed over the point of origin of the nerve, and the other over the painful spot. The direction of the current seems not to be so important, but it should be used gently and steadily.

Paralysis.—If *cerebral*, be careful not to employ it too soon; wait some months before trying it, till all head symptoms subside. First, test the excitability of the muscles by the faradic and interrupted voltaic currents. The amount of contractility the muscles show will tell what good may be obtained. "If the contractility is normal, though voluntary power is abolished, electricity will do nothing towards restoring the power of the will over them; if the contractility is somewhat, but only slightly diminished, the condition of the muscles may be improved; and when the contractility is found to be markedly reduced, but not abolished, great good may be achieved." Having tested the muscles, "treat them by means of the current to which they most readily answer; or employ the interrupted voltaic current where the excitability is normal, and the faradic where it is diminished."

If *voltaism* is employed it must be by the interrupted, not the continuous current. Large-sized sponges should be used, well wetted with salt and water, and one (the upper one of the two, that is, the nearest to the central organs of the nervous system) being kept steadily on one spot, the other should be moved slowly down along the course of the muscles; so that every part of each of them is successively brought into the circuit of the current, and by the movement of the lower sponge the current is *interrupted*. It does not matter whether the current is direct or inverse; only it must be remembered that the direct current acts more strongly on the muscles than does the inverse, and that the current must not be so strong as to excite pain." This rule also applies to faradisation.

"If the *faradic* current is used, sponges or metallic buttons covered with chamois leather are to be used; and they must be *well* wetted, or the current may only irritate the skin, instead of penetrating to the muscles."

In *traumatic paralysis* faradisation is invaluable. The muscles will often remain paralysed after the injured nerve or spinal cord has recovered, but faradisation will act wonderfully. The faradisation will, of course, be of no use till after the injured nerve has regained its power of conduction; but in recent cases of injury the interrupted voltaic current should be localised in the affected muscles, and direct faradisation be employed when farado-contractility begins to return.

"In cases of *local paralysis*, dependent on cold or on metallic poisoning, as from lead or mercury, or on diphtheria, the employment of electrisation should not be neglected when the affection has resisted other means of treatment. In these cases the faradic and the interrupted voltaic currents, used alternately and localised in the affected muscles, will generally effect a cure. As a rule the farado-contractility is unaltered in muscles paralysed by exposure to cold; but this is not the case in facial paralysis from cold. Here the farado-contractility is diminished or quite lost, while the excitability to the interrupted voltaic current is increased; and faradisation may be employed for weeks without effecting any improvement. In this affection, therefore, the interrupted voltaic current must be used and localised in the muscles till the farado-contractility is restored, and then the two forms may be employed alternately; and in this way cases of even years' standing may be cured or greatly benefited." (Ed. of Med. Times and Gazette, p. 69.)

Neuralgia.—You must understand the difference between the galvanic and faradic currents. For neuralgia the *galvanic* current is the best, although faradisation sometimes answers; a rough, harsh current does no good, but a rapidly interrupted and pleasant current will do much better—"better to use a mild current a long time than a strong current a short time." The direction of the current has been too much trusted in. Pain may be relieved and other benefits obtained by the passage of the current through the body by either pole, and in any direction. In this way you may treat migraine, facial neuralgia, gastralgia, sciatica, and other painful nerve affections. (Dr. G. M. Beard, p. 78.)

On using the constant current for the relief of trigeminal neuralgia, it is unimportant whether the current is passed from centre to periphery, or the other way. The relief obtained is just the same whether the positive pole is placed on the nape of the neck, or on the focus of pain on the face. (Mr. S. Craddock, p. 83.)

NEURALGIA.—*Phosphorus.*—Further experience of the use of phosphorus in cases of neuralgia confirms the opinion that it is a remedy of great value. We should commence by giving $\frac{1}{18}$ or $\frac{1}{12}$ of a grain every four hours. Unless this gives decided promise very little good will be gained by increasing it. Various solvents may be employed, but perhaps none is better than absolute alcohol, which dissolves ten grains to every 1000 drops by the aid of heat. Chronic cases are cured the least rapidly, when they are cured at all; while among them is found the only absolute failure of the remedy employed. Muriate of ammonia administered in doses of from one to two drachms every two or three hours, ranks next to phosphorus in efficacy; but it must be given in doses as large and frequent as these. (Mr. J. A. Thompson, p. 84.)

TOOTHACHE.—*Electricity.*—Pass a continuous current from the neck through the side of the jaw which is painful. The negative pole is placed on the anterior lateral portion of the neck, and the positive pressed on the cheek over the painful tooth. (M. Bouchaud, p. 71.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

WOUNDS AND ANEURISMS OF THE VERTEBRAL ARTERY.—Wounds and aneurisms of the vertebral artery are particularly liable to be mistaken for similar affections of the carotid, and no less than seven cases are recorded in which the carotid was tied for vertebral aneurism, and four for wounds of the vertebral artery. The reason of this mistake is, that pressure on the carotid for two inches above the level of the clavicle arrests the circulation through the vertebral artery also, which lies immediately behind it. As, however, the vertebral artery passes into the foramen of the transverse process of the sixth cervical vertebra, it is not compressed by pressure on the carotid above the level beforenamed, and in this fact rests our power of diagnosis between wounds and aneurisms of the two arteries. Ligature of the carotid for wound of the vertebral artery is attended by the most disastrous consequences, as the brain is deprived of its supply of blood, and the remedy used tends to aggravate the disease it was meant to cure. (Mr. T. Holmes, p. 142.)

EPISTAXIS.—In a case of epistaxis related by Dr. Leeper in which the ordinary mode of plugging seemed inapplicable as the patient was almost dying, the hemorrhage was arrested by passing into each nostril two medicated suppositories of perchloride of iron, each containing two grains. A bit of dry lint was then placed on the anterior nares. Ergot was administered internally. (Dr. W. Leeper, Dublin Journal of Medical Science, Nov., p. 364.)

WRITER'S CRAMP.—Patients in whom symptoms of writer's cramp have appeared, should always be warned that they will never be able to perform an excessive amount of writing, and that any attempt to do so may be followed by disastrous consequences. Unfortunately the disease rarely comes under the eye of a physician till it has existed months or years, and has made considerable advances. The application of the continuous current of electricity to the weakened muscles is of the greatest service, and is preferable to Faradisation. The positive pole should be placed over the brachial plexus in the axilla, and the negative in a basin of warm water in which the hand is immersed. Voluntary movement of the hand is to be practised simultaneously. (Dr. W. A. Holles, Practitioner, Aug., p. 85.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

Hæmoptysis.—*Hypodermic Injection of Ergotine.*—We possess no remedy equal to ergotine as a remedy for hæmoptysis. The prompt suppression of all oozing into the air-cells which, in view of the danger of phthisis in *all hæmoptoe*, it is so important to effect, can probably be in no way so effectually accomplished as by the employment of this agent. It acts the most rapidly and effectually when administered subcutaneously, for which purpose Bonjean's ergotin is the best. (Dr. Anstie, p. 97.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

Diarrhœa of Children.—Oxide of zinc is a remedy of value in children's diarrhœa. It should be given in doses of two grains every three hours, mixed with mucilage and a little simple syrup. (Mr. W. Berry, Practitioner, Nov., p. 356.)

Flatulent Distension of the Abdomen.—The most important point in the treatment of this affection is to select such kinds of food as give the greatest amount of nourishment, yet tend the least to produce flatulence. Let the patient take no bread, but eat biscuits instead; take no solid vegetables, but use lemon juice freely; shun soups, cucumbers, and fruit. Prefer meat, fish, fowl, or game—tender, but perfectly fresh; take no rich sauces, or meat twice cooked, or salted or seasoned. (Dr. F. Sibson, p. 99.)

Puncture of the Bowel in Extreme Flatulent Distension.—In cases of extreme flatulent distension, we need not fear puncturing the bowel with a fine trochar in order to give relief. The results of this practice are most satisfactory, as shown by a case in which it was repeated four times. The canula used should be three or four inches long, otherwise as the belly falls it will slip out of the gut, and moreover a considerable thickness of abdominal walls has sometimes to be traversed before the bowel is reached. Not the least pressure should be made upon the abdominal walls, as one may be tempted to do when the gas is nearly exhausted, because there is a liability to squeeze out a little of the fluid contents of the intestine. Care should be taken to puncture that part of the intestine which is highest as the patient lies in bed, otherwise a jet of liquid fæces may be shot out with the first sudden explosion of gas. The chances of extravasation of any fluid into the peritoneum from the fine punctured wound are very small. The puncture merely separates the muscular fibres, and on the withdrawal of the canula the muscular planes immediately readjust themselves, or a protrusion of mucous membrane takes place, which entirely prevents the escape of the intestinal contents. (Dr. H. C. Cameron, p. 153.)

Liver Affections.—*Carlsbad Water.*—The hepatic disorder most likely to be benefited by the Carlsbad cure is hyperæmia. This usually results from overeating and drinking, and especially much indulgence in rich articles of food, without sufficient exercise. There is generally dyspepsia, sallowness of skin, bilious vomiting, and a sense of uneasiness and fulness in the region of the liver. Fatty liver may be actually cured by this treatment; but if the pulse is irregular or there is tendency to syncope or vertigo, "the cure" will be contra-indicated. For gall stones and biliary concretions the Carlsbad waters are considered almost a specific. (Dr. S. H. Ward, Lancet, Aug. 2, p. 145.)

HERNIA.—*Radical Cure.*—After an operation for strangulated hernia, there is no reason why something should not be attempted to prevent a return of the protrusion. The real danger arises rather from the state of the strangulated bowel than from any way of dealing with the peritoneal sac and parietes after the strangulation has been relieved. The best mode of carrying this out is, after relieving the strangulation, to bring together the sides of the sac and the aponeurotic structures by means of a handled-curved hernia needle armed with silver wire. That this operation is successful in permanently curing cases of hernia is proved by the instances related. (Mr. J. Wood, p. 148.)

A new method of Operating in certain cases of Strangulated Hernia.—Mr. Annandale relates a case of large strangulated umbilical hernia in which he divided the abdominal parietes to the extent of three inches in the mesial line between the umbilicus and ensiform cartilage. The gut was easily removed from the sac by traction from within, and the firm ring at the neck of the sac was then divided freely by a probe pointed knife introduced through the wound. The bowels were freely moved three hours after the operation, but the patient remained exhausted and died from a fatty condition of the heart forty-eight hours after the operation. This operation is by no means intended to supersede the ordinary plan, except in certain cases, viz., umbilical hernia of large size, especially if the coverings are thin or ulcerated; 2nd, large scrotal herniæ which have been recently reducible; 3rd, other herniæ in which the usual operations cannot satisfactorily be performed. (Mr. T. Annandale, p. 152.)

ULCERATION OF THE RECTUM.—There are two forms of ulceration of the rectum. Syphilitic ulceration is the more common. It is most frequently met with on the dorsal aspect of the rectum, and occasionally extends completely round the bowel, within a couple of inches of the anus. This form of ulceration is said sometimes to occur as a result of dysentery; it is found occasionally without any obvious cause. There is a constant wearying pain and profuse sanious discharge. It must be treated on the same principles that we should treat syphilitic ulceration of the fauces, viz., mercurial applications locally and iodide of potassium internally; with the object of checking the discharge sulphate of copper lotion may be used as an injection. For ordinary non-syphilitic ulceration it is good practice to apply some caustic directly to the part. Nitrate of mercury lightly wiped over the ulcer with a brush is the most suitable. (Mr. C. Heath, p. 158.)

AFFECTIONS OF THE URINARY ORGANS.

ACUTE BRIGHT'S DISEASE.—In a large proportion of cases, rest in bed, with a scanty diet and a liberal use of diluent drinks, will suffice for the cure. A purely milk diet is very suitable. It may be taken cold or tepid, from half a pint to a pint at a time. An adult will take sometimes as much as a gallon in the twenty-four hours. Purgatives should not be ordinarily employed; they should be reserved for cases in which there is an excessive and increasing dropsical effusion which does not yield to other means of cure, but more especially for cases in which cerebral symptoms, the result of uræmia, are either present or apparently impending. Warm baths are of the greatest service, but the wet sheet and blanket bath is still more efficacious. A sheet is wrung out of warm water, and the patient is enveloped in the wet sheet up to the neck, then three or four dry blankets are closely folded over the wet sheet. The advantage of this kind of bath is that its action may be so much prolonged. *When the wet pack is* removed, the patient should be quickly rubbed dry, and enveloped in blankets. If the renal congestion is excessive, as shown by the scanty secretion of highly albuminous urine, with vomiting, headache, and other threatening nervous symptoms, local bleeding by leeches or cupping on the loins is often extremely useful, and is quickly followed by an increased secretion of urine. (Dr. G. Johnson, p. 110.)

ACUTE CYSTITIS SIMULATING ACUTE BRIGHT'S DISEASE.—In a large proportion of cases acute cystitis, when not excited by any obvious cause, such as stone, stricture, or gonorrhœa, follows directly on a chill. The symptoms are frequent micturition, with more or less of uneasiness or pain in the region of the bladder. The urine is acid, and contains albumen, partly derived from blood, and partly from pus contained in the secretion. It is, however, of normal specific gravity, and is albuminous only in direct proportion to the amount of blood and pus which it contains, and the most careful microscopic observation discovers no tube casts. Confinement to bed or a warm room is necessary. Liquid food, without stimulants, should be given. A warm hip bath night and morning, and a dose of morphia at bedtime to allay irritation and procure sleep, are also of service. If a chronic catarrhal condition of bladder remain, there is no remedy like copaiba, of which one to three capsules should be given three times a day. (Dr. G. Johnson, p. 108.)

CALCULI.—*New Instrument for Removing Débris from the Bladder.*—Mr. William D. Napier has invented a most ingenious instrument for this purpose. It consists of a metallic tube like a catheter, containing an india-rubber stem completely filling it, but capable of being pushed out beyond it. It is introduced into the bladder, and the stem pushed forward a little, when the terminal portion expands into a cup-shaped form, very like a convolvulus flower. This, when it is retracted, embraces any débris of calculous matter which may have gravitated into it, and so protects the delicate mucous membrane of the urethra as it is withdrawn from the bladder. When the india-rubber cup is retracted it forms an olive-shaped terminal projection from the metallic tube. (Mr. W. D. Napier, p. 180.)

Napier's Calculus Detector.—This is an instrument like a sound in appearance, but having the terminal portion black. It is not stated what this terminal portion is made of, but the black pigment is produced by momentary immersion in a solution of nitrate of silver. It is intended to afford evidence of the presence of a calculus by ocular instead of aural proofs—the outer substance being so peculiarly sensitive and readily impressed by contact with any hard particle, however minute, that evidence of the existence of calcareous matter is obtained on its withdrawal from the bladder by the impressions resembling slight scratches that are found on its surface. (Mr. W. D. Napier, p. 179.)

INDIA-RUBBER CATHETERS.—The value of india-rubber catheters is not generally understood. In cases of prostatic disease they are invaluable. They are sufficiently strong to be passed along the urethra without bending, and yet so flexible that they adapt themselves to every curve of the urethra. They can be passed as easily by the patient as by the surgeon. When once introduced they may be retained in position by inserting a hollow plug into the end, which plug has two small rings one at either side through which tape can be passed. The hollow plug is closed by a stilet which stiffens the first three inches of the catheter, and is removable when the patent wishes to pass water. The retention however of the softest india-rubber is not desirable if it can be avoided. It is better to pass the catheter as required. The necessity of retention must be measured by the difficulty experienced in introduction. (Mr. J. Hutchinson, p. 180.)

LITHOTOMY.—Mr. Gutteridge performs this operation in the most rapid manner as follows :—" Kneeling before the patient, when in the lithotomy position and with his eye thus in a line with the perineum, the operator first passed the staff, which he handed to his assistant ; he then made very firm pressure with the fingers of his left hand upon the anus, thus dragging the skin of the left side of the perineum, with the lower extremity of the gut, well over towards the right tuber ischii. He next rapidly plunged the scalpel into the groove *of the staff* at a point a little to the left of the raphé, and on a level with *the anterior* extremity of the tuber ischii, and then without running the scalpel along the groove, he made it cut its way through the soft tissues. Next he passed the beak of the cystotome into the roughened groove, and with the cutting edge directed a little upwards from the horizontal direction, he passed it onwards through the prostate and then turned the edge downwards and brought it out at the lower extremity of the skin-wound. This done, he passed his left index finger into the bladder, felt the stone, then withdrew the staff, and passed in along the finger a grooved conductor, much like a narrow blunt gorget, and along this, after withdrawing the finger, he guided the forceps, only one blade of which was *in* the groove of the conductor. The stone was seized and at once withdrawn and the operation thus completed." (Mr. Gutteridge, Med. Times and Gazette, Nov. 15, p. 548.)

Mr. Davidson, of Antananarivo, Madagascar, describes the mode of operating for stone practised by himself and colleagues at the hospital of that place. A bilateral external incision is made, deeper on the left than the right side. The staff used resembles Buchanan's, but is curved a little at the angle. When the bladder is reached a guide is run along the groove of the staff into the bladder, and the staff then withdrawn. The two halves of the guide are then separated as much as the size of the stone demands by a screw in the handle and then serve as a conductor for the forceps. The stone is then extracted in the usual way. Should the calculus be very large the knife may be applied to the right side of the prostate, thus making a bilateral incision. This is seldom required. Although a bilateral external incision is made, a unilateral one only is made in the prostate at first. (Dr. A. Davidson, p. 160.)

RETENTION OF URINE AND STRICTURE.—*New Instrument for.*—Mr. Teevan describes an instrument used by him in cases of stricture and retention of urine. It consists of an exceedingly fine whalebone guide or bougie, over which is passed a No. 1 silver catheter, open at the end, and grooved on its convex side from the proximal end of the curve for about three inches towards the handle. This groove is made by cutting out a piece of the catheter. The catheter is threaded on to the guide, but the guide only runs in the interior of the catheter from the point as far as the slit. The instrument much resembles one in use in America, invented by Professor Gouley, but differs from it in some important particulars. (Mr. W. F. Teevan, p. 171.)

STRICTURE OF THE URETHRA.—One mode of treating this affection, on the principle of giving the urethra rest, is as follows:—Let the patient retain his urine, and place him in the lithotomy position under chloroform. Place the left forefinger in the rectum for a guide, and open the urethra in front of the prostate to the extent of half an inch. An elastic bougie *à boule* is now to be passed from the wound towards the meatus, to see if it will pass; if not, withdraw it, and introduce into the bladder a large elastic catheter from the wound, and tie it *in situ*. In a few days try an elastic bougie down the penis, and if it pass, try a larger and a larger one; the part will soon dilate. The wound in the rectum will heal easily if the bladder be kept pretty empty, and only emptied through a catheter. (Mr. L. S. Little, Mr. F. Jordan, p. 175.)

Impassable Stricture.—When no ordinary instrument can be got in, one of the smallest filiform bougies must be taken, but if there is much induration a small whalebone one should be selected. Having placed the patient with his back against the wall, the surgeon must take his seat in front of him, and try to pass the bougie through the stricture. If a quarter of an hour's trial fail, the bougie should be withdrawn, and given that peculiar twist at the end which resembles a skeleton key. If this fails, one of Leroy d'Etiolles' "bougies tortillées" can be tried. If these means fail, do nothing more for two days, and then try again. If still you do not succeed, pass a full-sized bougie down to the stricture, and keep it there for a quarter of an hour, pressing it all the time against the stricture. This distends the parts, and the mouth of the stricture often becomes open, so that a small filiform bougie can be got in. This plan very rarely fails. In stricture the urethra is not only contracted but tortuous, so that as a rule metallic instruments are not suitable for their treatment. Chloroform is of very little use in the treatment of stricture, but leeches on the perineum and a hot bath are often of great service. (Mr. W. F. Teevan, Lancet, Aug. 2, p. 148.)

Subcutaneous Urethrotomy.—If the stricture be large enough to admit a dilator you may cure by dilatation. In some cases you may fully dilate, but the stricture will soon return, and will require an operation. Some strictures again are so hard and horny, and complicated with abscesses and fistulæ, that an operation is called for as much to give vent to suppuration as to dilate the canal. In the first case you may treat by subcutaneous urethrotomy, but in the latter cases external incisions are required. Pass a small straight staff through the stricture, and divide it subcutaneously with a small tenotome, which is introduced beneath the skin through a small puncture. Next introduce a good large catheter for half an hour, and draw off the urine occasionally. On the third day the patient will be able to introduce the catheter once a day. (Mr. W. F. Teevan, p. 169.)

Holt's Dilators in Stricture of Urethra.—The use of Holt's instruments is not so free from danger as has been generally supposed. Dr. Tibbits, of Bristol, relates two cases of death following their use. Rigors and symptoms of pyæmia preceded the fatal issue. The points of interest in the post-mortem examinations were: 1. There was no purulent matter at about the seat of stricture, but merely slight extravasation of blood into the tissues; 2. The mucous membrane at the seat of stricture was intact in both cases when examined with a lens. (Dr. R. W. Tibbits, p. 172.)

URETHROTOMY *versus* LITHOTRITY.—No doubt lithotrity in aged and debilitated subjects is often severe and dangerous, as well as lithotomy. "What is to be done when a patient is worn down by long-continued sufferings? The answer is a simple one: Select for the removal of the foreign occupant from the bladder the operation that will produce the slightest shock (and that *only a single one*) to the nervous system, at once relieves the bladder and constitution of all irritation, involves no cutting either of the prostate gland or bladder itself, and in its performances is attended *in most cases* with the smallest loss of blood, is completed in one or two minutes, entails no risk of subsequent hemorrhage, and makes but little demand upon the powers of life for its reparation. That operation, *applicable alike to stones of all sizes*, is the one to which I have previously drawn attention—'urethrotomy by the median section with dilatation of the prostate gland.' Mr. Erichsen in 1859 drew attention in the columns of the Lancet to the benefit of the 'median operation' with prostatic dilatation, but limited it to stones of small size or such as would now be dealt with by lithotrity. Long before his paper appeared, and adopting the mode of section proposed by Allarton, I had made it applicable to such cases as those referred to by Sir Henry Thompson, believing the safety of the patient, whether young or old, and particularly the latter, was greatly enhanced by the incision passing through structures that were in the main 'aponeurotic,' and from the restricted use of the knife in the deeper parts *the grave perils of the ordinary lateral operation were wholly avoided.*" (Mr. W. L. Crowther, Tasmania, p. 166.)

"Prof. Ellis has, years ago, shown that the prostate could not be dilated, and I would recommend any surgeon who has a doubt on the point to attend his incontrovertible demonstration on the subject. To say more would almost seem superfluous, but it may not perhaps be remembered that the prostate gland is encased in two jackets of white fibrous tissue, so dense and strong that the capsule is described, in Quain and Sharpey's Anatomy, fifth edition, page 1216, as 'rather difficult either to tear or cut.'" (Mr. W. L. Teevan, p. 169.)

VENEREAL AFFECTIONS.

GONORRHŒA.—*Local Remedies.*—Gonorrhœa may be treated successfully by local remedies only, and more successfully than by medicines taken internally. The injection used must be weak enough, and often enough used. A grain of sulphate of zinc to the ounce of water, used six or eight times a day, may be commenced with, and increased to two grains if necessary. If this is not answering as well as can be wished, sulphate of copper may be tried instead, in a solution of the same degree of strength as that of the sulphate of zinc. (Dr. P. Foster, Med. Times and Gazette, Oct. 25, p. 461.)

SYPHILITIC AFFECTIONS OF THE NERVOUS SYSTEM.—Syphilitic affections of the nervous system are of course comprised amongst the tertiary forms of the disease, and, according to Ricord, are to be treated with iodide of potassium. The iodide, however, is rarely given in sufficiently large doses, rarely beyond twenty grains three times a day, and very seldom so much. The patient improves up to a certain point, but does not get beyond it. Iodide of potassium should be given as freely as the bromide. It has been pushed up to ninety grains three times a day with much benefit. If there is any convulsive affection, the bromide should be combined with the iodide. (Dr. T. Buzzard, p. 59.)

FRACTURES, DISLOCATIONS, OPERATIONS, AND DISEASES OF BONES AND JOINTS.

AMPUTATIONS.—*Modification of Esmarch's Elastic Bandage.*—Before operating on limbs Professor Esmarch applies an elastic bandage from below upwards to the part to be amputated, so as to force most of the blood upwards. Mr. Cripps, house-surgeon to St. Bartholomew's Hospital, recommends for this purpose that "a short india-rubber tube be used, not only to prevent the blood from returning to the limb, but also for the purpose of removing it in the first place. The two ends of an india-rubber tube, twenty-one inches in length, and about three-eighths of an inch thick, are bound together with a piece of twine, the whole forming an elastic ring seven inches in diameter. A grooved reel revolving between a double handle completes the necessary apparatus. To apply this to the arm, three or four complete turns of the elastic ring are wound tightly round the hand in such a manner as to include the fingers and thumb, care being taken that the turns lie even and do not cross one another. The reel is then put under the free portion of the ring connecting the upper and lower coil. The reel is passed round and round the limb in an upward direction: thus each coil is unwound from below as another is added above. In this way four tight coils of india-rubber are carried up the limb to any distance required. The degree of tightness can be regulated with the greatest nicety by the distance the reel is drawn from the limb by the bandager. This method of driving blood from the limb answers perfectly in the arm and in the lower part of the leg; but in carrying the bandage over the popliteal space the flexor tendons prevent the artery being effectually compressed. A firm pad in the space would probably answer the purpose. To remove the bandage, it may either be unrolled by reversing the action of the reel, or the twine connecting its two ends may be cut with scissors. A very neat little reel in a holder may be obtained at Ferguson's, in Giltspur Street." (Prof. Esmarch, Mr. W. H. Cripps, p. 132.)

ANCHYLOSIS OF THE ELBOW-JOINT.—*Excision of the Extremity of the Humerus.*—In cases of anchylosis of the elbow-joint resulting from injury, the result of resection of the joint is almost always disappointing. Either too great a degree of mobility results or the union between the divided ends becomes more complete than could be desired. Dr. Patrick H. Watson, of Edinburgh, recommends that the end of the humerus only should be excised, the other bones being left intact. He has operated on six cases with very good results. A single linear incision is made on the inner side of the arm, and through this the entire expanded extremity of the humerus may be removed without interfering with any muscular structure of consequence, especially the insertion of the triceps, which is very important. More full details of the operation will be found described in the paper. (Dr. P. H. Watson, p. 125.)

ANTISEPTIC SYSTEM AT EDINBURGH.—Professor Lister principally employs three antiseptic agents: carbolic acid, boracic acid, and chloride of zinc. The advantage of boracic acid lies in this, that being much more soluble in hot water than cold the effect of soaking lint or wool in a saturated boiling solution is a copious deposit of the crystals throughout its substance on cooling and drying, and as these will be but slowly acted upon by the discharges from the wound, the lint retains for a considerable time its antiseptic properties. It is generally used as a dry dressing, overlapping a piece of the protective moistened with an antiseptic lotion. Carbolic acid is in every case where a cavity exists, employed by preference, in consequence of its volatility. Chloride of zinc is chiefly used in a solution of forty grains to the ounce of water for application to the surfaces of wounds after operating on a part where decomposition is already present. (Mr. R. J. Godlee, p. 134.)

Carbolised Resin-Cloth in the Treatment of Wounds.—An antiseptic material may be made much more readily and easily than the carbolized gauze of Professor Lister. A very thin cheap porous calico free from stiffening and unbleached, is the proper material to use, and it is to be steeped in the following composition :—Carbolic acid crystals, melted, two fluid ounces; castor oil, two fluid ounces; purified resin, sixteen ounces by weight; methylated spirit, forty fluid ounces,—mix. Melt the resin first, and then add one-third of the spirit, first withdrawing the resin from the fire, when mixed add one-third more spirit with the castor oil previously dissolved in it, lastly, add very gradually the acid in the remaining portion of the spirit. The whole must be agitated until the materials are thoroughly incorporated. The cloth steeped in this is to be pressed so as to free it from all superfluous liquid, and after being hung up until all smell of spirit has gone kept in tin cases until wanted. (Mr. E. Lund, p. 138.)

Dislocations of the Humerus.—*Jarvis' Adjuster.*—Jarvis' adjuster is an apparatus by which luxations of the shoulder may be reduced with much more ease and certainty than by any other method. A shoulder collar, something like a miniature horse collar, is fixed on the shoulder, and the arm just above the elbow encircled by a broad firm band. These are the two fixed points, and separation of them can be effected to any extent necessary, and with any degree of power desired, by means of a rack-bar contained in a steel case open at the top. The bar is moved within the case by means of a lever and cog-wheel. This apparatus admits of free movement of the limb whilst the extending force is yet constant and steady, and it will be seen at once what an advantage this must be in cases in which the head of the bone is driven through muscular structures which tightly embrace its neck. (Prof. Stokes, p. 128.)

Dislocation of the Hip.—*To Reduce Dislocation on the Dorsum Ilii by Manipulation.*—Give chloroform fully, flex the leg on the thigh and the thigh on the abdomen, abduct the limb fully, and then rotate outwards. (Dr. H. C. Cameron, p. 130.)

Dry Earth as a Dressing for Wounds.—Dry clay, finely powdered and sifted through muslin, or ordinary fine earth, is a dressing of the greatest value in surgical cases. It removes all fetid odour from putrid wounds or cancerous sores. In a case of compound comminuted fracture of the tibia the wound rapidly healed, and the patient was almost free from pain throughout the case. Ulceration of the leg may be cured by means of the fine clay dressing, without confinement to bed. (Dr. J. Groves, p. 137.)

Excision of the Thyroid Gland.—The thyroid gland may be excised, provided the enlargement is central and not of too large size. The danger from hemorrhage may be to a great extent done away with by ligaturing the parts before excising. The principal thing to remember is that the cellular sheaths of the vessels entering the gland at the point of entrance leave the vessels and spread over the gland, making a delicate membranous sheath for the gland, and in all ligatures applied this sheath must be included, as the vessels themselves are too fragile. This sheath having been exposed, and the vessels cut in doing so tied, by means of an aneurism-needle the gland must be ligatured in quarters, the ligatures being placed deeply. In one case catgut was used and the ends cut off short. The gland should then be removed by scissors and the wound closed. The removal of the gland is to be very carefully done, there should be no tearing away of the parts. (Dr. P. H. Watson, *Edinburgh Med. Journal*, Sept., p. 252.)

FRACTURES.—*Iron-wire Box for.*—Dr. McNalty recommends a box made of iron-wire, which folds up when not in use and can be let down at the sides. "Bandages, with the exception of the one for the foot are rendered unnecessary. The wound can be dressed by letting down the sides, unless it be confined to the back of the limb, and in most cases can be seen without disturbing the apparatus. The foot-rest remains fixed when the sides are let down; this is a most important point, as it is well known that one of the greatest difficulties in the treatment of fractured leg is to prevent either inversion or eversion of the foot. The sides and foot-rest are sufficiently high to keep off the pressure of the bed-clothes." (Dr. G. W. McNalty, Lancet, Nov. 8, p. 660.)

JOINTS.—*Mimic Diseases of.*—It is often difficult to distinguish a nervous or neuralgic affection from a real one. "First, as to pain. Alone, it is not to be at all relied on for a sign of inflammation of a joint, especially if it be severe. f a patient be ready to scream when the accused joint is touched, and yet the oint is not over warm and the patient not feverish, you may be nearly sure of neuromimesis; and more nearly still if the pain be rather in the parts outside r about the joint than in the joint itself, so that a light touch is said to hurt as much as a hard one, or a pinching of the skin as much as pressure on the oint itself. You must not even rely on what are supposed to be characterstic pains, such as those felt at the knee for disease of the hip, or about the middle of the arm for disease of the shoulder, or even those grinding and burnng pains at night which some regard as characteristic of ulceration of cartilages. All these may be mimicked." "You may take as a general rule, that acute pain in a joint, if it has existed for even a few days without either local or general increase of temperature, is not a sign of acute inflammation of the joint. It may be rheumatic disease or rheumatic gout, or some slight inflammation after njury, in a neuralgic person, but it is not acute inflammation." But pain often puzzles us. "You may often judge the pain to be mimicry by its inconsistent severity. If with scarcely any other sign of disease a joint will not permit the slightest movement because of pain, you may greatly suspect the reality of disease; but be very careful not to overlook the signs of increased pain on movement in cases of slight real disease, especially in the diseases of the hip in children. You may often find this the only sign making it clear that a child has real hip-disease. A suspected joint may allow free and smooth and painless movement till, for instance, in extreme abduction of the thighs, one adductor becomes much tenser than the other, or in extension the loin is quickly raised, or by some other movement it becomes evident that the joint will not allow extreme movement without pain, although within all but its extreme range its movements may be free and painless. In these, among other cases, chloroform or ether may help in diagnosis. In the real disease, as in the mimic, while the patient is utterly insensible, the joint may be moved as widely as in health, unless indeed there be such changes in its structure as might alone have proved its disease; but commonly you will observe that, in the real disease the muscles become alert, and restrain the movement of the joint before the patient has regained consciousness; while in the mimic disease there is no restraint till consciousness is completely regained. The test is a delicate one, but I think I may be sure that t is a true one, and fit to be relied on, whenever the chief sign of disease of a joint is a restraint of movement on account of pain and the guarding action of muscles. Closely allied with this pain on movement of an inflamed joint is its stiffness, with contraction or other set posture, dependent on muscular action; for this posture, whether it be due to choice or to reflex movement, is the posture of greatest ease, or of safest guard against weight or shock or other causes of pain. The absence, therefore, of the fixed, or nearly fixed, posture usually observed in a diseased joint, may always suggest the suspicion of mimicry. It would be rather strange to see a hip or a knee extended after many weeks of such pain as would be felt in an acute arthritis, unless, indeed, they were rheumatic or gouty, with exaggerated pain, or had been carefully maintained in good position. It would require the presence of many other signs of real disease to counterbalance the absence of this sign; for diseased joints, left to themselves, will be habitually, or always, in the position easiest to the *patient. But the* reverse of this is not true. Very commonly a joint mimicking disease assumes the posture of disease—assumes and maintains it stiffly in even an extreme degree. This may be seen even when there is no objective pain in the joint; but much more, when the joint is a little really painful, as after a blow or strain, the nervous condition of a patient may either make this pain so intense as to demand the position of greatest ease, or may bring about this position, for the relief of even a little pain. Especially the posture of hip-disease is apt to be imitated by the drawing up of one side of the pelvis and rotating it, so that the limb looks shortened." (Sir James Paget, p. 113.)

THE ELASTIC LIGATURE.—Professor Dittel, of Vienna, accidentally discovered that the slight yet continuous pressure of a simple elastic thread causes absorption of the parts pressed upon, so that the thread eats its way into the structures, and this without pain. A girl died with severe brain symptoms, and it was found that the india-rubber band of her hair-net, which she had worn night and day, had ulcerated through the whole thickness of the calvarium, and set up meningitis. Professor Dittel has now performed a large number of operations by means of the elastic thread or band, including five amputations of limbs. The time required for the separation of a ligatured part varies with the amount and density of the tissues which have to be divided; *e.g.*, for the separation of the mamma, from eight to twelve days are required. Among the operations for which the elastic ligature is admirably adapted may be named fistula in ano. (Sir H. Thompson, British Medical Journal, Nov. 29. p. 631.)

AFFECTIONS OF THE SKIN, &c.

ALOPŒCIA.—Cut the few thin hairs on the bared spot as short as possible, then thoroughly cleanse the skin from scruff by washings in warm water without soap. Then let the skin be rubbed for half an hour at a time with a drachm of tincture of phosphorus to the ounce of castor oil. This should be done three times weekly and persevered in for six months. The hair will usually soon begin to grow. (Mr. H. Finch, p. 202.)

CATGUT LIGATURES.—Catgut is an admirable material for ligatures, especially for wounds in which there is reason to expect union by the first intention, and in which it is desirable to avoid any mark, as in wounds of the face. Catgut never causes irritation, nor gives rise to suppuration as does silk, nor like metallic sutures does it give much pain. It is also very readily removed, not tending to tear open a wound during its withdrawal. (Dr. J. C. O. Will, p. 199.)

RINGWORM.—*Oleate of Mercury.*—All that is necessary is to rub gently into the spot or spots a few drops of oleate of mercury (10 per cent strength), and repeat this as necessary. The oleate of mercury possesses many advantages over other remedies. It is both certain in action and produces no staining or injury of the skin. It is also painless in its application. It readily penetrates into the sebaceous glands, hair follicles, and even into the hairs themselves, the mercury being in a state of solution in an oily medium, and it is therefore much more likely to destroy the fungus than the spirituous or aqueous solutions of mercury, &c. This penetrating power of the oleate may be increased by adding a small quantity of ether (one part to eight) to it. (Mr. L. Cane, p. 192.)

SPINA BIFIDA.—*Injection of Iodine.*—Dr. Watt, of Ayr, relates a case of spina bifida thus treated. Three times a small quantity of fluid was removed from the tumour, a week intervening between each operation. At the third operation, half a drachm was injected of a solution containing ten grains of iodine and thirty grains of iodide of potassium in an ounce of glycerine. Nine days afterwards the tumour is reported as being slightly decreased in size. Four weeks from the time of operation the tumour was nearly level with the back, and was replaced by a large half-dried resistant scab. At present a cake of firm condensed tissue occupies the site of the scab, and the child is in good health. This plan is that originally introduced by Dr. James Morton. (Dr. J. R. Watt, p. 201.)

Ulcers of the Leg.—Prepare an ointment consisting of three pounds of finely powdered chalk to two pounds of lard. The lard should be melted, and the chalk added gradually and stirred in. By the use of this ointment to a sore on the leg a crust is formed, first on the surrounding skin, then on the margins, and finally on the surface of the ulcer. It should be spread on lint, and the leg bandaged evenly with domette flannel. The chalk combines with the discharge from the sore, neutralizes its acridity, and forms an artificial scab, under which the healing process goes on rapidly. Suppose a sore is always covered with a thin greenish slough, steep a pledget of "chloralum wool" in equal parts of water and the solution of chlorinated soda (liq. sodæ chloratæ, B. P.); fill up the cavity of the ulcer with the wool, lightly teased out, and cover it with a chalk ointment dressing. Chlorine is much superior to carbolic acid in its cleansing power. (Mr. J. K. Spender, p. 196.)

The sore having been washed clean by the patient, saturate it with a weak solution of carbolic or sulphurous acid. Take some pieces of stout empl. saponis, two inches broad, and long enough to overlap four inches after passing completely round the limb. Pass them one by one through hot water containing a little carbolic acid, and apply them neatly round the limb, from three inches below the diseased surface to about the same distance above. The limb is then to be carefully bandaged from the toes to the knee. Change the strapping about once a week. (Dr. J. G. Black, p. 195.)

AFFECTIONS OF THE EYE.

Amaurosis.—*Strychnia.*—Strychnia administered by the mouth produces quite the same results as when injected hypodermically, but is perhaps not quite so rapid in its action. It is therefore not worth while to give it hypodermically. To obtain all the good which strychnia can produce, it is necessary to keep the system under the full physiological effects of the remedy by administering a dose as large as the patient can bear comfortably. As a rule half a grain in divided doses can be borne by most persons. The intense bitterness of taste may be avoided by giving it in the form of sugar-coated granules of $\frac{1}{30}$, $\frac{1}{20}$, $\frac{1}{15}$, $\frac{1}{10}$ of a grain in each, commencing with the smaller doses. (Dr. J. J. Chisolm, p. 184.)

Cataract.—*New Method of Extracting.*—The following is the plan of operating pursued by Dr. Taylor. After making the corneal section, excise a minute piece of the outer edge of the iris, and separate it with very fine scissors from its marginal attachments to an extent sufficient to permit of the easy exit of the lens through the wound. Lacerate the capsule through the slit in the iris, and extract the lens in the usual way. By this operation the advantages of iridectomy as pursued by Von Graefe are obtained, without the dazzling and defective vision occasioned by the sacrifice of a portion of iris. (Dr. C. B. Taylor, p. 183.)

Iritis.—Upon nothing can we so much rely as on the rapid dilatation of the pupil by belladonna or atropine. The earlier it is applied the better it will act, and its action places the iris at rest and out of harm's way from the effusion of coagulable lymph within the pupil, or between the iris and the lens. Should the eye prove intolerant of atropine, a leech should be applied just below the lachrymal tubercle. It will then generally be found that the eye tolerates its action. During the night, extract of belladonna, rubbed up with a few drops of glycerine, should be freely applied to the brow, especially as at night the disease often makes great progress. The belladonna should be spread on oiled silk to prevent it drying up. Spirit should never be used in the preparation of solutions of sulphate of atropine, as it is unnecessary, and liable to produce great irritation. (Mr. J. Hogg, p. 186.)

Slitting the Punctum Lacrymale and Canaliculus.—A small instrument invented by Mr. Greeenslade gives great facilities for this operation. A director and knife are combined. The director is passed, and then the knife, which was concealed in the handle, is slid along it. The advantages are that one hand is at liberty to steady the eyelid, and that all danger of wounding other structures than those required to be divided is avoided. (Mr. Greenslade, p. 184.)

MIDWIFERY, ETC.

CANCER OF THE UTERUS.—Our best means of treating cancer of the uterus are the administration of ergot internally and the application of caustics. The ergot should be given twice a day for several months. It possesses the power of producing uterine atrophy. The organ is kept contracting, and the blood supply is diminished. The use of caustics should be combined with this. The best, that is, the most destructive, and at the same time most manageable, are the chloride of zinc, the dried sulphate of zinc, and the nitrate of copper. The dried sulphate of zinc should be applied in considerable quantity, so as to produce a slough, the vagina being protected by cotton wadding, tipped at the uterine end with olive oil. The nitrate of copper is most suitable for injection a little way into the uterus. A healthy healing sore may not unfrequently be obtained by these means. (Dr. A. Milne, Edin. Med. Journal, May, p. 1037.)

EXCISION OF INTERNAL HEMORRHOIDS IN THE FEMALE.—Excision of internal hemorrhoids is preferable in every respect to ligation, yet is attended with great risks from hemorrhage. In the female, however, the lower part of the rectum may be everted from the vagina, and the parts seen, and thus any hemorrhage can be at once arrested by the application of some astringent or caustic. (Dr. H. R. Storer, p. 244.)

FEMALE RECTUM.—*An Improved Method of Examining.*—The anus and lower part of the rectum of the female may be completely everted by pressure from within the vagina. The patient being upon her side, introduce the finger into the vagina a sufficient distance, and then press steadily backwards and downwards upon the sphincter ani. If there is spasmodic contraction of this muscle, it should be ruptured by rapid dilatation with the thumbs, as suggested by Récamier for the treatment of anal fissure. (Dr. H. R. Storer, p. 244.)

Visual Exploration of the Upper Rectum.—First forcibly and rapidly dilate the sphincter ani with the two thumbs, so as to paralyse or rupture it; then introduce a vaginal bivalve speculum. It will be found that the visual exploration of the upper rectum is perfectly practicable, and to a much greater extent than can be accomplished by the endoscope. (Dr. H. R. Storer, p. 244.)

Duck-Bill Speculum for the Rectum.—A small duck-bill speculum is infinitely superior to the common speculum ani in affording light and space in operations on the rectum. If necessary, the sphincter ani and rectum may be slit towards the coccyx with a bistoury, to afford more room. (Dr. Demarquay, p. 245.)

INDUCTION OF PREMATURE LABOUR BY ELECTRICITY.—The induction of premature labour may be easily and safely effected in the following manner:—Pass within the os a metallic sound covered, except at the point, with a non-conducting material; this is made to form one pole of a galvanic machine, the other being placed upon the abdomen. A current of electricity, continuous or interrupted, is to be passed through the parts for about ten minutes, and the process repeated daily until the expulsive action of the uterus is induced. A little dilatation of the os is generally the first effect, and it is well then to substitute a larger conical point. Gaiffe's battery is so exceedingly portable that it is very suitable for medical purposes. (Mr. J. Varley, Dr. B. R. Morris, p. 211.)

INERTIA OF THE UTERUS.—*Electricity.*—There are cases in which the uterus having acted for some hours, and the labour being advanced to a greater or less extent, the pains cease and cannot be brought on again by ergot, friction, or anything else. In such a case the passage of a current of electricity brought on a moderate pain, the first for eleven hours, and the pain continued to increase until the current was interrupted. The head descended to the brim. After waiting about ten minutes the current was re-applied for three minutes, which had the effect of bringing the head into the brim. By the next pain the head was born, exactly twenty-three minutes from the commencement of the application. Maw's self-acting electro-magnetic machine is the best for use in obstetric practice, as it is difficult to get a female attendant to work a machine steadily. One of the electrodes should be placed on the abdomen, and the other on the perineum. The best abdominal electrode is a flat piece of metal, the size of the two hands opened, and curved to fit the transverse abdominal curve. This is to be retained by the patient's hands, a layer of blanket being interposed. (Dr. A. E. M'Rae, p. 218.)

Mr. Storrs, of Doncaster, relates a case in which injection of perchloride of iron, external pressure, kneading the uterus, cold affusion externally, and injection of cold water per vaginam produced no effect. A Stöhrer's portable coil machine was procured, and one pole placed on the abdominal walls and the other within the cervix uteri. Contraction at once ensued, and complete arrest of the hemorrhage. (Mr. R. Storrs, p. 235.)

INTERSTITIAL FIBROUS TUMOURS OF THE UTERUS.—It is of the utmost importance to determine whether any given interstitial fibrous tumour is nearer the mucous or the peritoneal coat of the uterus, because the prospect of successful treatment is far greater in the first than in the second case. If the size of the uterine cavity, as measured by the sound, bears a fair proportion to the size of the tumour, we may expect to find the tumour nearer the mucous surface, and consequently the case would be a promising one for enucleation. (Dr. A. Meadows, p. 250.)

It has long been known that if the capsule of a fibrous tumour is divided, the growth is gradually forced through the aperture by the contraction of the uterine fibres expanded over the rest of the surface. Dr. Greenhalgh, in a case of this kind, forced a small conically bulbous cautery into the tumour, behind the posterior lip of the os uteri, and through the upper wall of the vagina. In a week the opening thus made had enlarged to the size of a crown piece, and through it the tumour was beginning to protrude. In four or five days more the mass came to occupy the whole vaginal cavity, and was partly removed by the écraseur. Ultimately the entire mass was turned out by the finger, and a good recovery made. (Dr. A. Meadows, p. 251.)

LABOUR WITH CONTRACTED PELVIS.—It is exceedingly doubtful if the operation of inducing premature labour ought ever to be employed in cases of contracted pelvis. When the labour actually comes on, we ought first to see what nature is likely to accomplish unaided, without allowing the mother to run unnecessary risk. If artificial aid is found to be necessary, the long forceps should then be applied, and if delivery is found to be impossible it must be effected by cephalotripsy or craniotomy. (Dr. A. Macdonald, p. 212.)

NEW MIDWIFERY FORCEPS.—Dr. Lowenthal, of New York, describes a midwifery forceps used by him, which possesses some peculiarities worthy of note. Like those described by ourselves (Eds. of *Retrospect*) in 1869, the principle of it is the passage of the two blades, as one, into the hollow of the sacrum. For this purpose the convexity of one blade fits into the concavity of the other. When introduced the blades are separated, and are glided round the head of the child in opposite directions. The difference between the two instruments is that, whereas in our instrument the blades and handles are completely separable, in this instrument they are joined at the lock, which at the same time is so constructed as to admit of the blades revolving round the child's head. (Dr. Lowenthal, New York Medical Record, Feb. 15, p. 94.)

ORIFICES OF THE UNIMPREGNATED UTERUS.—The os externum is surrounded by circular muscular fibres which perform the part of a sphincter, and the same is the case with the os internum. These sphincters open for menstruation, probably for conception; and the internal one closes on the contact of a foreign body, such as the uterine sound. The point of this instrument is generally arrested when it reaches the os internum. Sometimes a slight pressure will pass it into the uterine cavity, sometimes it will not, and nothing short of unjustifiable force will succeed; although in these same cases a small wax bougie, warmed and bent, will generally pass. Resistance to the sound at the os internum is a sign of a healthy natural vital state of the parts. It is necessary to establish these facts, as otherwise any healthy woman may be pronounced pathologically wrong, and treated for strictures which do not exist. (Dr. J. H. Bennet, p. 204.)

OVARIAN DROPSY.—*Diagnosis.*—It is generally supposed that there is no more conclusive diagnostic test of ovarian dropsy than the linseed-tea looking liquid which may be drawn off by an exploratory puncture. Mr. Hutchinson, however, relates a case in which this proved fallacious, and which turned out to be extra-uterine gestation. The physical characters of the disease exactly resembled those of ovarian pregnancy, except that both loins were resonant. In ovarian dropsy one loin is dull on percussion. To avoid such an error in future, Mr. Hutchinson suggests that the patient should be made to support herself on her hands and knees, when the fœtus, if present, will fall to the lowest part and can be recognised by the surgeon as a heavy solid body. (Mr. J. Hutchinson, p. 236.)

PERSISTENCE OF THE HYMEN.—A number of cases are related by Dr. Gray, of Glasgow, which prove that persistence of the hymen is compatible with the wedded state, that destruction of it does not necessarily follow even the calling of a prostitute, and hence, that its persistence after attempted rape must not be relied upon solely as evidence sufficient to disprove the charge. (Dr. J. St. C. Gray, p. 261.)

POST-PARTUM HEMORRHAGE.—*Anticipation of.*—When the pains during labour are strong and quick, ceasing almost suddenly, and the intervals between them long in proportion to the length of the pains, post-partum hemorrhage is almost sure to occur. The uterus in these cases contracts sharply, and then becomes fully relaxed, and the same tendency continues after the birth of the child. Being able in this way to foretell the occurrence of flooding, it becomes easy to prevent it by giving ergot at the proper period before the birth of the child, so as to alter the character of the pains and make them longer, and the intervals between them shorter. (Dr. E. Whittle, p. 228.)

Injection of Perchloride of Iron.—There are cases in which the hemorrhage after removal of the placenta cannot be arrested by either pressure or cold, nor yet by the administration of ergot. It is in these cases that the injection of a solution of perchloride of iron should be tried. Six ounces to the pint of water was the strength of the solution used in a case related by Dr. Thomas Chambers. The effect was not produced in the way we should have imagined; it was not to cause coagulation of the blood at the orifices of the uterine veins, the uterus being not firmly contracted, but to cause contraction of the organ and so arrest the hemorrhage in the natural manner. (Dr. T. Chambers, p. 231.)

The result of experience in the use of perchloride of iron injections is very favourable. Instead of being obliged to grasp the uterus for an hour or two, and feeling afraid to leave the patient perhaps for many hours, the use of the iron solution at once removes all doubt and difficulty, by inducing firm and permanent contraction. Before injecting the fluid into the uterus, all coagula or remaining portions of placenta structure must be carefully removed. (Dr. Steele, p. 233.)

PUERPERAL CONVULSIONS.—*Chloral.*—The introduction of chloral appears to have quite revolutionized the treatment of puerperal convulsions. Whereas previously we had nothing worth relying upon, we now have an agent which, if we can once get it introduced in the proper dose into the circulation, is almost certain to prevent a recurrence of the attacks. A very interesting case is related. (Dr. A. D. Mackay, p. 227.)

Enemata of Chloral.—In a case of puerperal convulsions in which no remedy could be given by the mouth, a drachm of chloral hydrate was given in two enemata. After the second no more convulsions occurred, and quiet sleep followed. (Dr. J. Barclay, p. 227.)

RUPTURE OF THE PERINEUM.—When the perineum is ruptured into the rectum, take an ordinary good large curved needle and pass it in deeply on each side of the ruptured perineum, going well down to the bowel, or even into it. Use a good piece of thick wire, twist the ends together over the front, and get the parts drawn well together. These sutures can be left in much longer than quilled ones, and with less damage to the skin and surrounding parts. Then pass the finger into the rectum, and with a straight, blunt-pointed bistoury divide the sphincter on both sides to ensure freedom from movement of the torn edges of the wound. (Mr. C. Heath, p. 242.)

SUPPOSED STRICTURE OF THE CERVIX UTERI.—The treatment of dysmenorrhœa arising from contraction or stricture of the internal os is both unscientific and unsuccessful. The stricture in most cases is merely a congestive swelling of the lining membrane of the part, and although temporarily relieved by the hemorrhage produced, rapidly returns, and any relief obtained is merely temporary. Quite as good results and better in the long run, are obtained by scarification of the os and the passage of graduated bougies. By scarification, congestion of the lining membrane of the womb and of the ovaries is relieved, and the passages are relaxed. By this means the bougie is more easily introduced and does not rouse or increase irritation of the canal and interior of the uterus. (Dr. W. Cumming, p. 205.)

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THE

RETROSPECT

OF

RACTICAL MEDICINE AND SURGERY,

BEING

A Half-Yearly Journal,

NTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M.D.,

TE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN AT THE LEEDS SCHOOL OF MEDICINE, ETC.,

AND

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PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

RT. 1.—ON TISSUE-DESTRUCTION IN THE FEBRILE STATE, AND ITS RELATIONS TO TREATMENT.

By Dr. Francis E. Anstie, Physician to Westminster Hospital.

[It has hitherto been considered a maxim in pathology that the persistence of the febrile state at a certain high standard necessarily involves a large consumption of the nitrogenous tissues of the body. This idea is founded upon the fact of the large amount of urea passed in urine, notwithstanding the diminution or even entire cessation of nitrogenous food supplies.]

Now the extended inquiry which I have been lately carrying on has shown, in a very surprising manner, the possibility of a total absence of urea-increase uring the course of an intense pyrexia of considerable duration. That is the road fact, in the natural history of the state, which has come out: I now proeed to give details.

Before entering into the question now raised, it is needful to have distinct eas of what constitutes a genuine urea-excess. The first thing necessary to now is, what is the average urea-discharge of health.

Now it is obvious, in the first place, that the amount of urea will vary enorously according to the character of the diet. It would be impossible otherise to account for the exceedingly various estimates of the health-range of rea-discharge which have been given by excellent observers. In Parkes's ork on the urine, twenty-four authors are quoted whose estimates were all ade on males between the ages of twenty and forty; the figures vary from 36·1 to 688·4 grains for the twenty-four hours. The mean of the whole gives estimate of 512·4 grains urea-discharge daily. But in Senator's recent and ost valuable work on pyrexia I find, with surprise, that he quotes a stateent of Unruh, which would make the normal discharge in men as low as 270 ains (17·466 grammes) in twenty-four hours. It is quite certain, however, at unless the individuals from whom this was taken were feeding on a diet usually poor in nitrogen, some abnormal circumstance must have been prent.

The second point to be remembered is that the concurrent results obtained many observers show that in healthy adult women the daily urea-discharge es not exceed three-fourths of the quantity excreted by healthy adult men milarly circumstanced. According to our modern physiological ideas, it ould be easy to account for the whole of this difference by the inferior quanty of azotised food consumed by females.

The third point to be remembered is, that in health the urea-discharge is the sult, in by far the largest extent, of the decomposition of nitrogenous food ithin the body.

In the febrile state, on the contrary, there is usually such complete anorexia at the supplies of nitrogenous matter taken are very small, yet the urea-disarge is commonly far higher than in health; sometimes double the amount, more. Not only in case of a positive increase of urea under these circumances, but even when it sinks some way below the normal level, we may still nclude (from the small nitrogenous food supply) that exaggerated tissue destruction has largely to answer for its formation. The question is, can the urea-discharge be so far depressed below the normal rate, in any considerable number of cases of decided pyrexia, as to indicate decidedly that nitrogenous-tissue destruction is taking but little part in the actual urea-formation? The cases which will be brought forward in the present paper certainly tend, as far as they go, to answer that question in the affirmative.

Typhoid Fever.—In the case of this disease a large body of evidence has been collected, of late years, which appeared to show, with great uniformity, that when the fever was at all severe (as measured by continuously high temperatures, together with any marked delirium, the urea-discharge was constantly above the normal level, at a time when a very small amount of azotised food was being taken. For example, Dr. Parkes, besides speaking very definitely, from his own experience, in favour of a regular increase (save in certain abnormal conditions) of urea beyond the normal level (one-fifth on the average), also quotes three important series of observations, by Moos, Brattler, and Warnecke, extending altogether over 78 cases. The significance of the figures will not be perceived unless we remember that the patients were on exceedingly low diet—lower than we should employ in England. Moos (from eighteen cases) gives the average daily discharge as 575·64 grains in first week, 517·92 grains in second week, 404·04 grains in third week, 343·2 grains in fourth week. Warnecke gives, from results of fifty cases (thirty males and twenty females), for *men* an average discharge of 673·92 grains in first week, 617·44 grains in second week, 482·04 grains in third week, and 361·92 in fourth week; for *women*, 530·4 grains in first week, 471·12 in second week, 375·96 grains in third week, 319·8 grains in fourth week. Brattler gives (from ten cases) 607·4 grains daily in first week, 592·8 grains in second week, 421·2 grains in third week, 327·6 grains in fourth week, 247·6 grains in fifth week. The near approximation of these different series of results is very remarkable. Nevertheless, Parkes himself points out some objections to their conclusiveness; and my own results will be found to fully justify a revision of the usual belief. That is to say, they will show that the exceptions to the rule are probably so numerous as to form an important subject for consideration in regard to fever doctrines and fever treatment. The histories now to be given are of patients in whom neither stimulants, nor any important medicine were given during the observed period.

The first case which I shall relate is that of Charles Jefferson, aged 25, a small but well-nourished man, who was admitted into Westminster Hospital on the seventh day of typhoid fever, and upon whom the first rose spot was seen on the following day. The examination of the urine unfortunately could not begin till the ninth day. At this time there was no diarrhœa: but I may mention here that even at the period when diarrhœa did come on we did not lose urine from that cause, as the patient intelligently obeyed our orders to pass water into the chamber-vessel before going to stool. This circumstance shows, of course, that the case was not of the severest type. Full convalescence was established upon the twenty-fourth day, as marked by the cessation of diarrhœa; the temperature had become normal two days earlier. I had intended to print in diagrammatic form the respective curves of mean daily temperature, of urea elimination, and of water elimination, but there was unfortunately no time for this on the present occasion, so the figures must be verbally stated. On the ninth day the temperature, though not quite so high as it had been, was nearly 103° on the mean of three observations, the maximum being a good deal higher: in striking contrast with this was the fact that only 118 grains of urea were eliminated. During the five following days the temperature had steadily come down and the urea had as steadily gone up; the twenty-four hours' mean temperature on the fifteenth day stood at 100·5, while the urea had risen to 385 grains. Here, unluckily, the patient became mutinous; for although he had diarrhœa, and slight delirium every night, he felt so well and hungry in the day-time, that he was greatly disgusted with his rigid diet. I had kept him strictly upon milk alone—80 ounces daily; and except a very little lemonade, and the usual dilute hydrochloric acid mixture, nothing else was allowed to pass his lips. At this point he became so clamorous that he *would* probably have left the house had we not gratified him; but to do so with safety we had to make the diet so complex that it would have been impossible to estimate its value by means of analysis. Accordingly, we did not estimate the urea during the next three days; but so far as the volume (judged by the eye merely) of urine and its colour could afford a hint, it did not seem likely that any very material change in the urea elimination had taken place during this time. After this three days' interval we resumed the diet of 80 ounces of milk only, and our first twenty-four hours' estimation gave only 316 grains urea. Next day (same diet) the urea fell to 284 grains: the mean temperature was now 99°·6. On the day following (twenty-first) there was a sudden increase in the volume of the urine (from 550 c.c. to 2,000 c.c.), and the urea-reading was 717 grains: but herewith a remark is necessary. No corrections had been made, at any point in the case, for chloride of sodium; for on the one hand it was impossible for me to give the time for *exact* estimations of the chlorides (having a large number of urines of acute diseases to examine every day), and, on the other hand, the rough formula given by Liebig for chlorides becomes worse than useless where there may be great critical variations. Throughout the decidedly febrile period of Jefferson's illness there could be little doubt, from the very moderate level of the figures representing the total effect of the nitrate of mercury test, that the chlorides were in great part either retained or eliminated in the stools, as is well known to be usually the case. But on the cessation of fever there is usually a sudden and rather considerable appearance of chlorides in the urine; and part of the 717 grains that read as urea in my table in all probability really represented $Na\,Cl$, especially as the diarrhœa was declining. Respecting the diarrhœa and its possible influence on the urea-discharge, it may be remarked, first, that during the early period (three days) of *lowest* urea elimination there was no diarrhœa, indeed no action of the bowels at all: and secondly, it has been long ago shown by Parkes that the diarrhœa of typhoid fever, though it does favour the escape of chlorides, is not to any considerable extent a channel for nitrogen-escape.

The volume of water steadily fell again (though no accurate measurements were taken) during the next two days. On the twenty-fourth day the volume was ascertained to be 1110 c.c. (39·6 ozs.), and the urea was (again without correction for chlorides) 383 grains. On the day following it was only 223 grains (also without correction for chlorides). At present he is on a mixed diet, approaching as fast as he safely can to the food of a healthy man. As soon as he shall have become fairly established on that, I will report on the amount of his normal urea-discharge. He is a hearty eater when in health.

The broad features of the above case are: (1) That the water elimination and the urea-discharge closely coincided in amount, but that they ran a *precisely opposite course to that of the mean temperature* during the whole period between the ninth and the fourteenth day. [It will be seen, hereafter, that even the water and the urea by no means always correspond in amount.] (2) That the average urea-level was exceedingly low, for it might well have been expected (according to former estimates) that such a patient would have excreted a daily average of 500 grains during the second week of typhoid, whereas he actually passed only 300 on the average. (3) I may now add that the nitrogenous matters introduced as food (in 80 ozs. milk) proved on analysis to yield 113·5 grains nitrogen, or the equivalent of 227 grains urea: so that it will be seen that but a small portion of the urea-discharge needs to be accounted for by destruction of proteinous tissues. (4) In correspondence with the slight degree of muscular destruction which took place in this case was the remarkably small degree in which muscular strength was affected. This was repeatedly pointed out by me to the students; and the same fact has been noted in several other cases.

Just before this case occurred, another patient, Mary Hughes (aged 32, single), had also been in Westminster Hospital suffering from typhoid fever. The past history of the case was very confused. It was impossible to say whether, at her admission, she was already in the fourth week of typhoid fever, whether she had had a "relapse," or whether she had passed into typhoid after suffering from some other febrile affection. At any rate she was in a condition of very high pyrexia, the temperature fluctuating between

104° and 105,° or even more for the first two days, and crops of well-marked rose spots appeared daily for five or six days after her entrance to the Hospital. She had no diarrhœa at any time; but this is a feature which has been exceedingly common for some months past, even in some of the best-marked cases of typhoid at Westminster Hospital, and, I believe, elsewhere. She had no delirium, no appearance of anxiety, and although she lay in bed her muscular strength seemed scarcely at all affected. It was curious to see this in a patient the dry burning heat of whose skin would have led one to expect a very profound degree of prostration. From January 24th to February 2nd inclusive (nine days), the total nitrogen of the urine was daily most carefully estimated by my friend Mr. Frederick Hicks (formerly assistant to Professor Sir Benjamin Brodie), by the soda-lime combustion process as modified by Parkes. On several of these days I myself examined, for urea only, by Liebig's method; and the difference between our figures was only so large as corresponded well enough with the probable amount of nitrogen contained in the excreted lithates, uric acid, creatine, creatinine, &c. The diet was rigidly confined to 60 oz. milk (containing a little over 85 grains nitrogen) during the first two days, for the other seven it consisted of 40 oz. milk, and 20 oz. best beef tea (containing nearly 120 grains nitrogen). The nitrogen discharge was as follows:—105 gr., 115 gr., 149 gr., 103 gr., 87 gr., 116 gr., 77 gr., 44 gr., 170 gr. As it has been suggested to me by one or two friends that cases with a low urea-discharge may perhaps void a large amount of nitrogen through the skin, I may remark that at any rate this patient did not perspire *fluid* at all considerably, nor did the man Jefferson.

The daily volume of water eliminated by the woman Hughes corresponded somewhat, but not closely, with the fluctuations of the urea. The figures (in cubic centimetres) are: 512·5, 570, 590, 500, 710, 635, 700, 700, 980. From this it can only be said in general terms that both urea and water were low; but it will be seen that, *e.g.*, while on the seventh and eighth days of observation the volume of water was exactly the same, the nitrogen was 77 grains on the former and 44 on the latter day: and many other discrepancies could be pointed out. The above nine days were the only ones on which the total nitrogen was estimated; but for three or four days longer (I should mention that temperature became normal two days after the last combustion-research) I watched the urine, by Liebig's process, on the chance of a great rush of urea and water elimination, but nothing of the sort occurred. The urea slowly rose up to what appeared (on mixed diet) to be about the woman's normal discharge, viz., 350 grains. This being probably her standard in health, it is very remarkable to observe the low ratio of elimination during the nine days first mentioned, and especially during the first four of them, when the temperature was *never below* 103° at any part of the twenty-four hours. During these four days the urea-discharge did not average more than 238 grains; while the ingoing nitrogen averaged 102·5 grains, equivalent to 205 grains of urea. And it is interesting to observe that this evidence of a but slight destruction of the azotised tissues is fully in agreement with the woman's remarkable conservation of muscular power.

It will immediately occur to some, as a criticism on the above case, that the urea-discharge might well be low, seeing that the patient had been continuously or nearly continuously febrile for a month when our observations began, and the available albumen of the blood and tissues might have been used up by a copious urea-discharge in earlier stages of the disease. But it seems difficult to suppose that anything of the kind had taken place; for in such case there must have been extreme muscular weakness, whereas the very reverse was the fact. And indeed, supposing that we regard the above as a case of typhoid really at the end of the fourth week at the time of admission, the best evidence hitherto obtained (including Parkes's) goes to show that when the fever keeps up at this late stage the urea-discharge keeps up also. The remark may also be made, in anticipation of a similar criticism on Jefferson's case (in which the observations were not commenced till the ninth day), that his entire freedom from muscular prostration was inconsistent with the idea that there had been any such enormous combustion during the first week as might have temporarily used up the nitrogenous matters available for elimination. Since writing the above I have analysed Jefferson's urine, first day of *full diet*:—Chlorides, normal; urea, 503·88 grains.—*Practitioner, March, 1874, p.* 193.

2.—JURGENSEN ON THE SLIGHTER FORMS OF TYPHOID FEVER.

By the Editor of the Medical Times and Gazette.

Typhoid fever must be a subject of interest to the medical profession for many years to come, until its history is more fully worked out than at present, and we are able to say with absolute certainty what is and what is not typhoid. It is still a question whether many of the cases now returned as simple continued fever are not in reality typhoid; and we cannot but be indebted to all those who, by their observations, aid in settling this important question. There can be little doubt that a form of typhoid fever which, from its mildness, does small harm to the patient himself, is still quite as dangerous, *quoad* infectiveness, to the community at large as one labouring under a more severe variety of the disease; and it certainly makes all the difference to the patient himself, whether his doctor recognises the nature of his illness or not, and treats him accordingly. The philosophical and, in many ways, original lecture of Professor Jürgensen, of Tübingen, on the slighter forms of typhoid (Volkmann's *Sammlung Klinischer Vorträge*, No. 61), of which the following is but a brief abstract, will therefore be doubtless acceptable to most of our readers:—

There are several reasons why the slighter forms of typhoid fever have not received the attention they deserve. In the first place, medical men are too apt to consider the representative descriptions of a disease given in books as portraying actual forms of that disease, whereas they only exhibit at one view the aggregated characteristics of many varieties of it. Thus, when they find a case which does not fit into the typical description, they disregard it altogether, to the manifest detriment of the patient.

Again, slight forms of fever may not come under the physician's notice at all. They are called by the patient's friends "rheumatic" or "catarrhal," and receive domestic treatment.

Thirdly, many abortive forms of disease can only be diagnosed from *etiological* considerations, and to do this a physician must see many patients. Now, during large epidemics his time is necessarily devoted to the severer cases, as every man's strength has certain limits. Moreover, mild cases as a rule do not seek admission into hospitals.

Professor Jürgensen lays down the following dictum, which must be accepted if our present views about the acute exanthemata be correct—namely, that in each of them, since each arises from a specific cause, we must be able to find cases which shall represent every stage of intensity of that disease from the slightest to the most severe forms. By a specific cause is meant a something (*Etwas*) with properties peculiar to itself, which occur in it alone in all nature. This "something" produces, by its entrance into man's organism, certain peculiar and sharply defined anatomical and functional phenomena in individual organs and in the general processes of nutrition (*Stoffwechsel*).

There are three categories of infectious diseases—(1) those in which the person attacked can by contact with another produce in him the same disease in turn—*e.g.*, the acute exanthemata; (2) those in which a carrier of the infectious principle is required for it to enter the body from without—*e.g.*, typhoid fever and cholera; (3) those which require the aid of factors external to man's organism in order to become efficient—*e.g.*, malarial affections.

Now, the conditions of the existence of an infectious disease are that the "something" (*Etwas*) spoken of above shall meet with an organism in which mutual action and reaction (*Wechselwirkung*) can take place. The exact form (*Erscheinungsform*) in which the disease then manifests itself depends clearly on two factors—the intensity of the exciting poison, and the resisting power of the host,—and both vary very widely; so that these considerations alone would lead us to expect all grades of severity of such a disease. The patient himself will be the gainer by this view, for when the physician comes to consider the sick man's organism and its resisting power as one of the two factors which he must keep in view, he will be less likely to forget that he has a diseased person, and not the entity called disease, to treat (*einen Kranken, keine Krankheit zu behandeln hat*).

When two bodies, mathematically speaking, react on one another—*cæteris paribus*,—the effect produced depends on their relative size. We must therefore inquire whether the *intensity* of an infectious disease varies with the *quantity* of the poison received into the system or not, and so whether the existence of slight forms of the disease means the absorption of small amounts of the infecting material.

We can adduce several arguments in favour of such a view. Thus, epidemics vary in intensity as well as in extensiveness, and we may say in general terms that the most extensive epidemics are equally the most intense. Or (2) take an individual epidemic. The worst cases occur when it is at its height; and is this not most simply explained by the presence at the time of a large quantity of the poison? In the latter case objection may be made that in zymotic diseases there is a multiplication of the poisonous element in the organism itself; but the objection is less trustworthy than it at first sight appears. Jürgensen and Körner have proved that pus from a soft chancre can be diluted with successive additions of an indifferent liquid (*e.g.*, serum), so that, in inoculations with it, not only is the incubative period gradually lengthened, but the size of the resulting pustule diminished until it cannot with certainty be diagnosed as chancroid. Here the result is probably due to tissue changes, by which some of the germs are destroyed, for Ludemar Hermann has especially pointed out that the effectiveness (*Wirksamkeit*) of a poison depends on its concentration in the blood, and this is being continually lessened by the processes of excretion from the blood.

Some persons may consider that the cause of difference between the intensity of different examples of the same disease lies in a change in the *quality* of the poison received. It has been decisively proved that vaccine matter loses its power when transmitted through many generations of men, probably from the destruction of some of its germs through tissue-change; or else it may be that while the number of the germs remains the same, their properties have altered through time. Whatever view they take, all persons must admit that at any rate *a greater or less amount* of the poison can act so as to produce disease; so that this alone would account for its slighter forms, even if the resisting power of the receiving organism were to remain the same. The question next arises, Is the *resisting power* of different individuals really the same? and in answering it we must clearly distinguish between the *receptiveness* (*Empfänglicheit*) of an organism and its resisting power to the poison when received. *Receptiveness* to chancre-poison, for example, will depend on whether the skin of the penis is thick or thin, whole or excoriated, in two individuals; but the poison once absorbed, the one with the thicker skin may have severer symptoms than the other. This may explain the fact that some people hold out longer against the invasion of a disease than others, and also why cholera often follows dietetic errors. *Resistance* to disease must be broken up into two factors—(1) general resisting power—*i.e.*, a good constitution, about which we can make no certain prediction prior to experience; (2) resisting power to certain toxic substances, a factor of excessive variability in different individuals. This is shown by the effects of alcohol, opium, and the alkaloids, and is true, also, of the class of infectious diseases which most people only suffer from once in their life, though a few may have them twice or more. For instance, Jürgensen knows a person who has had scarlet fever four times, the two last times when the disease was only sporadic. On the other hand, how many escape such diseases—for example, scarlet fever—altogether; and what would have happened at the time of the Plague if every one had had it? So, then, since we find such variations in the grade of intensity of the poison, and in the receptiveness of the individual, we must admit that all gradations of intensity of the disease from zero to the maximum are possible, and to be expected

We can now turn to the phenomena of the slighter forms of typhoid fever as Jürgensen describes them, and see in what they differ from the ordinary forms. It was Griesinger who first called attention to such cases, and showed that it is their *short duration*, and not the slightness of a typhoid fever running its normal course, which characterises them. Although his facts have not been doubted, yet they have not received the assent they deserve. Jürgensen has founded his present statements on the observation of more than a hundred cases during three epidemics in and about Kiel in 1865, 1866, and 1868. In the last epidemic, twenty-nine persons connected with the academical hospital were attacked, and one died and was found to have all the anatomical characteristics of typhoid fever. There was a close resemblance between all the cases. In the first place, the onset of the disease is usually sudden (seventy-four times in eighty-seven), and in half the cases is accompanied by a pretty well-marked rigor. There is headache, while the temperature rises quickly to its maximum—sometimes to 40° Cent. (104° Fahr.) in thirty-four hours,—and not in the usual zigzag fashion. At the height of the disease morning remissions of 1° to 1½° and nocturnal exacerbations occur, just as in the ordinary severe forms, only the remissions are slighter. The absolute height of the temperature varies considerably, and the influence on it of quinine and of cold applications is relatively great. After cold baths there are often very decided irregularities in it—*e.g.*, higher morning than evening readings. In the second stage, a high temperature with only slight remissions is not so very uncommon, the thermometer standing above 40° Cent. for several days. The fever-curve of the slightest forms of typhoid exhibits the influence of insufficient infection in all their stages "by the prevalence of the law which governs the normal temperature over that called into operation by the typhoid poison." The usual steep curves of the third stage (high evening and normal morning temperatures) fail entirely in the slight forms, and the temperature generally falls gradually in from twenty-four to seventy-two hours to its normal level.

Swelling of the spleen was present in eighty-one out of eighty-eight cases. Roseola is often absent in the very short cases (ten days); but generally present in those lasting two weeks or more, and occurs about the fourth or fifth day; and as a general rule, the more intense the rash, the more severe the disease. Bronchial catarrh seldom occurs, and diarrhœa was only present in 16 per cent. of Jürgensen's cases, while perforation and hemorrhage from the bowels were not observed once. Albumen is often found in the urine early, especially in the cases in which the fever runs high. The period of convalescence is relatively a long one, and is accomplished by great muscular weakness, which may even pass on to temporary paralysis if the muscles be at all overstrained. Relapses are rather of frequent occurrence, though Jürgensen refers them all to errors in diet on the patient's part, as he found a definite connexion between the two in all his cases. He believed that the so-called "*Typhus ambulatorius*" of books is only a slight attack of typhoid fever, prolonged and exacerbated by dietetic errors, and he therefore insists even more strongly on absolute diet in the slighter than in the ordinary well-marked forms of the disease. No patient under his care is allowed to take solid food until the temperature in the rectum has not, for at least six days, reached 38° Cent., four readings being taken daily. Jürgensen's explanation of the typoid relapses after improper food is that in such patients the intestine does not at first offer a suitable nursery (*Boden*) for the increase of the fever poison, but requires an external irritation—*i.e.*, solid particles of undigested food—to fit it for reproduction. Therapeutically, in addition to absolute diet and rest in bed, Prof. Jürgensen makes use of quinine and cold baths in such cases as there seems danger from the fever process itself.

The same season of the year (August to November), and the same period of life (fifteen to thirty years of age), which furnish the greatest percentage of severe typhoid cases, also give the greatest percentage of the slighter.—*Med. Times and Gazette, Feb.* 14, 1874, *p.* 188.

3.—ON THE ANTIPYRETIC ACTION OF QUININE.

By] Dr. Clifford Allbutt, Physician to the Leeds General Infirmary.

[Fever is a disturbed balance, and like other disturbed balances of a moving equilibrium, it tends under favourable circumstances to resolution. Many fevers arise from a single interference, which is not repeated, and the initial velocity and the subsequent course of the disturbance depend upon the character of this single interference. It is a general rule that disturbances having a high initial velocity are sooner expended than those in which it is lower.]

This is not the time to sum up carefully what is known of the effects of pyrexia when thus considered, but I may shortly say that they fall into two chief categories, those seen in discharges, or in oppression of the nerve-centres, and those seen in greater combustion of tissues. This excessive combustion is strongly marked in the muscular tissues, not excluding the heart. Shortly, then, violent or prolonged pyrexia threatens life through the nervous system and through the heart. The degree of violent pyrexia which will kill is tolerably well known, but clinical experience has yet to decide what averages of daily aberration are compatible with the continuance of life for a week, a fortnight, a month, and so on. This ought to be made out. It is certainly true, however, that small gains in daily temperature are very large gains in time, and that as we descend the scale in daily temperature, we get largely multiplied increments of immunity. Thus an aberration of 107° is soon fatal, an aberration of 106° is alarming, an aberration of 105° with slight remittances is tolerable for some days, an aberration of 104°, with similar slight remittances, may be borne not only longer but very much longer, and aberrations of 103° may be borne for a month or more. If there be more oscillation, if, say, the daily temperature fluctuate between 100° or 101° and 103°, the combustion may be tolerated for very long periods, certainly for ten weeks, and perhaps for more than that time. But we need far more careful estimates of this, and such estimates must be made on so large a number of similar patients as to eliminate, as far as possible, the error of varying individual resistance.

Does an artificial diminution of the degree of the fever on one or on several days modify the after-course of the fever either in its duration or in its average intensity? Observers seem fairly agreed that in fevers which have a definite course it does not so modify it, and in other fevers it is hard to form a judgment. In the use of any antipyretic agent we have therefore to expect a temporary alleviation only. We cannot shorten the period of the whole aberration, nor can we flatten its trajectory, so to speak, except by a continuous application of our means. We know tolerably well, then, what our aims must be, namely, to stop superheating in the diseases in which this may occur, and in fevers of long periods to flatten the curve so as to bring it within those parallels which experience tells us are safe for its calculated duration.

Let us now endeavour to apply these reasonings. In typhus, scarlatina, pneumonia, and rheumatic fever, the pyrexial danger is generally of the brief and intense variety; in typhoid it is more often seen in the longer continuance of a more moderate daily average; in septic fevers it is seen in aberrations which although often high, yet so alternate with much lower degrees as to lessen the mean daily temperature, and thus to make them very tolerable to the system. A septic fever, again, is very often added to a specific fever, and may thus either modify its course or may prolong the pyrexia when the more definite movement is expended. Such an event is to be seen in the absorption of septic matter from the throat from scarlatina, from the bowel in typhoid, from the lung in pneumonia, from the skin in small-pox, and so forth. Such septicæmias, whether complicating or supplementing the course of a specific fever, must be carefully distinguished if our antipyretics belong to two equally distinct and correlative classes.

Before the action of quinine as an antipyretic can be interpreted and rationally applied, we must have something like accurate answers to all the foregoing *questions.* For my own part, I have nothing to offer but the crude impressions derived from a wide but fragmentary clinical experience. In typhoid fever I have used quinine for about fifteen years, being first incited thereto by Dr. Fuller, at a time when Dr. Fuller was testing the quinine treatment of that fever proposed by the late Dr. Dundas. Soon I extended the use of the drug from that to all other febrile states, and have thus arrived at certain opinions rather than conclusions, which may have some temporary value in the absence of any better results from others. Speaking generally, I have found quinine to be a very powerful antipyretic in septic fevers. In that kind of remittant pyrexia which is seen in pyæmia, in septic absorption, in erysipelatous peritonitis, and the like, I have generally found the quinine in daily quantities of 20 to 60 grains will greatly reduce the oscillations. I have very often found, indeed, that by its means the rises may be wholly prevented, and the patient made apparently fever free. Again, in the prolonged hectic of pulmonary disease and like affections, I have often found it easy to moderate the daily movements, and perhaps to prevent them; but in such long-continuing cases it is not desirable to add chronic cinchonism to the other symptoms, and five or ten grains is generally the limit of the daily quantity. Little indeed could be gained by wholly reducing the fever; to reduce it in great part suffices to prevent rapid wasting, to prevent chills and sweats, and to restore appetite; but the local disease itself is not removed. Indirectly, of course, we relieve it, insomuch as the fever and the primary disease act and react upon each other, and the primary failure has more chance of repair if the fever be moderated. But to moderate it seems better than to repress it with a heavy hand, and I have many charts extending over weeks of time in my possession, which show the effect of quinine in bringing the daily oscillations within much narrower parallels, and in thus giving the patient more chance of recovery. Many a case of dangerous pneumonia of the apex and the like has thus ended favourably which looked bad enough until the above means were adopted. Quinine also in doses appropriate to the occasion—and this the thermometer alone can decide—is to be given whenever septic incidents are seen in the course of specific fevers or following them, and if the absorption be slight or temporary, the immediate and repeated use of quinine is invaluable.

Turning now from this free estimate of the value of quinine in septic and hectic states to its effects in specific febrile periods, I speak with less confidence. Sometimes it fails even in very heavy doses to depress the curve at all, sometimes it depresses the curve for a time; but the depression is followed by a bound upwards which the same means cannot control. Such results we generally see in the ascending section of the curve. I have not had much reason to congratulate myself on the use of heavy doses of quinine in depressing the elevation of the first four days of typhus or pneumonia, or in the hyperpyrexia of rheumatic fever; often I have thought my interference to have been positively harmful, but of this it is difficult to be sure. Therefore, when we have to meet immediate danger from a rapid rise, I put no trust in quinine, but resort at once to cold baths, bags of ice, and the like. If, however, a high temperature is doing harm at the latter part of a period, quinine will often act like a charm. If, for example, towards the end of the third week of typhoid, a temperature of 104°·5 or 105° is likely to be too much for the frame already too far consumed to resist it as it would once have done, then 20 grains of quinine is often very valuable; and such doses, repeated as may be required, may determine the close of the period and release the sufferer. In the second week we get no such success as this. Again, after or during defervescence, we often meet with impulsive elevations of the curve, apparently not due to any septic absorption, but which betray the enfeebled tension of the regulating power of the body; against these quinine is most useful, and often cuts them short at once. Their return may also be prevented by the prescription of five grains of quinine every morning for a few days.—*Pract., Jan.*, 1874, *p.* 32.

4.—ON THE TREATMENT OF TYPHOID FEVER WITH COLD BATHS.

By Professor Behier.

[The following paper is from a clinical lecture delivered at the Hôtel-Dieu. The author does not partake of the exclusive enthusiasm which many profess for the treatment of typhoid fever by cold baths in cases attended with unusually high temperature, nor does he recommend its usual and systematic employment.]

The times are gone by in which, according to the old Hippocratic idea, the febrile movement was considered as an effort of nature destined to subdue or to eliminate the morbific principle, a salutary effort which the physician ought rather to favour than to oppose. We now know that this disorderly action is always dangerous and often fatal: dangerous by the exaggeration of internal oxidations and the rapid consumption which accompany the febrile movement: dangerous above all by the modifications of the blood which are produced by these excessive oxidations, which pour into the common medium organic *débris* which are often very abundant, and which are not sufficiently eliminated. But the elevation of temperature is the index and the result, if not the cause (or one of the causes) of this febrile state. We may hope, then, that the intensity and the consequences of the fever will be diminished by means which will reduce the temperature of the body. But if there be a malady in which the efforts of treatment should especially be addressed to the element *fever*, surely it is in the most obstinate and long-continued of our pyrexias—in typhoid fever.

It is about twelve years since I was one of the first to introduce into our country the employment of a valuable therapeutic agent in febrile affections. I speak of *alcohol*. This is not the time or place to speak of the internal action of this remedy. Does it diminish the combustion of the tissue by directly opposing itself to that process, just as, when in a certain degree of concentration, it hinders the continuation of alcoholic fermentation? I believe this, but it is of little consequence to us just now. An invariable fact is, that in sufficient dose it lowers the central temperature both in the normal and in the febrile states; it is therefore an antipyretic remedy. It is in this character, and not as a tonic, that you see me administer alcohol, in drinkable shape, to our febrile patients, often in considerable doses, under the euphemistic designation of *potion de Todd*. Observe carefully, however, that these doses are divided. Thus, you will prescribe 80 to 200 grammes (3 to 7 ounces) of brandy in 8 ounces of water with a drachm and a half of tincture of cannella and an ounce of syrup. You will give a tablespoonful of this every two hours, diluted with three or four times as much water, if the patient finds the mixture too strong. This remedy, I repeat, gives good results; but I have no hesitation in saying that we are in possession of a much more energetic and habitually efficacious antipyretic remedy in the rational employment of cold water.

The man who restored to honour the treatment of the pyrexias, and especially of typhoid fever, by cold water, is a practitioner of Stettin, Ernest Brand.

If cold water moderates fevers, it is not, perhaps, by moderating combustions (for it is proved, on the contrary, that it tends rather to increase them), but by hastening the departure, the waste, of caloric. Its action is thus diametrically opposed to that of alcohol, quinine, and other agents which directly attack and moderate the febrile combustion; its *rôle* is that of spoliation, not of economy of waste.

In an absolute point of view, hydrotherapy, in whatever form, does not constitute an anti-febrile remedy in the rigorous sense of the word; it does not restrain combustion, it only masks and attenuates to some extent the consequences of combustion, especially the elevation of the temperature of the blood.

Fever does, in fact, betray its action on the economy by two results which are distinct, though closely allied; the rapid consumption of the tissues on the *one* hand, and the elevation of temperature on the other. Each of these *conditions* has its own dangers, and consequently affords precise indications. In the slow, chronic fevers (*hectics*, in a word), the danger consists less in the elevation of the temperature than in the gradual and progressive deperishing of the organism; it has therefore never entered the mind of any physician to employ cold water in these cases, and thereby to render more active the very wasting which threatens the patient. On the contrary, that which constitutes the immediate and direct danger in pyrexias when the fever is more acute, so to speak, is not so much the exaggerated histolysis as the very fact of the febrile increase of heat; the delirium, the convulsions, the drowsiness, the tendency to syncope or asphyxia, the whole *ensemble* of ataxic and adynamic phenomena, is due, we can hardly permit ourselves to doubt, to the pernicious action of superheated blood upon the nervous centres. In a position such as this, our duty before all things is to cool the blood by the rapid extraction of heat rom it; an indication which hydrotherapy alone is capable of fulfilling.

As I have now endeavoured to throw some light for you upon this question, it remains for me to describe to you the mode of treatment which I employed for our patients, and the way in which I believe it is best to arrange the cold baths in the treatment of typhoid fever. Here are the cases:—

Case 1.—A young girl of 19 years, lying in No. 9, Salle St. Antoine. On admission (Nov. 1) she presented all the symptoms of grave typhoid fever. She was then in the sixth day of the disease; the morning temperature was 40°·2 C. (104°·3 F.); that of the day, 40°·6 C. (105°·1 F.) The pulse was 120 to 130. The adynamia was more distinct; the patient was deaf. The gaze was fixed, stupefied, the features sombre and concentrated; she answered questions drily, brusquely, and incoherently. There were *subsultus tendinum* and fibrillary muscular tremors. The tongue and the teeth were foul, the belly moderately tympanitic, gurgling in the iliac fossa. No rose-coloured spots.

In the first days of her stay in our wards she was submitted to the treatment which we habitually apply in cases of this nature (cool spongings, application of many [24] dry cups to the trunk and limbs, *potion de Todd* 100 grammes).

But we did not find any sensible amendment, and on Nov. 8 (fourteenth day of the disease) the weakness was more and more pronounced; she lay on her back, in extreme prostration, with continual sub-delirium. The pulse was small and contracted, the breathing short, hurried, and facial. We observed a marked relative dulness at the bases of both lungs, and abundance of fine sub-crepitant râles (hypostasis).

In presence of such a condition I thought of some more energetic measures; and I prescribed the use of cold baths. On the evening of the 9th the patient was placed in a bath of water at 20° C. (68° F.) She was kept in for about twelve minutes, when she began to shiver; she was then immediately taken out, rubbed briskly with a warm blanket, and replaced in bed.

Next day (the fifteenth) we found the patient in a veritable state of *collapse*. The temperature, which on the previous evening had been 40°·6 C. (105°·1 F.), was now 35°·8 C. (96°·4 F.); the pulse was small, hardly perceptible, 140 a minute. Nevertheless we gave a bath of thirteen minutes' duration. Half an hour after the bath, the patient being warmer again, the axillary temperature was 38°·4 C. instead of 35°·8 (a difference of nearly 5° F. The pulse was also improved; it was firmer and slower (120).

From the date of this change that marked the beginning of the treatment, the latter was continued without any incident; it consisted of three baths daily of 20° C. (68° F.) temperature, and fifteen minutes' duration. After the 11th of Nov. the axillary temperature never exceeded 38°·2 C. (100°·8 F.); intelligence was completely restored, the sordes on the teeth disappeared, the tongue was moist, with only a thin covering of mucus. The numerous fine sub-crepitant râles that were heard before the treatment were replaced by fewer and coarser sounds; the hypostasis had almost entirely disappeared.

On the 17th we stopped the baths. Temperature normal, tongue moist and clean, appetite fairly good, intelligence perfect, convalescence frankly established. The duration of the disease had been twenty-four days. [Here follows a chart of the pulse and temperature which shows the striking change produced by the baths, but which we are obliged to omit.—Ed. *Pract.*]

Case 2.—Marie C., aged 19, No. 25, Salle de Antoine, admitted Nov. 2, 1873 he is a robust nurse, who has lived in Paris for six months, ill for the last our days. Tongue white, with red edges. Diarrhœa, gurgling in the right liac fossa, numerous rose spots, deafness. Countenance stupid; *subsultus tendinum*, agitated delirium, absolute mutism. The first eight days of her stay in hospital she was treated with simple cold spongings, and *potion de Todd* 100 grammes; the mean temperature was 40° C. (104° F.) in the evening, and 39° C. (102°·2 F.) in the morning. On Nov. 8 (eleventh day of disease) there was stiffness of the neck, deglutition was impossible, and there were very distinct cerebro-spinal phænomena. We prescribed cold baths of 20° C. (65°·5 F.), given in the same way as to the former patient, three times in every twenty-four hours. From that day forward the temperature fell and remained always at a moderate level, only once over 39° C. (102°·2 F.). Intelligence and speech returned gradually; on the 20th (twenty-fourth day of the disease) the baths were stopped, the temperature was normal, and we began to feed the patient.

Assuredly we should not be in a position, on the mere authority of two cases, to pronounce definitely on the value of a treatment.

As a formal contra-indication, besides hemorrhage and intestinal perforation, Wunderlich mentions collapse. As to the last, I cannot share this view, and I believe, on the contrary, that the plunge bath, and cold affusions, are one of the best agents that we possess for dissipating that tendency to paralysis of the vasculo-respiratory centres which is the primary cause of collapse. Our patient in No. 9 is a convincing example of this. On the morning of the fifteenth day she was threatened directly with collapse, the nose and extremities were cold, the axillary temperature was at 96°·8 F., the pulse was thread-like and scarcely to be felt. Half an hour after the cold bath the patient had become warmer again, the pulse and temperature had risen, and all traces of collapse had disappeared.

What are, definitely speaking, the advantages of this treatment, which have gained it so many fervent adepts, and which cause it, in certain countries—Germany for instance—to be employed to the exclusion of almost every other remedy? This is the final point for us to elucidate. A simple glance of the eye, cast upon our thermometric curves, will spare you, so to speak, all commentary. You see that by means of a few baths administered in each twenty-four hours, we succeed in beating down and mastering a febrile movement which everything seemed to show would be violent and lasting. In satisfying this essential indication, the lowering of temperature, hydrotherapy weakens at the same stroke almost the whole group of the symptoms of typhoid pyrexia. The nervous centres are the first and the most happily influenced; the delirium departs, the intellectual torpor is banished, the patient comes back to himself, and interests himself in what surrounds him; at the same time the carphology, the *subsultus tendinum*, and all the other indices of profound nervous perturbation disappear. The respiratory centre also participates in this remarkable modification; the inspirations become deeper, slower, more powerful; hæmatosis is effected more completely, and the bronchi, resuming their contractility, expel the secretion which obstructs them. The skin resumes its tone and its suppleness; the blood propelled by the heart in stronger and more regular waves, circulates more actively, and revives the stagnant secretions. Even the digestive canal presents a notable improvement, the tongue cleans and becomes moist, thirst subsides, and tympany diminishes.

There remains one sole objection—the following: the cold, by forcing the blood towards the internal parts, may favour the production of intestinal hemorrhages.

The statistics of the Leipzig clinic bear out this idea, in appearance at least. In fact, of 251 typhoid patients treated with cold water, 18 had intestinal hemorrhage, or 7·1 per cent.; whereas, according to the tables of Louis, Griesinger, Ragaine, &c., with ordinary treatment hemorrhage does not occur in more than 3 to 4 per cent. of the cases.

Wunderlich thinks that the Leipzig statistics are derived from too small a number of patients (251) to be decisive, and believes that we ought to consider the influence of series. He does not lay the blame of the greater frequency of *hemorrhage* upon the baths, for these bleedings, in the 18 cases in which they were observed were not produced immediately after immersion, but some hours, often half a day, afterwards, when the centripetal repulsion of the blood no longer existed. Finally, which is a fact of the highest importance, these cases of hemorrhage were not serious, and the whole number ended in cure.

According to Wunderlich it would be precisely the employment of the baths which so singularly diminished the gravity of the complications in these cases. The patient, thanks to the treatment, is placed in such conditions that he bears these hemorrhages much better, and they become, so to speak, innocuous. Thus, says he, if intestinal hemorrhages are made more frequent, their gravity is much diminished by cold baths. But before accepting this dictum of Wunderlich, we must remember that I had occasion to point out to you that bleeding from the bowel in typhoid fever has not always the gravity which is usually ascribed to it, and that Graves, Trousseau, and myself had observed many cases in which hemorrhage terminated favourably.—*Practitioner, Feb.* 1874, *p.* 81.

5.—ON THE EMPLOYMENT OF STIMULANTS IN THE TREATMENT OF FEVERS.

By Dr. Thomas B. Peacock, Senior Physician to St. Thomas's Hospital.

Some years ago, under the influence of teaching which I cannot but regard as mistaken, stimulants were so largely given in fever and other forms of active disease as to constitute almost the whole treatment, and make it often difficult to decide, when patients were seen, how much of their condition was due to the disease under which they laboured, and how much to the treatment employed. Now, however, a more judicious system is followed, and stimulants are less constantly and freely given. But there seems danger that, in the reaction, the discontinuance of the use of stimulants should be carried too far. Believing, as I do, that the abuse of fermented beverages is the greatest social bane in this country, I should have been glad could I conscientiously have done so, to have recommended the entire abandonment of the use of stimulants in the treatment of fever and other forms of disease. To do so would, however, I believe, be to deprive ourselves of one of the greatest means of alleviation which we possess, though one which requires great care in its use. It would be very much more satisfactory if the employment of these and other remedies could be reduced to some definite and scientific rules; and endeavours have been made by the careful analysis of the results of different methods of treatment to deduce such general rules. But the cases which we have to treat are so variable in their character that it is impossible so to classify them as to be sure that the cases compared are really similar, and that the results obtained are to be depended upon as exact. I fear, therefore, that we must be content still to leave to the judicious physician the choice of the means of treatment to be pursued in any given case, guided by his knowledge of the disease he has to treat, and his experience of the results of the means employed in other similar cases. So far as the use of stimulants is concerned in the treatment of fever, it is impossible to lay down any general rules; but I believe them to be eminently beneficial when given when the active stage of disease is subsiding, and the patient's power is beginning to give way, with the view of upholding the strength while the disease is in process of cure. If the patient is much prostrated, and the pulse weak, intermittent, or irregular, or abnormally slow, they may confidently be given; and if under their use the patient becomes less restless, the pulse improves in character, and the prostration diminishes, they may safely be persevered with. The amount of stimulus which is required must also be left to the discretion of the practitioner; but generally it is more advantageous if exhibited in small doses, repeated when the effect begins to subside. If a given dose exhibited at certain stated intervals seems to excite the patient, it does not follow that no stimulant is required; it may rather be that the dose is too large and the repetition too frequent. When the patient's strength becomes greatly exhausted, as it often does when the crisis of the disease is passing or

The veins adjoining the suppurating tissues usually contain clots in a state of disintegration, or softened at the centre into puriform fluids. The disease, as thus described, is well known, and for the most part easily recognised. There are, however, cases which sometimes show themselves in which there is evidence of intense blood-poisoning, to which the patient may rapidly succumb, or from which he may ultimately recover without the occurrence of diffuse or secondary inflammation. This condition, which has been called "septicæmia," is by some regarded as only a stage of pyæmia, but by others it is thought to be a definite and distinct disease, which, although supervening on inflammatory processes, is dependent on a cause altogether different from that of pyæmia. There is also another kind of general reaction following wounds, which has been designated "traumatic fever." In the latter there is a high degree of temperature, probably due to the absorption of some of the early inflammatory products or blood which have undergone degenerative changes. There are therefore three different conditions which may follow the receipt of an injury, and occur during the inflammatory or suppurative processes. These have been named by Bilroth traumatic and inflammatory fever, septicæmia, and suppurative fever or pyæmia. This classification, it will be seen, corresponds very closely with that proposed by Mr. Callender in his remarks at the Clinical Society, in which he described three forms of blood-poisoning supervening on wounds—viz., primary septicæmia due to the absorption of the acrid and irritating fluids thrown out into the wound within the first twenty-four or thirty-six hours; secondary septicæmia, the result of absorption of products arising from the decomposition of residua, whether those residua be sloughs or matter passing into a putrid state; and, lastly, thrombolosis, which appears to include the ordinary typical cases of pyæmia, in which there is plugging in the veins, disintegration of the clot, and dissemination of the poisoned, broken-up fragments, giving rise to the secondary deposits in the internal viscera. Now, although some cases may present themselves in which it is difficult to determine whether they should be placed under this or that category, it is generally sufficiently easy in practice to determine the nature of any individual case. Seeing, then, that tolerably well defined lines of distinction have been marked out and widely accepted, we are opposed to any unnecessary transgression of these limits. It must be observed, however, that many excellent authorities, chief among whom is Mr. Savory, deny that there is any essential distinction between septicæmia and pyæmia so called—that septicæmia is but an early stage of pyæmia due to the absorption of a large quantity or a powerful dose of the morbific material, which destroys the patient before the secondary changes have had time to occur. This is doubtless true in some cases, but not in all; for there is a form of septicæmia, which is not unfrequently recovered from, which may be accidental or experimental, from the absorption of putrid matters or some of the products of decomposition. Frequently in these cases no secondary or metastatic changes take place. It may therefore reasonably be conceived that, while a condition of septicæmia may be merely the early stage of what would become in time a genuine pyæmia, there is another form of septicæmia in no way or only remotely connected with the disease commonly known as pyæmia.

It is no doubt true that in every case of pyæmia there may be distinguished two separate processes—the toxic and the mechanical; the latter of which leads to the formation of secondary deposits of various kinds, the ulterior changes in which will greatly depend on their nature and quality. These two processes are, however, intimately associated and practically inseparable, neither separately being efficient to produce all the phenomena of genuine pyæmia. The true nature of the toxic matter which gives rise to the blood-poisoning in pyæmia, its origin and the mode of its operation, are not yet fully known. These are, indeed, the *questiones vexatæ* of pyæmia; for the mechanical processes are capable of a ready explanation, provided that we knew what it is that produces the profound alterations in the character of the blood, and which spoils it for the performance of its proper functions. The fact that pyæmia is generally preceded by the suppurative process, and especially when that process has its seat in bone or tissues freely supplied with veins, has naturally led to the opinion that the poison is in some way or another connected with suppuration; and it has, indeed, been affirmed that the morbific material arises from the decomposition of pus, and that this material is then absorbed into the venous system, leading to disintegration of the thrombi which form in veins, and then poisoning in some manner the general mass of the blood. Chemists, as Bergmann, Verneuil, and Dr. B. W. Richardson, have even isolated a principle, sepsin or septin, which they assert is capable of inducing pyæmia. This principle is described as an alkaloid which is generated by the decomposition of albuminoid matter. But against the specificity of this body it is maintained that septicæmia and metastatic inflammation and suppuration may follow the absorption of some of the other products of decomposition.

The opinion that the *materies morbi* is something which is primarily developed in decomposing pus, and that this morbific element is directly absorbed into the general system by the veins or lymphatics, derives its chief support from the numerous and careful experiments of Virchow. But this pathologist attempted to show that the pus-corpuscles were themselves altogether negative in their action; and in proof of this he asserted that pus-corpuscles as such could not pass into the venous system without producing symptoms altogether different from those of pyæmia, and he showed that, as far as the lymphatic vessels were concerned, it is impossible for a particle the size of a pus-corpuscle to pass through the fine ramifications of a lymphatic gland, so that it could not be absorbed by this channel. Virchow therefore explains the phenomena of pyæmia in the following manner: Unhealthy suppuration is set up in a wound, and the ichorous fluid which results from the decomposition of the pus, penetrating into the thrombi which have formed in the adjacent veins, causes them to disintegrate and become disseminated through the capillary system, giving rise to the changes known as secondary deposits. But although this view affords an intelligible and clear explanation of many of the effects of pyæmia, some of the secondary lesions are left unaccounted for. There are, for example, lesions occurring in the second set of capillaries beyond the original wound, while the first set of capillaries altogether escape. Hence a great difficulty in the way of the universal adoption of the belief in the embolic origin of all secondary lesions presents itself. It has, however, since been shown by O. Weber and others that some particles, especially floccules of pus, may pass one set of capillaries, but become entangled in another set. But even this explanation will not account for many other secondary inflammations, especially those of the pleura, skin, and joints.

On the other hand, some have alleged that the poison is a miasm which possesses no specific properties, but which exists whenever the atmosphere becomes unduly charged with organic matters, and which may attack persons through the medium of wounds, the mouth, the lungs, or intestines. This miasm, which consists of dry particles of organic matter, pus-cells, epidermic cells, and perhaps germs, is stated to float in the air, and to be thus carried from patient to patient. When these particles come in contact with a wound, they inaugurate certain changes in the wound, which result in the production of poisonous material, which is absorbed into the general system by the veins and lymphatics. Many observers, indeed, have shown that such particles do actually exist in the air of hospital wards, and they further maintain that these may be the bearers of poisons, which may be absorbed by any opened wound. It follows, therefore, that, according to believers in this doctrine, the ventilation of wards and the constant purification of air becomes an all-important element in the prevention of pyæmia.

In favour of the view that the toxic matter is developed in decomposing pus is the frequent supervention of pyæmia on suppurative processes in localities favourable for the retention of pus and its subsequent decomposition, and the ready absorption of the morbific elements by the adjacent veins. One of the most frequent antecedents of pyæmia is osteo-myelitis. In this affection the pus accumulates in the interstices of the bone and the small venules, which on account of their intimate connexion with the osseous canals, are unable to contract, are constantly bathed with pus, and are thus, as it were, always ready to receive the poisonous materials which may be developed by decomposition of pus. When once the morbific matter has gained an entrance into the osseous veins it is readily transmitted to the systemic veins by means of the free communication which Demarquay and others have shown to exist between the two sets of vessels.

But Dr. Burdon-Sanderson maintains that the morbific material of contagious disease in general consists, not of substances chemically dissolved in infective liquids, but of extremely minute particles suspended in it—of organisms, in fact, which he calls microzymes. In the experiments the results of which were recently published in the Pathological Society's Transactions, he asserted that in all infective inflammations in the lower animals microzymes abound in the exudation liquids; and that the same forms are also to be found in the blood of animals when in a state of acute infective fever. He further asserted that the intensity of the action of the infective fluid is intimately related to the number and the activity of the microzymes which it contained. In very infective fluids the extremely minute single rods of the Bacterium vibrio were found; while in the fluids of more protracted inflammations, and particularly in the contents of infective abscess which had existed for comparatively long periods, the prevailing forms of miscrozymes were spheroids, dumb-bells, and chains of the Bacterium varicosum.

Although Dr. Sanderson did not actually state that these organisms are the cause of pyæmic infection, or that they constitute the pyæmia contagion, he declared that there can be no doubt that by their presence in exudative liquid, however that presence is to be accounted for, they modify in a very decided manner both the local and constitutional affections. But to this it has been objected that, although microzymes do exist in the infective fluids of pyæmia, their presence does not afford any explanation of the nature of the poison and the mode in which it acts. Microzymes exist in abundance in other fluids, and have even been regarded as the exciting causes of tetanus and many other diseases, and cannot therefore be deemed to be the real and essential cause of pyæmic infection.

Having considered the nature of the *materies morbi* of pyæmia and the mode of its development, we shall now briefly inquire into the means by which it gains entrance into the general system and produces its characteristic effects. As already indicated, the usual, if not constant, place of entrance is the surface of a wound. But all wounds are not equally favourable to the admission of the morbific element. Some wounds, even of large size, are comparatively harmless; while others, although small, are exceedingly prone to become the seat of abnormal changes and the centre from which the poison spreads. Experimental and clinical experience show that there are at least three channels by which the poison may be absorbed—namely, the veins, the lymphatics, and the cellular tissues. That the veins are most frequently the channels by which the poison is admitted into the blood appears to have been satisfactorily proved; but the evidence of absorption by lymphatics and the cellular tissue is less clear and certain. From the earliest times the veins have been regarded as playing an important part in the production of pyæmia. In fact by some, even at this day, they are held to be the *fons et origo mali*. Formerly, it was thought that a suppurative inflammation was set up in the veins adjoining a wound; and that the pus which was thus formed entered the general circulation, and in that manner produced not only the toxic but also the mechanical effects of the disease. But careful recent researches have shown the fallacy of this explanation. Now we know that pyæmia does not really mean pus in the blood, that suppurative phlebitis probably never exists, and that even the pus or pus-like cells which are found in the thrombi of veins are not the essential poisoners of the blood, but are accidental phenomena, the probable result of the migration of white blood-corpuscles. It would almost appear that the veins take a comparatively small part in the production of pyæmia. It is true that the experiments and observations of Virchow have shown that the poisoned disintegrated thrombi which form in the veins may be not only the carriers of the poison, but may, by becoming arrested in some portion of the capillary system, give rise to lesions known as secondary deposits and inflammations. But there is nothing peculiar in the formation of thrombi in the veins of a patient suffering with pyæmia. Thrombi are necessary to normal repair; but in pyæmia, instead of undergoing the ordinary changes of healthy repair, they generally undergo softening and disintegration. But they do not always undergo this disintegration, for after death the most careful examination may fail to detect any disintegration of the clot. This fact affords one of the most striking arguments against the embolic origin of all secondary changes. It may be that, owing to the hyperinotic condition of the blood, small fragments of fibrin are constantly being deposited on the centripetal end of the thrombus, and are swept away again as soon as they reach the strong current of the collateral circulation. But even this explanation fails to account for the exudations, and inflammations of tissues, not only topographically, but anatomically remote from the seat of the vein which supplies the thrombic fragments. A satisfactory solution of the difficulty may, we think, be found in the changes which exist in the blood, and by regarding the lymphatics and cellular tissues as sources of absorption of the poison. The blood not only contains an excess of white corpuscles and of fibrin, but it is altered in its physiological properties, either by containing suspended in it poisoned particles and poisoned fluids, or by the general changes which result from the fermentation set up catalytically by the presence of the morbific elements. In such a state of the blood there exist, therefore, certain conditions which are favourable to the development of secondary inflammations altogether apart from the influence of embolia. The white corpuscles of the blood which crowd along the walls of the capillaries may, under the altered relations between blood and tissue, become blocked in any portion of the capillary system; and being contaminated with poisonous material, they set up irritative changes, which result in extravasations, exudations, inflammations, and suppurations. These changes occur more frequently in some parts than in others; but what it is that determines the special disposition of certain tissues to react to altered blood conditions while others escape, we cannot say, but may describe it as a vulnerability of tissue. This vulnerability of certain tissues to certain poisons is exemplified in many diseases: in measles and small-pox, for instance, it is the skin; in rheumatism, it is the fibrous tissues and the joints; in typhoid fever, it is Peyer's glands, and so on; and in pyæmia it is not improbable that the serous membranes may be the points of election for the reaction to the morbific matter. It is, moreover, interesting to note that the secondary inflammations affect chiefly parts of great functional activity, and organs which in normal physiological conditions of nutrition aid in the elaboration and in the depuration of the blood. From this it would appear that in the morbid condition of the blood and tissues the ordinary processes of nutrition are perverted, and that irritative and inflammatory reaction results. Other causes of secondary inflammations may exist in the irritation which the poisoned matters may produce in the lymphatics. It may be true as Virchow has shown, that pus-cells cannot pass through lymphatic glands; but both the cells and the fluid of pus may get into the lymphatic vessels, and set up irritation and inflammation, either at the valves in the vessel or at the glands, so that inflammation and abscess may really arise in the course of lymphatic vessels at a considerable distance from any known gland. Lastly, the amœbiform movements and migratory character of living pus-cells and ordinary white blood-cells render probable the hypothesis that secondary inflammations and abscesses may have their origin from cells which have wandered along the cellular planes of the body to a distance comparatively remote from the seat of the wound. By either or both of these means secondary inflammations may arise which cannot be explained by the dissemination of poisoned emboli and their arrest in certain parts of the capillary system.

We have now considered some of the chief pathological facts of pyæmia, the essential nature of the disease, its mode of origin, and its chief mechanical and anatomical manifestations. We have purposely refrained from discussing the subtle questions concerning the changes which are imposed upon the blood by the action of the morbific material, as such a course would have led us too far into the domain of speculative pathology, our present object being merely to supplement the remarks which we made during the progress of the recent discussion on the indications for the prevention of pyæmia in hospital and private practice. We may thus sum up our knowledge:—The poisonous material of pyæmia, whatever be its real nature, is closely related to the suppurative process, and may actually originate in certain unhealthy conditions of that process.

But even where the poison does not really arise in a suppurating wound, a certain disposition of the tissues of the wound is necessary to admit of its entrance into the general system. There are, for instance, strong facts to prove that a perfectly healthy state of a wound is to a great extent inhibitory to the absorption of the poison. Billroth has accounted for this on the ground that perfectly healthy granulations prevent the intromission of organic particles and fluids; and in the same way may be explained the cases where no evil effects result from the presence of decomposing fluids in wounds with healthy granulations. Whether this poison is capable of being transferred from one wound to another through the medium of the air may be doubtful, but there is strong evidence that the virus may be conveyed to wounds and implanted there by means of sponges, dressings, and instruments. The outcome of all this is, that, if pyæmia is to be prevented, the well-established rules of surgery and hygiene must be persistently employed in the dressing of wounds and the treatment of patients. —*Lancet, March* 28, *and April* 4, 1874, *pp.* 446, 483.

7.—ON PYÆMIA.

By Dr. J. Hughes Bennett, Professor of Physiology and Clinical Medicine in the University of Edinburgh.

I have been greatly interested in the discussion which has taken place on pyæmia in the Clinical Society of London. Nothing could be more reasonable than the suggestion of Mr. Hulke, that there should be a clear understanding as to what the society were talking about. "It appears to me," he is reported to have said, "that Dr. Bastian's remarks are most pertinent on the question of septicæmia, but I do not see their bearing on the subject of pyæmia." In truth, in this, as in many other discussions which take place in medical societies, accurate definitions and logical precision of language have been too frequently forgotten. A notable example was presented last year in a discussion which took place in the Pathological Society of London, on the subject of tubercle. In no sense whatever could it be said to have advanced our knowledge, all the speakers apparently having puzzled themselves and bewildered one another, in a vain effort to explain German mysticisms, and apply new terms to very old and well known morbid conditions. The confusion which prevailed on the subject of pyæmia may, I think, be dissipated; and I propose, in this short communication, to make an effort to do so.

The term pyæmia was introduced by Piorry, and literally means pus in the blood, or purulent blood. He supposed that the formidable symptoms, which constitute the dread of surgeons, namely, the rigor, typhoid fever, depression, and multiple abscesses, generally terminating in death, following operations or injuries, were owing to pus in the blood. However the pus was introduced, whether by absorption from wounds or from phlebitis, or from an imaginary lesion he called hæmitis, or inflammation of the blood itself, the result was purulent blood, or pyæmia, and the cause of the fatal symptoms. But M. Piorry was no histologist. Had he examined the blood with a microscope, he would have found no pus-cells there; for, in fact, the blood of pyæmia contains no pus. Our present knowledge, therefore, demonstrates that the disease called by practical surgeons and obstetricians pyæmia is not necessarily caused by pus in the blood. This is further shown by the fact that if, as Mr. Hulke truly stated, we inject recent or so-called laudable pus into the blood of an animal, it produces no injurious effects. I have frequently performed this experiment (Clinical Lectures, 5th ed., p. 906), as have Sédillot and other pathologists. But, if putrid or ichorous pus be so injected, then we have the symptoms of so-called pyæmia; and the same result occurs if we inject putrid fluids, irrespective of purulent formations. It follows, therefore, that it is not pus, as pus, which produces the mischief, but any organic fluid in a state of putrefaction, that causes the blood-lesion, and as a result, the group of symptoms now called those of pyæmia. That pus is easily decomposed has been long known; and to the absorption of that fluid, when putrid, the French surgeons Boyer and Bonnet, many years ago, attributed the noxious effects referred to as pyæmia. This condition was well-named by Vogel ichorrhæmia, which is one of the varieties of septicæmia.

It appears to me, therefore, that Mr. Savory expressed the correct view on this subject, but did not appear to see that the term pyæmia ought to be abolished from medicine and surgery; and that the form of septicæmia to which he referred is correctly expressed by the term ichorrhæmia. In what the nature of septicæmia consists, is unknown. Why one variety should produce the phenomena of small-pox, another of measles, and a third of ichorrhæmia, we are ignorant. It is the same ultimate fact in pathology that exists in therapeutics, in reference to the proportion of different poisons or drugs. All we can do is to guard against the morbid influences, or modify them when present. It is in this point of view that the observations of Dr. Bastian are of direct practical importance. All trustworthy research indicates that the poisonous properties are not attached to imaginary germs constantly floating in the atmosphere, but rather to the ultimate molecules of organic matter, animal or vegetable, which belong to or enter the economy. I believe these not to be hypothetical, but visible molecules, detectable by good microscopes, and that, in the case of ichorrhæmia, the histological examination of the granulations of, and the discharges from, suppurating surfaces will do much to indicate how and when they are to be removed or destroyed.

Although I have endeavoured to show that, in the morbid condition called pyæmia by practical surgeons, there is no purulent blood, and that the expression is wholly inapplicable, there can be no question there is a true pyæmia, or suppuration of the blood. This I described so long ago as 1845 (Edinburgh Medical and Surgical Journal, October, 1845), in a case where I found, in a case of hypertrophied spleen and liver, that the blood was crowded with pus-cells. But in that case, I took great pains to show that there was no inflammation anywhere in the economy—no suppurating sore, no phlebitis. Nevertheless, as I had no doubt that pus existed in the blood, I called the case one of suppuration of that fluid without inflammation. But the pathologists of that day could not comprehend the existence of suppuration without inflammation, and hence I subsequently called the morbid condition I discovered leucocythæmia, or white-cell blood, because that term "expresses the simple facts, or a pathological state, and involves no theory." By some, it was supposed that the colourless corpuscles of the blood were different from pus-cells, and that they rendered the blood white, producing the white blood of Hippocrates. This error still prevails among the German pathologists, who call this morbid condition leukhæmia. It is now, however, recognised that pus-cells, lymph-cells, young mucus and epithelial cells, and the colourless cells of the blood are identical, and hence Robin has applied to the whole group the name of leucocytes or white cells. To have retained the term suppuration of the blood, or true pyæmia, in face of the constant employment of this last term by surgeons, to express a fatal poisoning of the blood, could only have led to confusion; although, as a matter of fact, leucocythæmia is the only purulent blood known in pathology, and the only suppuration of that fluid with which we are acquainted.

I propose, therefore, in order to avoid the confusion which now prevails from the incorrect employment of terms that can only mislead, to suggest the following definitions.

Septicæmia.—Poisoning of the blood from every source—a general term.

Ichorrhæmia.—Poisoning of the blood from ichorous matter or decomposed pus.

Leucocythæmia.—White cell-blood; excess of colourless cell in the blood.

Leukæmia.—White blood, from excess of fatty matter in the blood, so-called chylous or milky blood.

Pyæmia.—This term has no meaning, unless applied to ichorrhæmia or leucocythæmia. Its employment can only lead to confusion.—*Brit. Med. Journal, March* 14, 1874, *p.* 339.

8.—DR. HUGHES BENNETT'S STRICTURES ON THE "TUBERCLE" AND "PYÆMIA" DISCUSSIONS.

By Dr. LIONEL S. BEALE, F.R.S.

"In truth, in this, as in many other discussions which take place in medical societies, accurate definitions and logical precision of language have been too frequently forgotten." Such are the words in which Dr. Bennett criticises our discussions last year on the subject of tubercle. He speaks of the speakers having "bewildered one another" and "puzzled themselves," but tells us that he thinks the confusion which prevailed upon the subject of pyæmia "may be dissipated."

Dr. Bennett tells us there is a "true pyæmia or suppuration of the blood;" and that, in a case of "hypertrophied spleen and liver," the blood was crowded with "pus-cells." As "pus existed in the blood," the case, he declares, was one of "suppuration of that fluid without inflammation." It is now, Dr. Bennett affirms, "*recognised that pus-cells, lymph-cells, young mucus and epithelial cells, and the colourless cells of the blood, are identical.*"

I know not what other pathologists may think of Dr. Bennett's effort to dissipate the confusion which exists; but I cannot allow his paper to pass without expressing my dissent from every one of the above propositions. Dr. Bennett uses the word "pus-cell," as it seems to me, most improperly. He has not proved that the bodies he calls pus-cells are of that nature. But to assert that pus-cells and young mucus and epithelial cells are *identical*, is certainly open to grave objection. That they are like one another to look at, that they may exhibit similar reactions when tested, that they may be alike in chemical composition, may be true; but surely things may be like one another in many particulars without being *identical*. Although the bodies referred to by Dr. Bennett are alike in many points, no two are alike in all, and each has properties peculiar to it; each has a very different life-history, and performs a different office. They may be called by the same name, but differences cannot be dissipated in that way. As Dr. Bennett says, they have been *called* leucocytes or "white cells;" but they are not *cells*, nor are they *white*.

Dr. Bennett is surely as much open to the charge of forgetting "accurate definitions and logical precision of language" as any of those who joined in the discussion of tubercle last year. Dr. Bennett would introduce terrible confusion by calling leucocythæmia "suppuration of the blood." For my part, I cannot imagine any phrase open to more serious objections than this; and, whatever may be the fate of *pyæmia*, I do hope neither the Pathological Society of London nor any other Society will consider *pyæmia, suppuration of the blood, and leucocythæmia* identical.—*British Med. Journal, March* 21, 1874, *p.* 1874.

9.—ON CANCER.

By CAMPBELL DE MORGAN, Esq., F.R.S., Surgeon to the Middlesex Hospital.

Why should malignant growths, some cancers especially, when once formed, infect neighbouring and distant parts, and ultimately contaminate the system? The knowledge we have of late years acquired of the independent actions of cells renders a solution more easy, though I admit that it is not free from difficulties. It has long been known that cells are dispersed around a cancer tumour, the direction and rapidity of the dispersion varying according to the density of the tissue, and the abundance of connective-tissue spaces or of lymphatic or vascular networks in which it lies. It is now known that the white blood-corpuscles can wander from their vessels; more recently it has been seen that cancer-cells—like the white corpuscles—show amœboid movements, and can thus travel independently in tissue spaces, or even permeate delicate membrane. There is no doubt, I suppose, that these cells, whether the matter that forms their nuclei, or the free granules, are the active agents in the reproduction of cancer. We can understand that these cancer-germs may thus travel through their own power, or be carried along in the vascular systems or connective spaces in every possible direction. And it must be remembered that cancer is for the most part a structure infiltrating itself among the tissues in which it lies—not surrounded by a capsule or limitary membrane of any sort, as are so many non-infecting tumours. The newly developing and active cells are always in direct contact with the healthy tissues, on which, in fact, they are living. Once set free, the cells may travel in the three channels I have indicated. First, and chiefly, they may travel along the lymphatics to the glands, where they will form new centres of growth, and by continual invasion affect a series of them, and perhaps contaminate the lymph itself. Or, secondly, they may travel through the surrounding tissues, their direction being often determined by the density of the tissue. Thus it is often noticed that after breast operations the disease will reappear above or below the cicatrix, and will travel on the same plane, not passing across the dense cicatricial tissue. The disease will seem to prefer the easier course, but the densest tissues will not prevent this permeation. In malignant intraocular growths the germs are found at times extending through the dense sclerotic and then bursting out into a new growth into the free periocular structures. Thirdly, they may pass into the blood-vessels, and thus into the general current of the circulation; hence, we may get infection of internal organs, or of parts widely removed from the original tumour. We have evidence of this mode of conveyance in many recorded cases of sarcoma and enchondroma, and the secondary cancer growths in the lung and liver, so like in many respects to the deposits we get in pyæmia, point to the same mode of origin. Besides these principal modes of conveyance, we meet at times with evidence of conveyance in free cavities by gravitation, and sometimes, but rarely, of implantation by auto-inoculation.

There are some tumours of which the elements are disseminated far more rapidly than are those of cancer; on the other hand, some cancers show little tendency to dissemination. A small melanotic sarcoma, for example, will often give rise to numberless secondary growths in a marvellously short space of time. This is perhaps due to the great activity of growth and the abundance of granular matter which often characterise this form of tumour. If we look to epithelioma, on the other hand, we see that, great and destructive as may be its local action, yet its area is usually limited to the first part invaded and the contiguous lymphatic glands. The structure explains this. The component cells are coarser, have less independent movement, are more packed together, sometimes becoming coherent. They may pass into the lymphatics—coarser matter will do that,—but, like the cells from which they originate, they are little disposed to wander; the tumour grows more by continuity, less by dispersion. This is still more marked in rodent cancer, where the cell activity is at a minimum and the ulcerative keeps pace with the formative processes.

It seems to me, then, that we can account for the diffusion of cancer, when once formed, without calling to our aid a pre-existing or concurrent disease of blood.

Admitting that cancer, like other infecting tumours, is reproduced by the growth of germs which have been conveyed from it to distant parts, the question arises, How can we account for the fact that after removal there may be no reproduction for years? Can it be imagined that during all this interval the germs of disease deposited from the original tumour have remained inactive? Is not this contrary to the nature of cancer, and does it not show that there is a cancerous poison in the system? I admit the difficulty; but it is not removed by assuming that the blood or the system is primarily diseased. My conviction is that cancer and other tumours may remain in a rudimentary state for an indefinite length of time. In the case of recurrent cancer we often see that an enlarged and hard gland which is left after the removal of the main tumour will remain quiescent for years, and that then active growth will set in. The same will take place in a cicatrix which has perhaps remained hard. It may be said that this is only an evidence of the return of the blood to a cancerous state. But it is as difficult to understand the quiescence of the blood during this long time as of the cancer germs. Many facts, both in normal and morbid development, are indicative of the possibility of tissue remaining unchanged for long periods, and then taking on new phases of growth. Such are, for example, the change of epiphysial cartilage to bone after twenty years, the change in the female breast at the period of puberty, the shedding of the milk-teeth and the development of the second set. Still more to the point are, perhaps, such instances as the frequent enlargement of the prostate gland in advanced age, or the growth of hair on the female face from follicles which had existed, but in an inactive state, for sixty years. It is not at all improbable that what is here seen as a result of advancing degeneration often plays its part in cancer development.* It is probable, too, though it may not admit of proof, that in those cases, which are so common, of hereditary lipoma or sebaceous or dermal cyst, the rudiments are present from the first. We know as a fact that such tumours will remain small, only just perceptible perhaps for twenty or thirty years, and will then take on active growth. In the imperfect structure of an inefficient mammary gland, or an abortive testicle, is there not present from the earliest period some form of indifferent tissue prone to irregular development, but quiescent during the active periods of life? This is rendered the more probable from what is so frequently found in connection with epithelioma. We have here on the surface what may be hidden in the case of the deeper glands. Scales, or warts, or moles, and some imperfect epithelial growths, may remain without change for fifty years, and then may become the seats of a cancerous growth. If such a scale or other imperfect tissue were persistently irritated, it would doubtless become cancerous at an earlier period. Cancer has been there potentially for years; but its time has not come. Such, I believe, is the explanation of the fact that cancer germs which have wandered from the parent tumour may remain quiet for indefinite lengths of time. I can see no other way of accounting for such a case as this. A tumour is removed; a year afterwards a small nodule, not so large as a split pea, is discovered and removed, no other nodule being perceptible after careful search. After another long interval, another small nodule is found and removed; and again there is quiescence. This may be again repeated. Can we suppose that there are continual reproductions of a blood poison followed by periods of healthy condition?

While I believe it probable that the germs of cancer may thus remain in a sort of dormant condition for long periods of time, I would by no means imply that there is not in cancerous patients a special disposition to tissue change located in some, but not in all the structures of the body. The general tendency to degeneration may give rise to the primary local change; so may local irritation. Thus, out of a hundred chimney-sweeps or clay-pipe smokers, a certain number may have chimney-sweep cancer or lip-cancer, the number varying perhaps according to the duration and extent of the irritation. But the majority will not become cancerous, irritate how you will; and of the remainder, few probably would have cancer, unless irritation were applied. The same remark may be made as to such ordinary growths as warts. In some the least irritation will produce them; in others they never will appear. Like other tumours, too, they have their seats of election, and their favourable period of life, and they often appear to be inherited.

There are other facts observable in the later course of a cancerous disease which are difficult of explanation either on the hypothesis of blood disease or of localisation. For example, a local cancer in an ordinary situation may give rise to a general disease of a structure totally different from it. We meet, for example, with a wide-spread disease of the osseous system following a breast cancer, and the disease may not show itself as cancer deposit, but as entire absorption. This may be seen in many of the bodies of the vertebræ, which may totally or partially disappear without any cancer growth being detected, and while yet cancer deposit is taking place in other bones. These, no doubt, are exceptional cases. We find, however, that natural tissues are at times disposed to undergo changes which are equally inexplicable. The changes taking place in some out of all the muscles of the body, the fibrous growths on a number of the nerve-cords in multiple neuroma, and other cases, show that local tendencies to tissue-change exist in detached parts of general tissues. They may be allied to the degenerative changes which pervade unequally particular tracts of the arterial system. We cannot as yet account for these and such-like facts; but whatever may be the local condition which thus determines special tissue-change, whether its seat be in the parenchyma or in the nervous system, the same may possibly determine a preference for deposit or degeneration under the conditions of progressive cancer. We have, in fact, to solve the question, Why have diseases seats of election at all?

Another remarkable and not very explicable phenomenon is the arrest of cancer growth and the gradual wasting of the diseased mass. This is an occasional event which is very important, as it encourages us to hope that a cure may yet be found for the disease. Although this recession of cancer is sometimes coincident with the development of tubercle, I doubt whether we have sufficient data on which to establish the conclusion that the one disease is antagonistic to the other. It is, however, worthy of note that, frequent as is the occurrence of cancer in consumptive families, the co-existence of the two diseases in one member is not a common event. But cancer is a disease of the middle and later periods of life, and tubercle generally of an earlier one. Those who live to be victims of cancer have passed the period when tubercle would be most likely to set in. That this arrest or recession is not due to any exhaustion of the cancerous element in the body is shown by the fact that, while the disease is retrogressive in one part, it is in active growth in another. The retrogression differs from the degeneration of the earlier formed cells, which is constantly going on in the interior of tumours, as shown by the presence of fat-granules, compound granular cells, and such like. In the case alluded to, the activity of the whole mass is arrested, new cells cease to be formed, and the tumour fades. I doubt whether this can be from the plugging of the vessels feeding the tumour. This no doubt often takes place, and the death of some portion of the mass may be the result. But when we find a widely spread mass becoming quiescent and then fading throughout its whole extent, we must, I think, look to some other cause—for cancer is so far like a parasite that it draws its nourishment largely from the tissues in which it lies. When, for example, a nodule of enchondroma or of sarcoma is detached and floated off in the current of the circulation, and afterwards grows in the lung or the liver, it must in the first instance obtain its nourishment from surrounding tissues. We must look elsewhere for an explanation. It only shifts the difficulty back a stage or two, as I have so often done before, to suggest that the recession of cancer takes place in obedience to the law under which local atrophy, independent of inflammation or disuse, may occur; or that it may be due to some want of organising power inherent in it from the first, as some cancers seem born to be atrophic. It is, under any circumstances, a most important subject for investigation.

Lastly, I would suggest the question—Why is it that cancer, if a blood disease, should be so pre-eminently a disease of women? Taking all cases together, I suppose we should not be far wrong in saying that the uterus and the mammary gland are the seats of the disease in 90 per cent. Does it not look as if the mere tissue-changes dependent on the peculiar vital conditions of these organs were the starting-points of the disease? Were it not so, and if the disease were from the first in the blood, surely we should find that men were as prone to cancer as women. Other forms of tumour are nearly as prevalent in the one sex as the other. It is a point worthy of consideration.—*Medical Times and Gazette, March* 7, 1874, *p.* 259.

10.—ON CANCER OF THE BREAST.

By Christopher Heath, Esq., Surgeon to University College Hospital.

Sir Astley Cooper gave as the result of his vast experience two years for the full development of cancer in the breast, and from six months to two years more as the limit of the patient's life; but Mr. Sibley's statistics of the cancer-wards of the Middlesex Hospital give fifty-three months as the average of life when the primary disease is removed, but only thirty-two months when the case is left to nature. This is taking all cases of cancer together; but Mr. Baker's statistics are still more useful, since they separate scirrhus from medullary disease, with the following results:—In scirrhus, when left alone, the average duration of life is forty-three months, and when operated on, fifty-five months; in medullary disease, when left alone, life averages about twenty months, and when operated on, forty-four months. You will see, therefore, how poor a chance our second patient had if left untreated; and my own experience has fully convinced me that to leave a rapidly growing cancer of the softer variety to burst through the skin and form a large fungating mass beneath the patient's face is great cruelty, when it is still possible, by even a severe operation, to give some months of comparative comfort, and to render the disease, when it returns, much less offensive and trying to the patient, by ridding her of the bulky breast.

To do good, however, in any case of cancer, the treatment must be very thorough; for any treatment, whether by the knife or caustic, which does not remove the whole of the disease, only does harm by stimulating the part left to increased action. In the case of the breast, it is, I believe, impossible for any surgeon to be sure that he has removed every germ of disease unless he extirpates the entire breast; and this is the rule I always follow, though I am sorry to say it is not universally accepted. Dr. Hughes Bennett many years ago called attention to the fact that the microscope showed disease in tissues apparently healthy when in close proximity to diseased organs; and the late Mr. Charles Moore directed attention especially to the mischief of inadequate operations in cases of cancer, in a paper which you may consult in the fiftieth volume of the Medico-Chirurgical Transactions. To do your patient justice, you must remove the entire breast in all cases, with all skin which may be the least implicated so as to have lost its healthy softness, any of the pectoral muscle which may be involved, and all enlarged axillary glands. The incision you should employ is, I believe a matter of perfect indifference, though in the majority of cases the common elliptical ones above and below the nipple are satisfactory. But you must vary your incisions according to the amount and position of skin involved; and, so long as you contrive that your wound shall have a good drain, its direction is unimportant. After the first incisions, I recommend you to make a very sparing use of the knife, and to trust very much to your fingers. You will find no difficulty, unless the patient is unusually thin, in stripping up the skin and its subjacent fat from the breast with your finger, aided by an occasional touch of the knife; and this plan has the advantage of securing you against slicing small portions of the breast or making "button-holes" in the skin. You can also better appreciate when you have reached the thin border of the breast, and so avoid leaving some of it behind, as is too often done. Forcibly raising the border of the breast, the surgeon at once ascertains whether the pectoral muscle is involved or not, and if it is, he should in my opinion, not scruple to cut into it freely, so as to remove, if necessary, a considerable portion of the muscle; and here again hemorrhage will be saved if the fibres are torn as much as possible. In removing the axillary glands, it is especially desirable to be chary of the knife, both on account of the hemorrhage and because the finger can hook them out so much more satisfactorily than any instrument. In cases of extensive glandular affections it is necessary to lay open and clear out the axilla almost as if for a dissection; and though, fortunately, the principal glands lie along the edge of the pectoralis, I have been obliged to expose a couple of inches of the axillary vein on more than one occasion, but without any harm resulting. It is common enough to expose the subscapular vessels and long subscapular nerve; but, by using the finger entirely, I have never had any trouble from hemorrhage. It has been proposed to cut across the pectoralis major in severe cases, so as to insure a removal of all the disease; but I have never found it necessary to do this, for it is, I find, perfectly possible to pass the finger up to and beneath the clavicle, and no more would be gained by the division of the muscle.

After completing the removal of a breast, I am in the habit of mopping out the wound very freely with the solution of chloride of zinc recommended by

Mr. De Morgan, both on account of its antiseptic qualities and its possible effect upon any cancer-germs left in the tissues. I used to employ it of the full strength (gr. xl. ad ℥ j.), but I fancied that it now and then caused the skin to slough, and I have therefore reduced it to one-half the strength, and I think, with advantage. In closing the wound, it is most important to leave a sufficient and dependent opening for drainage, and this is most conveniently placed near the axilla; and for a dressing I find nothing so satisfactory as oakum or carbolised tow, for the supply of real oakum has failed. This soaks up the discharge, and allows it to permeate, and hence keeps the wound dry, which I take to be the great point in favour of rapid healing and limited suppuration. It is convenient to place a piece of gauze between the skin and the oakum, so as to prevent the latter adhering, and to cover the oakum with a folded towel to absorb the moisture, which is abundant at first.—*Medical Times and Gazette, Feb.* 7, 1874, *p.* 147.

11.—ON THE RELATION OF URIC ACID TO GOUT.

By Dr. William M. Ord, Assistant Physician to St. Thomas's Hospital.

Sydenham's brief but pithy definition of gout, as having its origin in the impaired concoction of matters both in the juices and parts of the body, is certainly out of harmony with the current ideas of the nature of this malady. Dr. Garrod's published opinions may be taken to be the best expression of these ideas; and it must be confessed no man has a better right to our respect than he, who, detecting uric acid in excess, as urate of soda in the blood, has since, by patient and conscientious observation, put this and similar discoveries beyond doubt. To examine Dr. Garrod's opinions:—In Reynolds's "System of Medicine," three propositions are offered by Dr. Garrod (p. 883): 1st. That to produce this form of articular inflammation the blood must contain urate of soda in excess, and even the phenomena of irregular gout, or gouty manifestations, require the same conditions. 2ndly. That gouty inflammation is invariably accompanied with the deposit of urate of soda in the inflamed tissue. 3rdly. That the amount of deposited urate holds no relation to intensity of inflammation; that in some structures infiltration may give rise to scarcely any inflammation, tending to show that the deposit is rather the cause than the effect of inflammation. In an earlier paper, Dr. Garrod spoke of excess of urate of soda in the blood and joint deposits as pathognomonic, so that gout might be defined from rheumatism as clearly as measles from scarlatina by the rash. The propositions, therefore, mark an advance in speculation; urate of soda rises from a sign to be a *materies morbi*, and its deposit in cartilage or ligaments indicates these as agents in freeing the blood from excess; afterwards, the blood being still impure, other surfaces are selected, so that gout very nearly returns to its derivation—*i.e.*, a dropping (*gutta*) of morbid matter into joints. The cause of excess of urate is held to be partly defective excretion (kidneys), partly undue formation of uric acid in the blood. Dr. Garrod carefully guards against routine practice consequent on the simple acceptance of this idea, pointing out that each case exhibits its peculiarities, and demands, in some respects, separate treatment. This qualification being properly recognised, the conception of the cause of gout suggested in the above propositions is, as Dr. Gairdner always held, insufficient. The author maintains that it is most important to regard gout as a mode of decay, and a gouty man as journeying to the grave through definite regions of textural degeneration. Excess and deposit of uric acid are notable accidents of the way, but in errors of the entire nutritive process must be sought the explanation of that systemic loosening and poisoning to which gout may be reduced. Granted that uric acid with soda is deposited within gouty joints, does the fact necessarily mean that the acid is poured out to free the blood from its presence? The sum of evidence appears against the necessity of such a conclusion. Uric acid, a product of disintegration of tissues, and food, is little soluble compared, say, with urea, and therefore the kidneys cannot remove much of it at a time from the blood. Similarly, if uric acid be largely formed within the tissues, the blood cannot take up much of it. In arcus senilis we do not infer the blood to be too full of fat, nor the cornea to be an eliminative organ; on the contrary, we infer degeneration of cornea, with comparatively insoluble deposit of fat within the corneal tissue. Again, cholesterine crystals in old sebaceous cysts, or in advanced atheroma, suggest local degeneration and immobility of resulting substance. Therefore, it is here claimed that urate of soda in parts of the body remote from the centre of circulation, in tissues little vascular and of low vitality, may be taken as a kind of degeneration or want of tissue organisation. Secondly, I would ask, is it necessary to assume that the deposited matter is either the cause or effect of gouty inflammation? As Dr. Garrod remarks, "Gouty deposit does occur in the cartilage of the external ear without any recognised preceding inflammation," and "in joints which have become callous, considerable deposits may occur without the production of much local inflammation." If, however, such deposits are meddled with ever so gently, sharp gouty inflammation always follows. Taken deposits, or gouty constitution, or injury alone, no gouty inflammation follows. After death the great toe joint is constantly found the seat of unsuspected mischief—more or less ulceration (Dr. Garrod). The blood may be full of urate without producing gout, and the great toe may be injured again and again, and yet no gout appears. To show that the great toe is mainly the first joint attacked in primary gout, the author quotes Sir C. Scudamore's table of 516 cases, in which one or both great toes were affected in 341, or about two-thirds; in thirty-one, great toe with other parts of foot; and in only twenty of the whole was the lower extremity wholly exempt. In a word, the lower extremity, which bears the greatest strains in the exertions of daily life, is mainly the site of the first development of gouty inflammation. This holds good especially in hereditary gout; in acquired gout it is otherwise, as that means mostly general degeneration of body following default of particular organs. These organs are first attacked. So much for primary gout. Further, in our daily experience every fresh attack has mostly an exciting cause—a long walk, a joint-wrench, fall, nervous excitement or anxiety, &c., all disturb the circulation of some part; the temporary congestions of the healthy man become the characteristic inflammations in the gouty. In some cases the quantity of urate of soda seems to bear relation to the inflammation. Extension of gouty inflammation, however, requires not uric acid for its explanation. The author believes it may be propagated from part to part by nerve influence, as in simple inflammation. The direct influence of the nervous system in gout is admitted by all authors in one way or another. In some cases a strong will can fight down the gout; others defy with impunity (for a time) the received laws of diet and regimen. Too much importance is attached to the supposed improvement of general health after an attack of gout. This is to a certain extent true in many early attacks, but recurring inflammations damage tissues and diminish power of recovery. To avert local attacks and check inflammation must be the aim of practice. The old rules of regimen hold good still. As regards remedies, eliminant medicines—and amongst them pure water—hold high value. In addition, all possible means to promote the concoction of healthy tissue—as cod-liver oil, milk, and fatty food, good meat, various tonics, as iron, arsenic, and vegetable bitters. We must try to wash away the offending extractive, not neglecting to repair and refit as perfectly as possible. *Summary.*—1. Gout is a mode of decay of the whole system, marked by deposit of urate of soda in and about joints, and by local inflammation of a particular kind. 2. The deposit of the urate is the result of local or general disintegration, and is not to be regarded as a means of eliminating urate from the blood. 3. The local inflammations do not necessarily depend upon the deposition of urate, and the deposit is not a consequence of inflammation; at the same time it is probable that excess of urate in the blood produces irritation of tissues. 4. The local inflammation is peculiar, in respect of the ease with which it is produced, of the pain by which it is attended, and of the products—which are chemical rather than structural (chemical substances of low molecule, tending to crystallise or to be dissolved, being formed in the part, instead of substances of high molecule, tending to be organised). Interstitial subcrystalline deposit is common, suppuration rare, in gout. 5. The local inflammations are set going by local exciting causes. 6. The local degenerations and inflammations tend to infect the rest of the system through the blood, and to set up similar actions elsewhere through reflex nervous action.—*Medical Times and Gazette, Feb.* 28, 1874, *p.* 233.

12.—ON THE EMPLOYMENT OF QUININE IN THE TREATMENT OF RHEUMATIC FEVER.

By Dr. A. W. Barclay, Physician to St. George's Hospital.

[Dr. Barclay states that his usual course of practice for some years has been to treat cases of rheumatic fever with large doses of alkali, his object being to neutralise the acid which is so abundantly formed in this disease. How far the effort is successful may be open to doubt.]

It can be readily shown in many cases that at the very time when the sweat-glands are stimulated to great activity, and a copious sour-smelling perspiration covers the whole surface of the skin, the kidneys may, under the influence of remedies, be secreting excessively alkaline urine.

It would lead me too far away from the main question were each of the points just alluded to discussed at the length which their importance demands; but there are one or two considerations to which I may call attention in passing. First of all stands out clearly before us this acid-forming tendency which so remarkably characterises rheumatic fever, and its special excretion by the skin and the kidneys. Uninfluenced by treatment, the urine indicates the presence of acid by its copious lateritious sediment, even sooner than the skin. It does not seem that as yet we can penetrate much deeper into the mystery than to say that in rheumatic fever the two conditions of acid formation and joint inflammation are associated, and invariably associated, together. Many other circumstances in the chemistry of life lead to the generation of acid. In certain forms of dyspepsia, the patient may have his stomach full of sour fermenting aliment, and acid liquid rising into his mouth, with constant heartburn, and yet have no symptom of rheumatism. If we examine his urine while the stomach fermentation is going on, we shall find it neutral, or even intensely alkaline. A few hours later it has become excessively acid, with an abundant deposit of lithates. The acid has passed through the blood, and has been eliminated by the kidneys; but it has given no indication of its progress. Yet again, when the dyspeptic symptoms are very few and slight, the patient a free-liver, and ostensibly in the enjoyment of good health, there may be no period during the twenty-four hours in which the urine, if collected and allowed to stand, does not exhibit the same acid reaction, with the pink-coloured sediment adhering to the vessel. Such an one may have no rheumatic tendency whatever, though in this case too the acid which is being constantly excreted by the kidney is almost certainly formed in the stomach, and passed through the current of the circulation in order to reach its principal outlet. Mere acid formation is clearly not the cause of those symptoms to which the name of rheumatism is given, either in its acute or chronic form.

Next we have the striking fact of the kidney secretion getting rid alike of excess of acid and of alkali. It must be in obedience to some fundamental law of the economy that this result obtains. No more striking evidence of its reality can be adduced than the hourly variations of the acid and alkaline reaction of the urine seen in some of the forms of stomach fermentation just alluded to.

It is a little difficult to understand why for a time the generation of acid in the stomach should leave the blood in such a state as to produce excessive alkalescence of the urine; but it is clear that the acid is not wholly generated at the expense of the food. In addition to the fermentation excited there must be also a process of excessive acid secretion carried on by the glandular apparatus, and this leaves the alkaline base in the blood in such quantity that it cannot remain there, and it has to be got rid of by the eliminating action of the kidney. Hence the urine becomes for a time intensely alkaline; and if the acid pass out of the body by vomiting, it will necessarily remain in an alkaline state for a considerable period. If, on the contrary, the acid be retained in the stomach, and gradually passed on into the intestinal canal, the various absorbents begin to take it up in a liquid form; but as it can have no permanent place in an alkaline fluid like blood, the functions of the kidney are again called into requisition to get rid of the excess.

Further, these facts prove that although the blood is alkaline in health, there must be very considerable deviations on either side from what may be called its normal degree of alkalescence under ordinary circumstances, and that still greater deviations are not incompatible with the continuance of life. Any substance having an acid or alkaline reaction, if absorbed from the stomach and primæ viæ, must in passing through the circulation to its outlet in the renal secretion cause such deviation temporarily to occur, its persistence being prevented by the tendency of the kidney to get rid of the excess whenever it is present.

The results of clinical experience can leave us in no doubt as to the reality of this eliminating power of the kidney; and it would seem to me that it must be regarded as one of its special functions. Physiology may perhaps some day explain why the structure of the kidney causes it to be the medium of separation from the blood of urea and uric acid to the exclusion of other glands in health. For the present we must be content with admitting that it is so. So long as the kidney remains unaffected by disease, these elements are not found in excess in the blood, their formation being exactly counterbalanced by their excretion. In disease, on the other hand, we find an excess of uric acid in the blood associated with gout, an excess of urea associated with Bright's disease. In gout there is no accumulation of urea in the blood; in Bright's disease there is no great excess of uric acid. But in either case, when the elimination by the kidney is stopped, the waste material may exude through some other channel.

The analogy between the function of the kidney as regards those two elements, which are necessarily regarded as its special secretion, and its power of recovering from the body an excess of either acid or alkali, seems to me to be so striking, that I should be disposed to regard the one as not less important nor less clearly proved than the other. It is quite unnecessary to speculate on the remote effects of excessive alkalescence, or of possible acidity of the blood. Such conditions are of themselves abnormal—are, in fact, disease. The ordinary law of health is infringed; and as we find in all organised beings a tendency to return to the law of their life whenever it has been broken through, so we cannot doubt that in our own highly-developed organism there must be some means by which the law of normal alkalescence is preserved. This means seems to be provided in the kidney; and I think we are justified in concluding that it is not a mere passive outlet. It is quite conceivable that if the blood-serum became unusually charged with either acid or alkali, the excess would characterise every secretion, and that in the urine the fluid passing by osmosis would differ in this respect from that ordinarily passed only in the same degree as that in which the altered blood differed from healthy blood. But this is not so; and plainly such a result would fail to satisfy the requirement that the kidney should be the means of restoring the blood to a state of health. The intensely acid condition of the urine sometimes met with is an impossibility on the theory of its only passing by osmosis. The acid is actively and rapidly excreted, in order that the blood may be normally alkaline for the fulfilment of its ordinary functions.

Were further proof of this conclusion necessary, I think it would be found in the very remarkable fact already noted, that during the active and advancing period of rheumatic fever, if we pour alkali into the stomach to the extent of provoking free and rapid elimination by the kidney, acidity may still be traced in the perspiration. It has been usual with me to order the administration of the alkali every four hours at least; because if this period be exceeded, the urine will again become acid before the next dose is given. The kidney seems to seize the abnormal element in the blood, and expel it as soon as possible from the body. In order to reach this gland, the alkali must enter the current of the circulation, be fully mixed with the blood in the lungs, and distributed equally all over the body. Yet the sweat-glands may be secreting an acid fluid while the urine is intensely alkaline. My experience would rather go to show that when the perspiration has become alkaline, the disease is more under control, and is probably in the retrogressive stage; but individual experience is perhaps the most fallacious ground upon which an opinion can be based in the practice of medicine.

In speaking of excess of acid or alkali, I think it is not to be assumed that any such thing as free acid or free alkali exists in the blood or the urine. The reaction to test-paper is produced by salts in which the acid and the base do not fully neutralize each other, either because of their combining in varying proportions—one or more equivalents of base to one or more equivalents of acid—or in consequence of the reaction of one ingredient being too powerful to be neutralized by the other. The chemistry of life is so complex that we may not dogmatise or speak with too great confidence on a subject which cannot be brought to any scientific test. But it is a curious and somewhat inexplicable fact, that while there is this necessary elimination of alkali from the blood, in order to maintain its alkalescence at the normal standard, there is no restriction on the absorption of the alkali, which may be taken up in such quantity as to render the urine, and therefore the blood, alkaline to almost any extent. It seemed to me at one time that the solution of the difficulty might be found in another law of vital chemistry with which we are pretty familiar. By this law any complex acid may be resolved, by its exposure to the oxygen in the lungs, into a more simple form; and when carbon enters into the formation of the acid, one at least of the simpler acids resulting from combustion will be carbonic acid, which forms, with the true alkalies, a very stable salt in the blood, having a very decided alkaline reaction. It is well known with what extreme rapidity a dose of citrate of potash will render the urine alkaline. The salt is readily taken up from the stomach, quickly burned into the carbonate, and rapidly eliminated by the kidney, to restore the blood to its normal degree of alkalescence. If, therefore, there were any check upon the absorption of a salt having an alkaline reaction, we could present to the absorbent vessels a neutral salt, which would have the same effect in increasing for the time being the alkalescence of the blood beyond its normal standard. It seemed not impossible in considering the subject that the absorption of an alkaline salt might be due to its entering into combination with acid in the stomach, and passing into the circulation as a neutral salt. The proof of such an hypothesis must be extremely difficult to obtain; and for the present it can only be stated as a fact, that in the form of an alkaline salt, the alkali will pass in most cases as readily into the blood as in that of a neutral salt. In a succession of examples of acute rheumatism, I made the experiment with bicarbonate of potash alone, with citrate of potash alone, and with a combination of the two; and the result was, that on an average there was no difference in the length of time required to render the urine alkaline, so long as the cases selected were of equal intensity. When the disease was very acute, a longer time or larger doses were required to neutralise the acid. The conclusion forced itself on my mind, that however it may be explained, the alkaline salt is really taken up as such, and passes through the blood to the kidney unchanged, except it meet with any acid capable of entering into combination with it. From another point of view, the hypothesis that alkali could only be taken up in the form of a neutral salt seems also to be untenable. If it be assumed that free acid does exist in the stomach, and that its combination would aid in the absorption of the alkali, we should still be at a loss to explain how the urine was rendered alkaline, because it is only true of a limited number of acids, that they become converted in the lungs into carbonic acid, and the salt found in the stomach would in many cases pass unchanged through the kidneys, and give no alkaline reaction to the secretion.

As a matter of inference, it can scarcely be doubted that while the citrate or bicarbonate of potash is passing through the blood from the stomach to the *kidneys*, *if* the urine be alkaline, the blood will also, during the transit of the alkali be in a state of excessive alkalescence. The question then immediately arises, whether such a result is beneficial or the contrary. Excess of acid is one of the never-failing accompaniments of rheumatism. How far it is subsidiary to other changes it is at present impossible to say; we only know that it is abnormal. From other sources we learn some of the evil effects of excess of acid, though not necessarily seen in all cases in which acid is present, and it seems rational to try to avert such consequences, if possible, by neutralising acid. With this view, we pour large and frequent doses of alkali into the blood, in the hope that it will be distributed to every part of the body; but we find that, in spite of all our efforts, with the exception of the kidney secretion through which the excess of alkali is being constantly expelled, the acid continues to show itself wherever we can search for it; the other secretions continuing acid in spite of the excess of alkali in the blood. The endeavour to neutralise the acid is clearly rational, but it may be questioned how far we succeed in attaining our object. In fact, as rheumatism is not merely acid but an acid-generating disease, so till the disease ceases the tendency will go on; and yet, after all, we are probably doing a real good, though we cannot by treatment succeed in arresting the acid formation. Notwithstanding the passage of an excess of alkali through the blood and a highly alkaline reaction of the urine, the disease will go on for some time unchecked—fresh joints become involved, and even cardiac inflammation may be developed. In fact, whatever may be said of the alkaline treatment of rheumatic fever, it cannot be called curative in the proper sense of the term. It may help to shorten the duration of the attack, it may serve to lessen the intensity of the inflammation, it may diminish pain, and it may not unfrequently ward off those more serious consequences which give to rheumatic fever its formidable character as a cause of heart-disease. All this it may do, and in my own opinion, and that of many others who have made the experiment, it does actually accomplish, but it does not cure. It does not put an end to the paroxysm, as bark puts an end to an attack of ague; it does not eradicate the disease, as iodide of potash will eradiate the traces of syphilis, commencing as soon as the system is sufficiently saturated by full doses of the remedy, and continuing till complete recovery takes place, with a progress unbroken as long as the remedy is used, retrograding as soon as it is left off. It does not even expedite the recovery in the marked way in which it is expedited when colchicum is administered in gout in a case not yet blunted to its action. Ere long I doubt not sufficient statistical evidence will prove its beneficial action; but as far as I can judge, it never can be capable of any logical proof. I think that no one can fairly and impartially test it as a mode of treatment without coming to the conclusion that it is, on the whole, the best that has been suggested, but that it fails to cure the disease.

Among the large number of cases of acute rheumatism treated with alkalies, one cannot fail to remark that some of them give indications of depression, which can only be explained as the effect of the long-continued use of the remedy; and a question arises whether it is wiser to diminish the dose or risk the evil consequences of the depression, each having a tendency to make the case more protracted. Had we any positive rule by which to measure the quantity of alkali necessary, it would not be so difficult to decide; but there is certainly none such. Various means have been tried to meet cases of this kind. Stimulants avail to a certain extent, but have seemed to me to tend of themselves to keep up the disease, to lengthen its duration, and even to reëxcite it after it has subsided. Quinine is that which, on the whole, has answered best in my experience, and it is chiefly to its value that I wish now to direct attention. It is somewhat difficult to define the meaning of the term depression as applied to cases of this kind. The pulse keeps up, there is no delirium, there is no collapse; but there is a general impression conveyed by the symptoms present that the patient is unduly depressed, and it has been observed that in such cases the duration becomes protracted, and convalescence does not set in so quickly.

It is then that I have given quinine with apparent benefit. One must not wait, as in slighter ailments, for the tongue to clean before its administration. It is given simply to meet this one system, because we know that there is worse behind. If the prostration be not removed, delirium will follow, and the case will at once wear a graver aspect. It is true that we do not now look upon delirium as evidence of inflammation within the cranium. The theory that the dura mater, being like the pericardium in physiological characters, must have similar pathological tendencies, has given way before the teaching of actual pathological facts that there is no such metastasis as used to be assumed. Still, when present, it indicates changes the gravity of which cannot be overlooked. The blood can no longer stimulate the brain to the performance of its normal functions, and life is in extreme peril. Hence the importance of tracing such a change at its earliest period, and arresting it before it has reached that stage of development when nothing can be done beyond pouring down large doses of stimulants.

In hospital practice it is very easy to watch the daily changes taking place in the urine in cases of this kind, and I have been struck with the apparent coincidence of nervous depression and phosphatic deposit in the long continuance of the alkaline treatment. When first seen, the urine of a patient suffering from rheumatic fever is characterised by intense acidity, with a copious deposit of reddish lithates. After a few large doses of alkali, it becomes perfectly clear, but of a dark colour; as treatment proceeds, the tint resembling brown sherry changes to amber, and ultimately to straw colour, and sooner or later a deposit is seen which it requires no special chemical knowledge to designate as phosphatic, with alkaline urine. This coincidence has been so constant that I have come to regard the presence of such a sediment as the most certain evidence of depression.

Apart from the conclusion thus arrived at, that in such cases quinine was indicated with a view of correcting the depression of which the presence of phosphatic deposit became the sign, I had previously observed that in prescribing quinine, if there were any perceptible increase of the earthy salt prior to its administration, the deposit very frequently disappeared under its use. For some time the rule seemed so absolute that I did not meet with any exceptions. More recently it has been seen that in a certain number of cases the quinine fails to arrest the chemical change.—*St. George's Hosp. Reports, vol.* 6, 1873, *p.* 101.

13.—ACUTE ARTICULAR RHEUMATISM TREATED BY ACIDS.

Cases under the care of Dr. Wilks, at Guy's Hospital.

In recently making some comments on the treatment of acute rheumatism, Dr. Wilks expressed a considerable distrust of the ordinary methods. He remarked that we do not yet possess an accurate knowledge of the essential cause of the disease, nor of the mode of its operation. We are, in fact, ignorant of the natural history of the disease. Medical practitioners are too apt to be credulous in matters of therapeusis, and are not sufficiently careful always to differentiate between effects and coincidences. Usually, for instance, the diagnosis of acute articular rheumatism is regarded as a sure and certain indication for the exhibition of alkaline remedies, and these remedies are persisted in in every condition or modification of the disease, without the slightest evidence of a beneficial influence resulting therefrom. Nay, it frequently happens that the more severe the disease the stronger does the belief in the particular remedy become, and medical men not uncommonly allege in support of the efficacy of the treatment that the complications were of the most serious nature. If the patient recover after having had severe heart disease or symptoms referable to an affection of the nervous system, it is regarded as proof positive of the value of the remedy. Whereas we can not only not be certain that recovery did take place as a result of the treatment, but that it did not occur in spite of the vaunted remedies. On the other hand, some have asserted that there is no known cure for acute rheumatism, and that mint-water will answer every purpose. This is Dr. Wilks's opinion, and it was merely to strengthen this view that he gave mineral acids in the subjoined case, never *supposing* that they would do good or harm. Dr. Wilks maintains that, until we know everything concerning the disease, and especialy the relation between the joint affection and the internal complications, we have no scientific basis for the selection of remedies. It is even doubtful whether the subsidence of the arthritic inflammation during the course of the disease is desirable, for in all fatal forms of rheumatism this is usually seen to occur. It may be remarked, however, that sometimes, in cases of internal complication, it does seem as if large doses of potash or of quinine have some influence in arresting the disease. In several cases treated by quinine, Dr. Wilks has noticed the urine to become alkaline on recovery, as if this were the natural process of cure; as is seen also in typhoid fever. The same thing occurred in the present case.

For the following notes we are indebted to Mr. J. C. Ferrier.

G. W., a well-nourished man, aged thirty-five, was admitted on January 14th. A week before this he began to suffer pain in the hip-joints; subsequently both knees, and the right shoulder, wrist, and hand were affected. The pain in the joints was preceded by a feeling of coldness, but there was no actual rigor.

On admission the left knee was swollen from the presence of fluid within the joint. The right hand also was swollen and painful. A systolic bruit could be distinctly heard over the base of the heart, and extending a short way along the aorta. A venous hum was audible at the root of the neck. Perspiration was acid; temperature 101°; pulse 96; respiration 24. Tongue moist and covered with a white fur. The urine was acid, and contained a large quantity of lithates.

Ordered fifteen drops of dilute nitro-hydrochloric acid every four hours, and a diet to consist of milk, bread, and beef-tea.

Jan. 16th. Temperature 101·8°.

19th. Joints previously affected are better, but the right hand and both knees are still painful. Systolic basic bruit still very loud.

20th. Temperature 102·8°. Perspires freely.

23rd. Hands and knees almost well. Temperature 101·2°.

28th. None of the joints are swollen, but some feel stiff, and sore. Bruit hardly audible. Urine alkaline.

Feb. 2nd. Bruit almost entirely gone. The patient is convalescent.—*Lancet, February* 14, 1874, *p.* 231.

DISEASES OF THE NERVOUS SYSTEM.

14.—ON NERVOUS MIMICRY (NEUROMIMESIS) OF DISEASES OF THE SPINE.

By Sir James Paget, Bart., F.R.S., Consulting Surgeon to St. Bartholomew's Hospital.

[Cases of nervous mimicry of diseases of the spine are nearly as frequent as the imitations of diseases of the joints.]

The chief things to study in the spine are pain, stiffness, weakness, and deformity.

As to pain,—spineache and backache, often not distinguishable, are perhaps the most common sufferings of the whole class of hysterical and other such nervous patients. Few escape them. But, then, similar pain is so common in other patients besides the nervous, and is found in so many and so various diseases, that pain alone can rarely decide a diagnosis. I must leave out of the question all the "backaches" that belong to lumbago, rheumatic gout, uterine, ovarian, renal, and other maladies, and try to answer for you only this: Are there any characters by which we may distinguish between pain that signifies real disease of the spine and pain that mimics such disease?

Well, we cannot always do so. There is no pain which is characteristic of real spinal disease—no manner of pain which may not be closely mimicked; but, in many cases, the nervous mimic-pain has characters which are not at all found together in real disease of the spine.

Nervous spineache is often described as very severe—raging, racking, as if the back were breaking, and so on. Now, such pain as this does not occur in real spinal disease, unless when the pain of some slight disease is immensely exaggerated in a nervous constitution, or in the acutest form of inflammation of the vertebræ—a very rare disease, always associated with serious general illness and impaired mobility of the spine. If there be no considerable attendant illness, an intense and horrible pain in or at the spine does not mean serious disease of the spine. I do not say that such pain is always merely "nervous" when it is the only wrong complained of. It may be due to aneurism, or cancer, or to some distant malady in a nervous constitution; but, unless in the two cases I just now mentioned, it is not a sign of spinal disease.

And this negative becomes the stronger when with such a pain there is excessive tenderness of one or more parts of the spine. Some tenderness on pressure of spinous processes may be found with real disease of the spine or cerebro-spinal membranes; but excessive tenderness is not. This is rather characteristic of merely nervous disorder, of the so-called spinal irritation, and usually you find it, not at one, but at two or more parts of the spine, most frequently between the scapulæ and at the loin. At these tender spots the nervous patients cannot bear to be touched; they flinch and writhe when the finger taps or presses them very gently. You may be nearly sure that there is no disease of the spine when you see this, or when the tender parts of the spine are not painful in moving or in coughing or sneezing. And you may be quite sure, I believe, when a light blow or pressure produces more pain than a hard one, or when you find the same pain or flinching if the skin over or near the spine is pinched without pressing on the spine itself.

Again, the merely nervous pain is usually variable, though it may be never wholly absent; and its variations seem to be more dependent than those of real diseases are on disorders of distant organs, as the ovaries or uterus, the colon or rectum. In these variations the pain may seem paroxysmal, but I think it is not often so of itself.

Another usual character of this pain is its frequently extreme increase in fatigue, not only in bodily fatigue, as in long sitting upright or standing, but in long occupation of any kind, even in mental occupation if it is not very agreeable. With this character, also, it is common for the pain to be accompanied by nausea or even vomiting, and it is a nearly sure sign of merely nervous disorder if pressure on the spine produces shivering or nausea, or a feeling of going to be sick.

And, lastly, it is a sign of nervous pain alone if the pain has lasted many weeks and months, and nothing has come of it; no immobility of the spine or ribs, no paralysis, no fever, or wasting, or great disturbance of the general health.

I think you may be sure that a patient in whom you find all or most of these characters of pain in the spine has not spinal disease. But you will find many with pain like enough to this to excite suspicion, but not nearly like enough for conviction; many in whose cases the diagnosis must rest much more on other signs than that of pain. Among these, let me warn you that the greater danger is that of thinking cases to be mimicries of disease, or "muscular," or "only rheumatic," when they are really serious organic diseases of the spine or of parts within it. I will try to diminish the danger of being wrong in either direction by mentioning some of the more frequent groups of cases difficult of diagnosis.

The curvatures of the spine that occur frequently in young persons are often painless, are seldom very painful, and have no characteristic pain; yet pain of any kind should always lead you to examine for curvature, and to suspect, if there be curvature already, that it is increasing. And the same rule should be observed with older patients; for it is not extremely rare for lateral curvature to begin after middle age nor at all rare for it to increase at any *period* of life. In either case there may be pain, for which the spine should be often looked at for signs of disease more characteristic than the pain alone can be.

Aching spines are common in men with very nervous constitutions—in such as might be called hysterical with as much justice as many women are. Especially they are common in such men from thirty to fifty or more, and in women who retain their nervous or completely hysterical constitution to the middle or later times of life, and in whom, very probably, it is mingled with gout. There are far more cases of this sort in the cultivated classes of society than you are likely to suppose. Men and women of mature, or post-mature age, with spines whose nervous achings disable them from active life—at least from active bodily life; for in some of them the mental life is as active as if it really took their whole nervous force for its own use. They suffer severely, are painfully fatigued with exercise, have no comfort unless when they are lying down or reclining with their backs firmly supported; even slight manual work tires them: and yet in some of them the mind seems incapable of fatigue, or it only tells its fatigue in some increase of their spineache, or in feelings of nausea or vomiting. In many of these cases the diagnosis is not difficult: the nervous constitution is very strongly marked, and, after months or years of weariness and ache, you find the spine as straight, strong, and pliant as ever.

And yet among these are the chief cases in which you must be on the watch, lest you overlook a really diseased spine; for, among elderly persons, you may not only, as I have said just now, find cases of beginning or increasing lateral curvature, but, occasionally, one of disease leading to angular curvature, setting in very insidiously like a case of nervous pain or chronic rheumatism. The first sign of this is, usually, a constant aching, which is sometimes even severe, fixed to one part of the spine, or radiating from it, or extending round the sides; and there may be some tenderness. But, chiefly, you may know them by the diminished mobility of the affected part of the spine and the adjacent ribs. The patient holds the affected part of the spine as still as he can, when he turns or bends. If he coughs he does it carefully, and sneezing is very painful. If the dorsal portion of the spine be affected, the corresponding ribs scarcely move; if the cervical, pressure on the top of the head is often painful. After some few weeks of such pain, yielding of the spine may become evident. If the patient is the first to notice it, he feels himself stooping, or obliged to support himself with his hands, or not so tall as he was; and, when you strip him, you may find two or three vertebral spines projecting in an incurable angular curve.

These things, you see, are much the same as those by which you recognize the progress of many cases of caries of the spine in children and young persons, and I suppose that this disease in elder persons is of the same kind. But I have not yet seen a fatal case or one examined after death; and I have only seen one in which abscess was associated with the angular curvature.

You may make another group of cases from other patients, in whom a strain of any part of the spine is followed by a very long abiding pain. Some of these will tell you that at the strain they felt, or even heard, something crack, and that they have had pain and weakness ever since—aching, wearing pain, increased by exercise, or in certain postures. Here again I must advise caution, especially when the injury is recent. In a large majority of these cases the long-abiding pain and other signs of spinal disease may be referred to the same nervous mimicry as we see in those whose sprained knees or ankles remain for weeks or months painful and weak, yet without disease. But in some the pain continues because, as may happen in an injured joint, the injury has been followed by inflammation; and this may be of serious form. The pain alone will not prove it; but you may be afraid of it when you find diminished mobility of the spine or ribs, great pain in coughing, greater in sneezing; and much more afraid when you find occasional or constant fever, and loss of weight and general strength. In short, for the diagnosis between the real and the mimic diseases following such injuries of the spine, you may study by the rules which are more easily learnt in the similar affections of the joints.

It is happy for our need of diagnosis that the impairments of the mobility and strength of the spine are more significant than its pains. The spine or any part of it, when really diseased, is often stiff and weak, though other signs of disease are absent or ill-marked; in the mimicry the mobility is often perfect, though other signs are vehement.

Fixity of the spine or any part of it is extremely rare in any nervous mimicry. I will not say that it is never seen, but it is so rare that when you find a patient carefully—not with muscular spasm, but with care—holding his head or neck or any part of his back very still, turning himself or bending cautiously, you must look for disease of the spine. There may be muscular rheumatism, or inflamed cellular tissue, or abscess, or whatever else may give pain in moving; but there is not likely to be only a nervous mimicry of spinal disease.

This fixity of spine is the more significant of real disease the less the attendant pain. Still more so is it when the ribs connected with the suspected and stiff part of the spine are also motionless or too little moved in breathing, and when the breathing is chiefly diaphragmatic. There may in this case be disease within the chest—perhaps the pleurisy that is often associated with acute inflammation of the dorsal part of the spine; or it may be very uncertain what disease of the spine there is; but it is very nearly certain—as certain, I think, as any diagnosis of unseen disease—that there is not a mere mimicry. Whenever you are told of "spinal irritation," "hysteric spine," or whatever else an unreal disorder of the spine may be named, look carefully to the mobility of the spine and ribs; if it is impaired, look much further before you venture to conclude that the malady is only nervous.

The very opposite state of the spine, in which it is all limp, so that when the patient tries to sit up, he, or more often she, bends or tumbles this way or that, like a baby, is nearly certain to be without organic disease. There may be real paraplegia; if there be not, you may believe the weak tumbling spine is in itself healthy, though it may contain an idiotic spinal marrow; as a good skull may hold very foolish brains.

A feeling of weakness or giving way at one part of the spine is ambiguous. It is often complained of in neuromimesis; but it is also often present in carious or other real disease of the spine. It must always be considered likely to be a grave symptom if the patient habitually, and almost unconsciously, helps to support the spine with the hands or elbows as he sits. Of course this self-supporting posture may be either unintentionally or on purpose imitated in a nervous mimicry; but it so often has a real meaning that it must not be made light of, unless all other evidences of mimicry are clear.

You will often find that with this weakness of one part of the spine there is some distress in moving it. The patient does not willingly rise or turn in bed, and if he walks he does it slowly, often stooping or leaning forwards, going stiffly or shuffling, not staggering or unsteady, unless the spinal marrow be involved in the disease. This sign is, I think, less rarely mimicked than the last; the two together are weighty evidence for real spinal disease, and if defective movement of the ribs be added, you had better believe that the spine is certainly diseased; whether with struma or rheumatism, or whatever else.

The other leading sign for the diagnosis between real and mimic diseases of the spine is in its shape. Is it misshapen, wrongly curved, or in any way deformed? If so, it probably is or was really diseased; and yet even here there is room for error.

An angular curvature of the spine—I mean such backward outstanding of one or more vertebræ as is due to thinning or loss of substance of their bodies or intervertebral discs—is, I believe, quite inimitable by any nervous or muscular condition. But in not a few persons you will find that one or two vertebral spines naturally project a little, or are placed a little to one side of the exact line or curve in which they should be. This is most often seen in the lower dorsal and lumbar part of the spine; but it is so little like disease that it would be unnoticed if spineache or some fright about curved spine did not call attention to it. I think you will seldom have any difficulty in distinguishing this natural error from any effect of disease.

A lateral curvature of the spine may be imitated by disorderly muscular action; not, indeed, perfectly, yet nearly enough to be often deceptive. Spasmodic wry-neck is well enough known, and may imitate the distortion of considerable disease of the cervical part of the spine. Lateral curvature of the dorsal and lumbar parts from similar muscular disturbance is much rarer, but you may expect to meet with it; and you may often detect the mimicry by noticing that the curvature has formed very quickly or even suddenly, and has become marked or extreme in so short a time as could not have sufficed for a real lateral curvature. A few days will make an imitation-curvature stronger than as many months will make a real one. Besides the mimic curvature is not a perfect likeness. It is sometimes single, though very marked; real lateral curvatures, if very marked, are at least double; and the vertebræ are little or not at all rotated, as they are in well marked real lateral curvatures. If these signs of distinction are not enough, ether or chloroform will help. You can straighten the mimic curvature when the muscles cannot act; you cannot so straighten a real curvature.

Other deformities of the spine may be imitated by hysterical and other such persons. The spine may be rotated without curvature, or drawn to one side, so as to go up obliquely from the pelvis; or in those with very weak limp spines, there may be, when their trunks should be erect, an appearance of an uniform posterior curvature. But I believe you may easily detect the mimicry in all these. Most of them are extravagant, going beyond almost any real deformity of the kind that such a patient could have; and, almost always the wrong may be righted by putting the patient in some unusual position, as lying very flat on the back or front, or with the hands touching the feet, or hanging on a swing. By these, or the like means, you may out-trick the trickery of the muscles.—*Lancet, Nov.* 29, 1873, *p.* 763.

15.—ON THE NERVOUS MIMICRY OF TUMOURS.

By Sir James Paget, Bart., F.R.S., Consulting Surgeon to St. Bartholomew's Hospital.

[There are three principal forms of nervous mimicries of tumours, viz., the muscular or phantom tumours, the imitations of aneurisms, and those of cancer.]

In what are called phantom tumours the imitation is due to the swelling of part of a muscle during contraction. This part, swelling and becoming harder than the rest, feels something like a smooth round or oval tumour or cyst, placed in or between the muscle. The rest of the muscle feels natural, whether relaxed or contracted. In the former state of the muscle the apparent tumour is most distinctly felt; in the latter state it may be nearly concealed, as if sinking in the muscle.

This condition of muscle is altogether a strange one—not imitated, so far as I know, in any other state of muscle, unless it be in crimping, or in the flickering and passing contractions of dying muscles, or in the lumps, which, it is said, may form in muscles that are struck soon after apparent death in any acute fever. If it may be compared with anything in living muscles, it may be with cramp which draws them into knots or lumps; but there is no pain in phantom tumours as there is in cramp. Whatever it may be, the condition seems due to erroneous nerve force in the muscle; for one form, the phantom tumours of the abdominal muscles, is most frequent in evidently hysterical women, and in other cases that I have seen there was certainly an abnormal state of nervous system, and in others it was very probable.

There should be no great difficulty of diagnosis in the ordinary cases of this kind. A real tumour that lies deep in or beneath thick muscle may be pressed down beyond clear touch when the muscle contracts and hardens; and it may be difficult to feel the boundaries of a venous tumour or nævus in a deep or thick muscle. But these things will not mislead you if you will study the case long enough to be clear as to the inconstancy of appearance. If in one minute an apparent tumour is under the fingers and in the next minute is gone; if it shifts from one part of a muscle to another, as one set of fibres after another contracts; if it wholly disappears when the muscle is long relaxed,—this is a phantom, a mere mimicry of a tumour.

There is rather more difficulty in some of the cases of phantom tumours which occur in the abdominal muscles of hysterical women. These are sometimes large, hard, and more nearly fixed in place, size, and form, than those in the limbs, and they may be deceptively complicated with disorderly states of the intestines, or the aorta, or other abdominal or pelvic organs. But in any case, however difficult of diagnosis, ether or chloroform will bring all the help you need. With complete insensibility and muscular relaxation all signs of tumour disappear—the phantom vanishes.

Let me tell you the most striking case of this kind that I have seen. It was in a healthy-looking woman, about thirty years of age, in Sitwell ward. I admitted her for a tumour in the upper part of the abdomen, behind, as it seemed, the first division of the left rectus abdominis muscle, but larger than that seemed. It was roundish, firm, nearly hard, constant in its characters and place, often painful, and distinctly pulsating, like an aneurism. A light bruit was audible in it. Of its history I only remember well that there was nothing to help in diagnosis. In full medical and surgical consultation with my colleagues, the questions were whether there was a tumour with pulsation communicated from the aorta or an aneurism. If a tumour, where, or in what? If an aneurism, of what artery? The opinions were many and various, partly, perhaps, because the examination was painful, and, therefore, possibly, incomplete. So one day I gave the patient chloroform, and the tumour, the aneurism, and the doubt dispersed; they were all phantoms.

This case, in which an abnormal nervous condition of a muscle and of the abdominal aorta appeared to be combined, may lead us to the next set of mimic tumours—the imitations of aneurisms by what I suppose to be abnormal nervous conditions of arteries. The evidences of this condition are, indeed, not complete; but, at least, the mimicry of aneurism is more frequent in persons of nervous constitutions than in any other. It is sometimes associated with well-marked hysteria, and the likeness of aneurism often varies, according to the state of the patient's nervous system.

This mimicry is most frequent in the abdominal aorta, in which it has often been described as a nervous abdominal pulsation; but, so far as I know, it is not described as occurring in the subclavian, innominate, or carotid arteries, though in those it is not very rare, and sometimes is not easy of diagnosis. The chief characteristic of the mimicry is that the affected artery feels much larger than it should be, and pulsates largely at least in one direction, dilating obtrusively, and often so much more visibly than in an ordinary person, that it is hard not to believe that it is largely dilated. And yet there certainly is no considerable dilatation; it is rather as if the arterial walls were thin and had too little muscular resistance, as we might suppose them to be in a condition of partial paralysis of their muscular tissue. In the most marked case of imitation of abdominal aneurism that I have seen, examination after death found no disease. A lady of very nervous constitution had suffered severe sea-sickness in a passage to Ireland. The sickness scarcely ceased during her stay there, and was aggravated on her return voyage, after which she continued vomiting nearly all her food, till, at the end of some weeks, she died exhausted. During these weeks there was so large and strong a pulsation in her abdominal aorta that some were convinced that she had abdominal aneurism. As she lay on her back, one could see the artery pulsating behind the abdominal wall. It raised the hand laid on it, thrusting up the fingers with a firm throb and a thrill, and a low but distinct blowing was audible when the stethoscope was lightly pressed on the artery. Yet there was no aneurism. After death the artery was found absolutely healthy in both size and structure.

I say again, I do not know what is exactly the state of the arteries in these cases. There are few opportunities of examining them after death; and I have heard of none that told more than the one I just now mentioned to you. But, clinically, these cases are well marked. The artery feels large, wide, and full; but soft and compressible, or even, if one may so call it, puffy, without any of the hardness or stiffness constantly found in aneurisms. The pulsation is full, but rather soft—like that of an artery in the reaction after large loss of blood; and, which is chiefly characteristic of the absence of dilatation or aneurism, the extra full pulsation is in only one direction. There is no unusual lateral dilatation; the too much throb is only forwards.

The likeness to aneurism in these cases is sometimes greater than could well be expected; for many things besides the excess of pulsation may contribute to it. In the abdominal aorta an unusual appearance of prominence may be given by an unusual anterior curve of the lumbar vertebræ; and by thinness with concavity of the abdominal walls, and emptiness of the stomach. Hardness or toughness of the pancreas, or of lymph-glands about the aorta, may yet further increase the likeness to abdominal aneurism.

In like manner, the imitation of subclavian aneurism is made more marked when a nervously pulsating subclavian artery has glands beneath or about it; and still more when it lies on a cervical rib, and is somewhat raised and, I suppose, widened. And the imitation of upper carotid aneurism is added to by the very bulbous form of the first portion of the internal carotid and its occasionally tortuous course. That of the lower carotid aneurism may be augmented by an enlargement of an overlying lobe of the thyroid gland.

With all those helps to difficulty you may sometimes be troubled in the diagnosis of a mere nervously pulsating artery. In nearly all the cases I have had to see aneurism was suspected; but a few rules will ensure against error. The nervous artery has no lateral expansion; it does not open the finger and thumb when they laterally compress it lightly; you can trace the straight lines of its sides; in the abdominal aorta the pulsation is lost, or nearly lost, when the patient sits leaning forward; in the other arteries by relaxing the parts over them. Always the artery has at least its natural softness and compressibility; commonly it has more. There are no paroxysms of pain: and if the case has been watched for months, or even years, there is little, or more often no increase of size. On the other hand, the extent of artery which may feel dilated may much increase, so that we may see and feel, for instance, an abnormal throbbing in the innominate, carotids, and subclavians of the same patient —a state unheard of in aneurisms.

Before leaving this subject, I just mention to you the cases in which you will find, in certain patients, anæmia, enlarged thyroid gland, protruding eyeballs, and pulsating carotid arteries. The study of these cases might clear the obscure pathology of the mimic aneurisms; but with this I am not occupied, so I leave them, and go on to the last set I have to speak of.

It may seem absurd to say that cancerous tumours can ever be imitated by any nervous disorder; and, indeed, they cannot; yet you will find few cases requiring a more positive diagnosis than those in which you will have to assert, as beyond all doubt, that a patient has not cancer, but neuralgia.

The cases in which the diagnosis is most often necessary are those of suspected cancers of the breast and of the tongue. Of course any part may be similarly suspected; but, taking all other parts together, they would not supply, at least in surgical practice, so large a number of cases of suspicion as these two.

For the breast, the usual case is that a patient complains of all the pains commonly described as characteristic of cancer. She has dull, aching pain, it may be, of the whole or some part of the breast, and often the pain extends down the arm, more often goes right through to the scapula; and sometimes it is a darting, lancinating pain, shooting this way or that, or burning. I think that you will not find any pain of cancer of the breast which will not be described by patients with this neuralgic mimicry of the disease.

But, however severe the pain, and however like what is supposed to be characteristic, your diagnosis may be clear. That is not cancer which you cannot feel. Cancer is a growth, not any kind of pain, and indeed in its early stages seldom attended with pain; so that when pain is strongly marked, and it is very difficult, or impossible, to feel any tumour or "lump" in the breast, you may be certain that there is no cancer.

In most of these cases the pain has an entirely mental origin. The patients have been seeing or hearing of cancer, and their minds have been filled with thoughts of the pain and misery of the disease, till the idea has generated the sensations from which it was derived, and the subjective sensations are referred from the centre to the periphery.

I have said "the mind has been filled;" but this is not necessary. I have known these pains mimicking the pains of cancer in persons who had indeed often thought of cancers, but never intently, nor with any sustained fear. Just as one may, in dreaming, have vivid impressions of objects he has rarely, perhaps only once, seen, and scarcely ever thought of, so may these imitative pains be felt keenly and often in those who have seldom considered or feared them.

In these wholly mental cases the confident assertion of your judgment will commonly suffice for cure. Sometimes, indeed, patients need repetition of the assertion, especially those who are hysterical, &c. Their trust seems to die out after a few weeks or months, and some of them rather prefer the disease.

There are cases, however, in which the pain is not wholly mental. Neuralgia of the breast sometimes follows a blow. That which in ordinary persons would be followed by aching for some inconsiderable time is apt to be followed in the nervous by long-abiding pain, which the mind will invest with all the characters of what it supposes to be the pain of cancer. And, again, neuralgia of the breast is not rare in gouty or otherwise "painful" persons, and most of these are ready to fear that their pain is due to cancer, and to endow their pain with the characters which they suppose to belong to it. As fear can give to any harmless form the shape and substance of that which it most dreads, and can see spectral terrors in a scarecrow, so can it give to ordinary neuralgic pains any of the characters of the pains of cancer. It is especially prone to do this when the neuralgia is felt in a part notoriously liable to cancer. In this part fear will make pain cancerous, even while neuralgic pains, quite as severe, are common in other parts of the body, and in them are regarded without dread.

The diagnosis is more difficult when, with the pain described as like that of cancer, there is, or appears to be some change of structure of the breast; for of course a very nervous patient may endow any mammary disease with any form of pain.

In these cases—which, however, are far more rare than those of neuralgia without change of structure—you must make your diagnosis on the characters of the tumour itself, if there be one. But be cautious about one or two fallacies. Many neuralgic breasts are a little larger than their painless fellows. But mere largeness is not tumour, much less cancer. And many mammary glands, whether neuralgic or not, are rather firmer or tougher in one part than in others; and when the tough part is grasped, and, as it were, doubled between the thumb and fingers, it may feel something like a hard tumour. Let me advise you not to use this mode of examining a mammary gland. I have known it often fallacious, never useful. If you cannot feel a tumour by feeling or pressing over and round it, you may believe there is not one; you may be certain there is not a hard cancer.

And if you can feel a tumour, do not believe it to be cancerous merely because it is painful. Judge by what you can feel, not by what the patient feels; remembering always that in the early stages of cancer of the breast, in which alone there is any difficulty of discerning it by its tangible characters, it is rarely very painful. All the popular ideas of the pains of cancer are derived from the cases of advanced disease, or of cancers growing rapidly or inflamed or ulcerating. In early cases, and in nearly all in which the diagnosis is obscure, the greater the pain the less is the probability of cancer. And, let me add, neuralgia, even of the severest kind and longest duration, does not tend to cancer of the breast. I must have seen more than a hundred cases of neuralgia, and I have seen only one in which it was followed by cancer, and I have no belief that in this one the sequence was a consequence.

Most of what I have said of the breast might be applied, with change of name, to the mimicry of cancer of the tongue. Nervous people hear of the cancer, and then, with less or more of thought about it, have pain and aching in their own tongues, usually at the side and in that frequent seat of cancer, at the junction of the middle and posterior thirds. Others have neuralgia of the tongue; a dull, heavy aching on one side of it—a condition which is far from rare in the habitually neuralgic, especially, I think, in the gouty, and these give to their neuralgic pains what they believe to be cancerous characters. And others, with dyspepsia, have sore tips or edges of their tongues, which they, too, magnify.

It is strange to observe the tenacity with which some of these patients cling to the most dismal view of their cases. Though nothing wrong can be seen or felt, and though months, or even (as I have known) years, may pass without any disease appearing, yet will they believe themselves on the brink of misery with cancer of the tongue. But your diagnosis may be as fixed as their fears, and had better be more positive. That is not cancer which you can neither see nor feel. And do not be deceived by an unusually large cluster of papillæ of the circumvallate group, or by any little thickening due to irritation of a tooth, or by a fissure or psoriasis. A patient's fear will give any of these the sensations of cancer; but, as I said of the breast, so here, your diagnosis must be founded, not on the patient's sensations, but on your own.—*Lancet, Dec.* 13, 1873, *p.* 833.

16.—ON FEIGNED HEMIPLEGIA.

By Dr. ALEXANDER DAVIDSON, Physician to the Liverpool Northern Hospital, and Dr. CHAUNCY PUZEY.

The subject of feigned hemiplegia has been only very cursorily alluded to either in standard works of general medicine or in writings specially devoted to the consideration of feigned diseases. So far as we are aware, no detailed history of any such case has been recorded in medical literature, and we think that the following well-marked example will be both interesting and valuable.

Not long ago we were requested to visit, on behalf of a railway company, and to report upon the case of a young man who was believed to be suffering from hemiplegia, the result of a railway accident. The history we received was as follows:—

Eight months previously in a collision, he had received a blow on the left side of the body, which was stated to have been followed by hæmoptysis and the appearance of blood in the urine and motions. For this he was leeched, poulticed, &c., and remained under medical treatment for five weeks, when he was brought to Liverpool. At this time no symptoms existed of any injury to the brain or spinal cord; but two months after the accident, we were informed he began to complain of pain in the dorsal region of the spine and of numbness of the left leg. He was kept in bed under medical observation; and one night, four months after the accident, he was found doubled up in bed, staring about, struggling and shouting. This attack was stated to have continued for seven hours, and he required several men to hold him still. Similar attacks recurred every night for a week following. After this the left leg became powerless, and weakness of the left arm was noticed; and the symptoms gradually increased until he appeared to have total loss of sensation and of motion on the left side.

We saw the patient in the presence of his medical attendants, and made the following report on his condition:—

We found the patient in bed, lying on his back. His countenance was rather pale, but did not exhibit signs of serious ill-health. The pulse was somewhat rapid, but regular and of fair strength. The body appeared well nourished. The heart, lungs, &c., were examined and found normal. There was no facial paralysis; the tongue was protruded normally, and articulation was perfect. Memory and intelligence appeared unimpaired, his answers to all questions being ready and sensible. The pupils were normal, and the vision appeared good. His own statement was that he suffered from headache, that his memory was not so good as formerly, and that his vision and hearing on the left side were somewhat impaired. In all these respects, however, he said that improvement had now begun. He admitted that he usually slept well, and said that he occupied a considerable portion of his time in reading. He exhibited signs of much tenderness when the lower dorsal vertebræ were touched, and complained of pain extending from that locality towards the left side; but he said he had no feeling in the skin all over that side of the trunk up to the clavicle, and pricking with a needle failed to elicit signs of pain. When raised in bed, he was able to sit erect with both his hands in front of him, and without assistance. The skin of the back was sound, and there were no bedsores. No paralysis of the bladder or rectum existed. We were told that the bowels acted three times a week, and that on those occasions he went to the closet outside his bedroom, with the assistance of one person.

On proceeding to examine his left leg, which was stated to be completely paralysed both with regard to motion and sensation, we found no difference in appearance between it and the right limb. The muscles on each side were equally firm and well developed, and the measurement of the two legs corresponded in every particular. He professed to be unable to move the left leg to the smallest extent, but when the leg was raised from the bed and bent backwards and forwards, distinct muscular contractions were felt by us. On applying a weak faradaic current, we found the electro-contractility of the muscles perfectly normal. On pricking different parts of the leg with a needle, no sensation appeared to be excited, and no reflex movements were caused by this or by tickling the sole of the foot. On one occasion, however, at a later period of the examination, when his leg was surreptitiously pricked under the bedclothes, his attention being drawn away by a feigned examination of his eyes, a decided jerk of that leg took place; at another time, when pricked unexpectedly on the inner side of the left buttock (as he lay upon his face), he flinched most decidedly; but after this was openly remarked on and the pricking was repeated in the same spot, no movement occurred.

In order to observe his mode of progression, he was had out of bed by the aid of his brother. He then stood on his right leg, with the left leg slightly bent at the knee, the heel drawn up, and the toes scarcely touching the ground. He then proceeded towards the door, supported by his brother and holding on to the bed. With this help he made a series of steps with the right foot, and at each step we observed that the left leg was jerked up (by muscular action) just as occurs in hopping on one leg. The dragging of real paralysis was wanting. As he was getting into bed again the knee of the affected side was bent almost at a right angle, before it touched the bed.

The paralysis of the left arm was stated to be complete with regard to feeling, but incomplete as to movement. Both arms appeared equal as to size, muscular development, and temperature. On pricking the left arm with a needle, no pain was complained of, and no reflex movements were excited. On being asked to show to what extent he could move that arm, he raised the forearm slightly, but used the help of his right arm to raise it at the shoulder. The muscles contracted well under the faradaic current.

He was again seen by us after a lapse of three months. The general characters of his condition remained, with the following alterations. The left arm was stated to have recovered much of its sensibility, and the power of movement had partially returned. He was also able to move the toes of his left foot to a slight extent, and said he had some feeling in them. He still appeared to be unable to stand or walk. A marked difference, however, was noticed in his manner of progression from what we had previously observed. The heel of the left side was still slightly drawn up as he moved, but now the toes were dragged along the ground, the right knee being also slightly bent. The following circumstances were noted as he lay in bed. On attempting to bend the knee while we elevated the leg, he at first forcibly resisted, the extensors of the thigh and leg being felt to be strongly in action; after a few seconds, however, they were suddenly relaxed. The skin of the thigh seemed to be as insensible as before, the patient even running a needle into different parts of it himself, to show how little he felt. When turned over on to his face he appeared to forget which was the affected side, for, seizing his *right* buttock and pinching it hard he declared he felt nothing. On his mistake being referred to, he insisted that it was the other side which he had pinched. The impression left by his fingers, however, remained evident on the right buttock. The feet were clean, but the soles were dirty, as if used on the ground (though he stated that he had been absolutely confined to bed for some months). The skin of the soles was smooth and hard, and there was no appearance of that crackling or scaling of the cuticle which we see in the soles of adults who have been confined to bed for several months.

The conclusions which we arrived at as the result of these examinations were:

That though some injury of the side had been received in the accident, its effects had entirely passed away; that no disease of the brain or spinal cord existed; and that the hemiplegic symptoms were feigned.

The case was tried in one of the law courts. The medical attendants of the patient and a physician (who saw him shortly before the trial) gave evidence that in their opinion the paralysis of the arm and leg was real, that it was due to injury of some of the nervous centres, and that the man was probably permanently disabled. In consequence of the difference between their medical opinion and ours, a further examination of the plaintiff was made during the trial, in the presence of another physician and a distinguished surgeon; and on this occasion some entirely new features appeared. For the first time, loss of sensation was complained of in the left side of the face, neck, and tongue, but without any dragging of the face or deviation of the tongue when protruded. The mode of progression was again different, the dragging of the leg being much more exaggerated than before, and the patient making no effort to support himself on the sound leg, but allowing himself to fall down if unsupported. It was also noticed that when the patient's eyes were closed the apparent line of insensibility, as tested by the needle, varied from a quarter of an inch to an inch to the *right* of the median line. The opinion given at the trial by the two gentlemen who had now seen the case for the first time was that it was a case of hysterical paralysis; but nothing had occurred in this third examination to shake our opinion. However, the jury gave a verdict for the plaintiff, with substantial damages.

Not very long after the trial he was seen by one of us walking about apparently quite well, and when spoken to, admitted that he was "nearly well, thank God."

To the medical practitioner in ordinary or in hospital practice such cases of feigned disease do not frequently occur; and when they do appear, he has usually ample opportunities for detecting and exposing the fallacy of the symptoms. And for this purpose there are three methods open to him. Firstly: The observation of the character of the symptoms, and the comparison of them with cases of undoubted disease. Secondly: Having the patient watched without his knowledge, so as to detect any movements or actions inconsistent with his supposed ailment. (This is of course best managed in a hospital.) Thirdly: The employment of violent or disagreeable methods of treatment, calculated to weary or disgust the patient; and even these may be resisted for a long time.

But it is different if the physician has not medical charge of the patient, but is called under certain medico-legal circumstances, to form his opinion on observations made by him in one or two examinations. Then the first only of these methods is available to any extent; and it is only by a minute observation of the symptoms and general features of the case, and a careful comparison of these and of the statements of the patient with the known characters of real disease, that he can hope to arrive at a correct conclusion.

And in the first place, the general combination of symptoms, and their consistency with one another, should be considered.

In our case, the total anæsthesia of the left limbs and left half of the trunk, combined with the nearly complete paralysis of motion in the limbs of the same side (the face being unaffected), formed a group of symptoms which it would be extremely difficult, if not impossible, to explain on pathologico-anatomical grounds.

Secondly, the real nature of the defective sensibility must be tested.

It is well known that anæsthesia has often been feigned for various purposes. Numerous such cases have been recorded, especially by military surgeons, where much greater pain than the prick of a needle has been borne with the most unflinching resolution, and without showing the least sign of feeling. In the instance just recorded it was evident, from the involuntary movements which were on one or two occasions caused by the unexpected application of a needle (when the man had not his muscles under control), that he really had tolerably acute sensibility. Where treatment by powerful shocks of electricity is available, it will usually be able to cause such pain as will lead to the exposure of feigned anæsthesia.

Thirdly, the symptoms of motor paralysis must be carefully investigated; and if the paralysis is complete, or nearly so, it will not be difficult to distinguish between the real and the feigned disease.

Several tests are available. 1. The well-known appearance of dragging in a paralysed leg. This symptom was at first entirely absent in the above case; the slight dragging which was subsequently observed differed from that of ordinary paralysis. (See report.) 2. The test suggested by Dr. Hughlings Jackson. In real hemiplegia the paralysed arm falls forward when the patient stoops; in case of feigning, the arm will probably be retained by the side. (We omitted to use this test in our case.) 3. The condition of the muscles in long-continued paralysis. Where paralysis has continued for several months in a limb, we should expect that the muscles would both diminish in size and lose their electro-contractility. Neither of these conditions existed in our case. 4. In cases where the patient says that he has been confined to bed for many weeks, the condition of the soles of his feet may be taken as a test of the correctness of this statement. A scaly and cracked condition of the cuticle of the soles will, according to our experience, be invariably found in adults (at least in the lower ranks of life) who have been confined to bed for a considerable time.

In all cases of suspected feigning of disease we should endeavour to ascertain whether the patient has had opportunities of becoming acquainted with the symptoms of the disease supposed to be simulated. In our case we were informed that an acquaintance of this man, who was in the same accident, became actually hemiplegic within twenty-four hours after meeting with his injury, and that this fact was well known to the patient.—*Lancet, April* 4, 1874, *p.* 474.

17.—THE DETECTION OF MALINGERING BY ELECTRICITY.

By the EDITOR OF THE BRITISH MEDICAL JOURNAL.

Malingering for the purpose of extortion, or for procuring the means of subsistence without work, or merely for exciting compassion and interest, is much more frequent than is generally supposed. Where the physician's suspicions are aroused, faradisation of the skin with a wire brush and a powerful current, may be an excellent means of settling such questions. This proceeding is extremely painful, and yet never does any harm, except in cases of cerebral disease, where it should be avoided. We need, therefore, not be reluctant to employ it in suspicious cases, as it is far more humane than the actual cautery which has often been called into requisition, and yet sufficiently disagreeable to make a repetition of the procedure dreaded by such persons. The Welsh fasting-girl might still be alive, and in the enjoyment of a hearty appetite, if her case had been looked upon in a proper light, and if faradisation of the skin had been a few times employed as a diagnostic and curative measure. Faradisation may also be usefully employed where certain forms of paralysis are assumed by the malingerers. We have by this means succeeded in unmasking a person who intended to extort money from a working-men's benefit society, on pretence of having suffered paralysis from an accident. As in peripheral paralysis the muscles always lose their faradic excitability, the presence of the faradic response in the muscles of the arm enabled us in that case to state decisively that the man was shamming.—*British Medical Journal, Nov.* 22, 1873, *p.* 614.

18.—A CASE OF SUPRAORBITAL NEURALGIA RELIEVED BY SUBCUTANEOUS INJECTION OF STRYCHNIA.

By R. J. A., a Correspondent of the Lancet.

Mr. A., who had been for some time a resident in a climate in which ague was prevalent, and who had an attack of that complaint, laboured under a severe attack of supraorbital neuralgia. It was distinctly intermittent, and considerably affected the general health. On seeing him, he said he had eaten very little for eight days. Two minims of solution of strychnia (four grains to the ounce) was injected into the right arm. Two days afterwards he told me he had slept well; his appetite was better; but there had been some slight return of the pain, and at the time of the second consultation he felt the supraorbital pain, though somewhat diminished. Two minims of the solution were injected, and the pain instantly disappeared. After a third hypodermic injection, the pain disappeared. Since then he has been in good health, the injection in the last instance being followed up by administration of iron and quinine as double citrate. It is right to mention that a quinine treatment was adopted without a favourable result.

Were it not that we had the history and the unilateral character of the disease, it might be supposed that the effect which strychnia as a subcutaneous injection has on the stomach would act thus in removing the pain. The most interesting feature of the case, I conceive, is the immediate disappearance of the pain on the hypodermic injection of the strychnia.—*Lancet, April* 11, 1874, *p.* 533.

19.—PHOSPHORUS IN NEURALGIA.

By S. Messenger Bradley, Esq., Manchester.

My custom is to commence with one-hundredth of a grain, and gradually increase this by one-fiftieth of a grain at a time, until, if necessary, one-tenth of a grain is taken with each dose. Beyond this quantity I do not go; as I think that, if the remedy be of use, relief will be attained by this dose equally with a larger. After trying several preparations, I now use a formula which Mr. Potts, dispenser to the Manchester Royal Infirmary, hit upon, and which seems to answer every purpose, in being tasteless, transparent, and readily prepared. He dissolves ten grains of phosphorus in two ounces of ether, agitating the solution from time to time; and of this solution, one minim (containing one-hundredth of a grain) is administered in an ounce of water, with half a drachm of glycerine. The glycerine suspends the phosphorus so perfectly that a transparent mixture is the result. The addition of a little bitter infusion entirely removes any *soupçon* of lucifer matches which may hover about the medicine.—*Edinburgh Medical Journal, Dec.*, 1873, *p.* 572.

20.—INJECTION OF CHLORAL IN TETANUS.

M. Bouillaud, on the part of M. Oré, a professor of the Bordeaux Medical School, related a case to the Académie des Sciences, February 16, in which chloral had been injected into the veins as a remedy in traumatic tetanus. A man, after a wound of his finger, became the subject of tetanus, in consequence of which his mouth became so closed that no remedy could be administered. M. Oré therefore threw an injection containing ten grammes of chloral into the veins, which produced peaceful sleep; and this was followed by a second and third injection, with the effect of obtaining a sleep of eight hours. —*L'Institut*, Feb. 18. [The patient was going on well when the above statement was made, which might have been postponed until the issue of the case had been decided.]—*Medical Times and Gazette, Feb.* 28, 1874, *p.* 254.

DISEASES OF THE ORGANS OF RESPIRATION.

21.—ON "REST" IN THE TREATMENT OF CHEST AFFECTIONS.

By Dr. FREDERICK T. ROBERTS, Assistant Physician to University College Hospital, and to the Brompton Consumptive Hospital.

[There is no doubt that tissues actually diseased or in danger of becoming so, require to some extent mechanical rest, just as does a diseased joint. This remark applies to all chest affections, but in the present article its bearing on pleurisy only is considered.]

To anyone who is acquainted with the pathological conditions resulting from this disease, it must be evident that to restrain the movements of the affected structure, to prevent the friction of the inflamed surfaces against each other, and keep the parts as much at rest as possible, ought to be the *first principle* to be followed in its treatment. The state of things is very similar to that observed in connection with an inflamed synovial membrane in a joint, and in this condition the first thing the surgeon attends to is to maintain the joint perfectly motionless. I have never seen this principle definitely advocated for the treatment of pleurisy in the published writings on this affection, and, with the exception of two or three suggestions on the subject, it does not seem to have received any particular notice. In the course of my hospital practice during the last ten years, a large number of cases of this complaint have, as a matter of course, come under my care. In the year 1864 I, quite independently, arrived at the conclusion, from certain observations, that the primary object to be arrived at in treating pleurisy should be to endeavour to procure rest; and since that time experience has fully convinced me that I was not mistaken in my opinion. I have on previous occasions brought the subject before the profession, among others in a paper on "Rest and Position in the Treatment of Medical Diseases," published in vol. ii. of the "Liverpool Medical and Surgical Reports," 1868. In my "Handbook of Medicine," lately published, the principle has been definitely laid down and advocated, and an indication given of the mode which I usually adopt in order to procure the needed rest. Now I propose to present a more detailed account of my experience of this treatment, and of the manner in which it is carried out.

With regard to the methods employed for procuring rest in the treatment of pleurisy, that which is of special importance is the use of certain appliances fixed round the affected side more or less extensively, so as to limit or prevent its movements. As subordinate aids, it is advantageous in severe cases to keep the patient quiet; to give instructions to restrain the breathing as much as possible (though this is generally done instinctively, on account of the pain the act induces); and to forbid all conversation. In a pathological point of view, the results which might be fairly anticipated from this mode of treatment are, that the inflammation would be limited and subdued; that the effusion of lymph and fluid would be checked; and that whatever morbid exudations had been poured out would be more readily absorbed, followed by organization of the remaining lymph, with the formation of adhesions.

In actual practice cases of pleurisy present very considerable differences when they come under observation, and it will be necessary to point out to what extent the treatment by rest is applicable to the different classes. In one group of cases, of very common occurrence in hospital practice, the inflammation is localised to a small patch, and appears to have but little tendency to spread. There is generally a good deal of pain, especially on breathing or coughing, but there are no constitutional symptoms. Physical examination reveals limited friction-sound. In these instances the firm application of three or four strips of plaster round the side, in the manner to be presently described, almost invariably gives complete relief, and even allows the patient to continue his occupation. Nothing more is needed, and in a week or two the plasters may be removed. In another class of cases, a patient comes under notice who is evidently in the early stage of a severe and extensive attack of pleurisy, judging by the local and general symptoms and physical signs. It is an unfortunate fact, however, that in many instances the symptoms are not very prominent at first; and it is by no means uncommon to find that abundant effusion has taken place before the patient is aware that there is anything particularly wrong. Should a case come under treatment in this early stage, I would strongly recommend that a trial should be given to the plan of *mechanically fixing the entire side* by one of the methods to be now described. In order to be of any use it should be done effectually, so as to restrain the movements as much as possible, and the sooner the application is made, the more likely is it to be of service. The plan I originally adopted was the following:—Strips of adhesive plaster, from four to five inches wide, were fixed at one end close to the spine, and then drawn tightly round the side as far as the middle line in front, the patient being directed to expire deeply. In this manner the whole side was included, commencing from below and proceeding upwards, each succeeding strip partially overlapping the one below. One was also fixed over the shoulder. Over this layer of plaster strips of bandage of the same width were fixed in like manner, having been previously dipped in a mixture of mucilage and chalk, such as is used in the treatment of fractures. Two or three layers of these were laid on, and then heated sand-bags applied, in order to dry the application as soon as possible. This is a most effectual mode of fixing one side of the chest, while it leaves the other quite free to act; and I would, by the way, commend it to those who are called upon to treat fractured ribs. The plaster adheres firmly to the skin, and the bandages adhere to the plaster, a firm casing being formed which will remain on any length of time. With regard to pleurisy, however, I have since then adopted another plan, which, so far as this disease is concerned, seems sufficiently efficacious. It is merely to use strips of plaster, putting on two or three layers in the following manner:—The first strip is laid on obliquely *in the direction of the ribs*, the second *across the course of the ribs*, the third in the direction of the first, about half overlapping it, the fourth as the second, and so on until the entire side is covered. A strip is also passed over the shoulder, which is kept down by another fixed round the side across its ends. Now it is difficult positively to prove that this treatment actually checks the course of pleurisy; but, taking a common-sense view of the matter, it is not improbable that such a result might be anticipated; and, from my own experience, I have not the slightest doubt but that it is brought about. I have carried it out now in a good number of cases, and in all the course and termination have been most satisfactory, while relief to the pain and other distressing symptoms has been generally immediate. I feel convinced also, that in many of those cases of extensive pleuritic effusion which come under observation, the accumulation might have been prevented or moderated had this plan of treatment been adopted at an early period.

In another set of cases of pleurisy there is found to be a moderate amount of effusion when the patient first comes under treatment. Here, too, I would recommend efficient fixing of the side. In those cases in which I have carried it out, I have almost always had satisfactory results, whereas I have more than once regretted the neglect of this plan of treatment. Where the effusion is very abundant, but little can be expected from it, though I think that occasionally it has appeared to aid absorption. Now and then cases present themselves in which there is extensive exudation of lymph over the surfaces of the pleura, with but little fluid. Here the only object to be desired is to bring about adhesion of the surfaces as soon as possible, and strapping the side firmly aids this most certainly. In bilateral pleurisy of course this mode of treatment can scarcely be practised; and where this disease complicates others it will be of less service.—*Practitioner, March*, 1874, *p.* 182.

DISEASES OF THE ORGANS OF DIGESTION.

22.—ON THE SOLVENT ACTION OF PAPYA JUICE ON THE NITROGENOUS ARTICLES OF FOOD.

By Assistant-Surgeon Dr. G. C. Roy, Corresponding Member of the Glasgow Medico-Chirurgical Society.

[Popular beliefs are often pregnant of wholesome truths. It was on the evidence of an ignorant body of milkmen that Dr. Jenner brought to light the protective influence of cow-pox in the human system.]

It is the practice amongst the native cooks in India to add a few drops of the milky juice of the plant under consideration to tough old meat, to make it tender and supple. Four years ago, anxious to ascertain whether any such virtue really existed in the plant, I added a few drops of the fresh juice to a pound of minced goat's meat, and stewed it over a slow fire. To my surprise, the whole ran into a diffluent mass in five minutes, owing to a larger quantity of the juice having been used on the occasion than is necessary to make it tender and eatable. Since then I had no further opportunity to test its property until I went to England, when I carried with me a quantity of the juice obtained by incising superficially the unripe fruit, and drying it in the sun. Through the kindness of Dr. Parkes, the esteemed professor of Netley, I was allowed the opportunity to carry on the experiment.

The plant belongs to the natural order Papayacæ, and is termed Carica Papya. It is indigenous to tropical climate, and has a rapid growth, consisting of an unbranched stem from ten to twelve feet high, surmounted by large, deeply palmated stalked leaves. The fruits or pepos are edible both in their ripe and unripe state. The former is used as dessert, and the latter cooked as vegetable. The fruit is about the size of a melon, with a green rind, enclosing a sweet, delicious pulp, in which innumerable seeds, about the size and appearance of black pepper, are imbedded. The plant grows extensively in Bengal and over all India, and bears fruit in one year.

Incisions on the stem, or breaking off the leaf from its joint, yield a few drops of this milky juice, but an abundant flow of it can be obtained by scarifying the unripe fruit in the same way as the poppy capsules are treated. The fluid is liable to decompose, especially in hot weather, if kept over for a few days, and hence it should be dried in the sun in shallow dishes, and stored for use. One ounce of the juice can be obtained in an hour's time from the fruits of a vigorous plant. The dried stuff is of a yellowish white colour, hard, friable, and has a peculiar smell. Two drams of this beaten up with one ounce of water will, I believe, give the approximate proportion of strength of the original liquid. At the risk of being tedious, I will give the details of the experiment as they were noted down at the time.

April 30th—thermometer 66 deg. The juice was obtained on 20th December, *en route* to England. A solution of it was made of the strength of 1 gramme to 3 c. c. of distilled water. Two pieces of fresh lean of beef, each weighing ten grammes, were taken and finely minced. Whilst to the one 10 c. c. of distilled water were simply added, to the other 1 c. c. of the made-up solution was mixed up with 9 c. c. of distilled water and added also. Both cups boiled for five minutes. The medicated portion became soft and half dissolved in the fluid, whilst the other dish remained tough and unchanged.

Another piece of the same meat, of the same weight, was moistened in lump with 1 c. c. of the solution, and kept over for next day's observation.

1st May. The superficial portion of the lump was soft and gelatinous, making the piece very slippery to the touch. When cut up into pieces, and a little more water added, the whole became pulpy in an hour's time, without the aid of heat.

The animal was killed on the 28th of April, the thermometer standing at 66 deg.

Another experiment which was made the same day to confirm the result was equally successful.

Two of the cups, medicated and unmedicated, were kept under cover by Dr. Parkes, to note whether the solution had any power to retard decomposition.

2nd May. Decomposition had commenced in both the vessels, but it was more advanced in the cup with papya solution.

27th May. A fresh solution was made (1 gramme to 20 c. c. of water), and the following experiments were tried :—

In four separate dishes, beef, hard-boiled white of egg, freshly prepared moist, gluten and arrow-root, each 10 grammes in weight, were treated with 2 c. c. of the solution by 8 c. c. of distilled water. To make a standard for comparison, the same articles, of similar weight, were digested in four more dishes, with 10 c. c. of distilled water only. The whole kept over till next day without heat.

28th May. The dish of meat with papya soon became gelatinous on being stirred with a glass rod. The albumen of egg was soft, and when mashed, broke into a uniform pulp. The gluten specimen was soft on the surface, and its superficial layer dissolved in the fluid, giving it a slight turbidity. Arrow-root was dry and visibly unchanged.

The dishes with water were unaffected. The meat fibres remained without change. The egg, when broken up, remained lumpy. The gluten was clear, and even when it was worked up with finger, did not give the liquid the least turbidity. Arrow-root dish was moist, but without any change.

It should be remarked that the dish of meat and of albumen contained more fluid than the corresponding ones with water, whilst the arrow-root dish, which was unaffected in both instances, was dry in one and moist in the other.

All the dishes were preserved for further test.

29th May. Both the dishes of meat were eaten away by a cat, which grew exceedingly fond of it. The whole of the gluten was dissolved in the solution, whilst the dish with water was yet unchanged. Each of the cups was mixed up with 50 c. c. of distilled water, and left over for next day.

30th May. The cups filtered for test. The two arrow-root solutions gave no reaction when tested with Fehling's Solution for sugar.

The drugged albumen was fast undergoing decomposition, and had an offensive smell. When filtered, nearly the whole of it passed through. The filtrate was clear and decidedly acid. It gave no precipitate with heat, or heat and nitric acid, or ferrocyanate of potash.

The watery specimen of albumen, when filtered, left behind a quantity of residue. The filtrate was milky in colour and neutral in reaction, and gave a slight precipitate with each of the reagents.

The specimen of gluten solution in papya passed through the filter, leaving merely a trace behind. The solution was tolerably clear and acid, and gave an abundant precipitate with heat. More than ¼ was coagulated with heat and nitric acid, and some flocculi formed with ferrocyanate of potash. The precipitate, with heat, was insoluble in liqr. potassæ.

In the watery dish of gluten very little was dissolved. The lump was still sticky. The water solution was clear and strongly acid, and gave no precipitate with any of the reagents. Nitric acid made the solution clearer than before.

3rd June. Solution of the strength of ½ gramme to 10 c. c. of water. As this was kept over for some days before use, it was found to have lost its acid reaction, and settled itself into two parts—the clear liquid above, and a sediment below. The clear supernatant liquid gave the same reaction with a piece of meat, showing that the solvent agent was soluble. A standard preparation of meat with water was made at the same time.

5th June. Both the dishes were mixed with 50 c. c. of water, and left to filter through.

8th June. The papya dish was somewhat advanced, whilst in the watery dish there was no sign of putrefaction. 10 c. c. of the papya filtrate was dried in a porcelain crucible *in a hot* air bath, and 10 c. c. of the watery filtrate was treated in a similar way. The quantity of solid dissolved out in the papya filtrate weighed 2 grammes, whilst in the watery filtrate it was ·09 grammes.

Another equal quantity of each filtrate was tested in a test tube with heat and nitric acid, and it confirmed the result that the albumen dissolved out in one was more than double the quantity in the other.

A piece of gelatinous meat, as changed by paypa juice, was examined next day by Dr. Welsch, under the microscope. It swarmed with vibriones in active motion. As for the muscular fibres, the disintegration was complete, and those fasciculi that were yet entire were just separating themselves into ultimate particles. The digestion could not have been more complete.

As very little solid stuff was left for further research, the remnant was reserved for chemical analysis.

A solution of the strength of 1 gramme to 30 c. c. of distilled water was prepared and filtered. Reaction distinctly acid. A portion was boiled to dryness in a crucible. The vapour did not redden litmus, but the concentrated solution became more strongly acid, and remained so when thoroughly dried. A little more heating charred the side of the capsule. When incinerated the ash gave an alkaline reaction. A deep precipitate on boiling. The coagulum was strained, and a portion treated with the following reagents. Nitrate of silver gave a white precipitate soluble in ammonia and acids; no precipitate with chloride of calcium, cold or boiled; no change of colour with perchloride of iron; a white cloudy precipitate with liqr. potassæ; some precipitate with chloride of barium.

Remarks.—The above experiments in detail conclusively show the solvent action of the juice on all nitrogenous articles of food. Its effect in making the meat tender has been noticed in several botanical works, East and West Indian, but, so far as I have been able to find, no systematic experiment has been made up to the present to test its virtues medicinally. Some have contented themselves in merely mentioning the practice of the natives as alluded to above, and some West Indian authors ascribe to the plant the power of hastening decomposition in dead animals—so much so that they go the length to assert that a joint suspended under its branches will fall to pieces when cooked. Nay, the belief in the West Indies is so strong, in its power of hastening decomposition, that it is said that live animals fed on the unripe fruit will not keep long after death.

That there is a considerable power of disintegration inherent in the plant is established beyond doubt and cavil. But the question is, what is its peculiar nature? Is it chemical or dynamical? Is it, like the yeast, a fermenting agent, the presence of which in dead animal substances destroys the stability of their composition; or, are the solution and disintegration allied to natural digestion, and the results of chemical change? If the former, there is a valuable discovery of a nitrogenous ferment, which stands in the same relation to protein compounds as yeast does to starch. The conversion of insoluble starch to soluble substances constitutes the process of digestion of amylaceous principles of food in our mouth, and this conversion is chiefly assisted by an animal ferment, Ptyaline, that exists in saliva. The digestion of nitrogenous principles is mostly a chemical process, in which the gastric juice plays an important part. The rapidity and completeness with which the papya juice acts on meat, when aided by high temperature, surpasses all digestive processes on record. The smallness of quantity used to bring about the change negatives the assumption of any caustic virtue in the plant. Besides, I have put the juice on my tongue, and applied it to the skin without any irritant effect. I was first inclined to believe the solvent action was due to some fixed organic acid, either tartaric, citric or malic, as will be seen from the records of analysis, but I have failed to arrive at any determined result. The disintegration takes place too soon to be the effect of mere putrefaction. The moving vibriones observed under the microscope were no doubt generated by keeping, and were not the cause, but the effect of disintegration. In all putrefactive changes these are looked upon as the initiators. But inasmuch as the boiling temperature which destroys the existence of vibriones hastens this peculiar change, it *is fair* to suppose that the solvent action is something different from putrefac*tion*. Besides, no reagent has yet been able to bring about putrefaction in fresh meat, in five minutes. The fruits in their ripe and unripe state are edible and quite harmless.

The digestive agent is not acid, for its reaction is too feeble, and even when long keeping makes it ammoniacal and neutralizes the acidity, it yet retains its peculiar virtue. The solvent principle is soluble in water. Coagulated albumen dissolved by it will not coagulate again with heat acquiring the property of albuminose; gluten is thoroughly dissolved, and can be reprecipitated.

The whole action is so identical to healthy digestion that I wonder we have not availed ourselves of this medicinal property, in cases of invalids and dyspeptics, to substitute a process of artificial digestion.

A few grains taken immediately after a meal will substitute the power where it is wanting. I have not tried it in any case internally, but from its effect on the cat, which grew so fond of it that it became a task to prevent its depredation, I believe it is harmless. My attention was also directed to discover whether it could be utilized in preparing soluble meat or something like a liquid extract, but its liability to decomposition is a bar to its use. Further investigations are yet wanting to establish its repute, but, as far as they have been gone into, the result is highly encouraging. I intend to resume my operations as soon as I am relieved of my present arduous work, which leaves no time for experiment.—*Glasgow Med. Journal, Jan.*, 1874, *p.* 73.

23.—ON CARLSBAD AND ITS MINERAL SPRINGS IN RELATION TO AFFECTIONS OF THE LIVER.

By Dr. Stephen H. Ward, Visiting Physician to the Seamen's Hospital, London.

Anyone who desires to study the physiognomy or, as I would rather express it, the external aspect of diseases and constitutional affections in their more chronic form, should visit the different continental watering-places during the height of the season. Dr. James Johnson glances at the characteristic aspects of disease which distinguish different health-resorts, as Spa, Schwalbach, Wisbaden, Wildbad, Kissingen, &c.; but he considers that Carlsbad presents a greater medley than any of them. However, one will not have been long at the last-named spa without discovering the prevalence of the sallow or more or less jaundiced complexion, pointing to liver affections as those for which it is largely resorted to.

The limits of my paper will not allow me to do more than bestow a short notice on the different hepatic disorders which are likely to be benefited by the Carlsbad "cure." They are—

1. *Hyperæmia*, indicated by a sense of fulness and uneasiness in the right hypochondriac and epigastric regions, by more or less dyspepsia, by sallowness or even a jaundiced tint of complexion, and generally, at first, by an undue secretion of bile, which may show itself in bilious vomiting and diarrhœa. The first stage will, in most cases, have been treated at home, and the hyperæmia will have merged into more or less chronic congestion, with enlargement of liver, perverted or deficient secretion, &c., before the patient from England gets to Carlsbad. Immoderate eating and drinking, and especially habitual indulgence in rich articles of food, and the lethargy and disinclination for exertion induced by the habits of the gourmand, are the usual exciting causes of hyperæmia of the liver, and engorgement of the vessels of other abdominal viscera. Direct irritation and congestion of the liver occur also as the result of undue and frequent use of alcoholic liquors, especially upon the empty stomach, such congestion causing more or less enlargement of the viscus, and being often but the first stage of cirrhosis.

2. *Fatty liver* (infiltration, not degeneration) is usually but a symptom of a general deposition of fat in and about the tissues and organs of the body; and when it is the result of the excessive indulgence alluded to under the previous head, coupled with deficient exercise, will probably be much relieved by the

Carlsbad "cure." It is only in an advanced stage of this affection that we have any marked symptoms, as enlargement of the liver, uneasiness and dragging sensation on lying on the left side, irregular and indolent functional action, annoying flatulence, a sallow and often a greasy state of skin. There is something in the rapid decrease of fat under these mineral waters which is more than can be explained by the rigid adoption of altered hygienic arrangements, and may be fairly attributed in some measure to the action of the waters themselves. Should the state of the pulse, &c., indicate fatty accumulations about the heart, or should irregularity in the circulation, tendency to syncope or vertigo, point to the possibility of degeneration of tissue, the "cure" will be contra-indicated, or have to be adopted with much caution.

3. *Jaundice*, where it is a symptom of the preceding affections, or associated with chronic catarrh of the gall-ducts, or with habitual obstruction of the bowels and accumulations in the large intestines, will be relieved by the "cure." As Niemeyer, however, remarks, "if jaundiced patients with an incurable obstruction of the bile ducts go to Carlsbad, their jaundice is not improved by the use of the waters; but they die sooner than they otherwise would, because the symptoms of congestion are increased, and the destruction of the liver-cells is hastened by augmented secretion."

4. *Gall-stones and biliary concretions.*—For these the Carlsbad "waters" are considered almost a specific. They remove those already formed, have, indeed, the credit, which must be taken *cum grano salis*, of a solvent action, and act as a preventive of re-formation. Gall-stones would appear, according to the experience of Carlsbad physicians, to be far more common than they have been considered to be. Out of 372 patients who came under the care of Dr. Hlawacek in one year, 64 had gall-stones, and out of 581 patients in another year, 91. The fact is that gall-stones are sought after at Carlsbad very carefully, by washing the evacuations over a fine sieve, and many small biliary concretions are thus found which have painlessly passed the ducts into the duodenum, and which would have escaped the notice of ordinary medical observers.

Chronic derangement of the functions of the liver, usually torpor, with or without enlargement of the organ, the result of climatic influences in India or of exposure to malaria, especially when associated with much cachexia and anæmia, is not likely to be benefited much by Carlsbad and its waters. Here the more tonic waters of other spas will be indicated, or the occasional use of saline aperients, iron and quinine, and the nitro-muriatic acid treatment.

Individuals suffering from congestion of liver secondary to advanced lung or heart mischief, or from cancerous affection or amyloid or fatty degeneration of the liver, or from cirrhosis in its advanced stage, had better remain at home under the treatment of their own medical advisers.

The Carlsbad cure is a powerful remedy, and in properly selected cases is doubtless very valuable. Many individuals, however, get worse under it, and are consoled by the statement that they will feel the benefit when they return home; and this may prove true in many instances. But to some the benefit does not come, and the inevitable termination of serious organic mischief has been precipitated, or an unfavourable result has been determined, in affections which, under different treatment, might have done well. I am not saying this to discourage or deter individuals who propose seeking renewal of health, at Carlsbad or elsewhere, but simply to show how important it is that an invalid's case and constitutional power should be well considered before he starts for a goal some 700 or 800 miles distant, where, after all, the desired end may not be attained.

What is called the "nachcur," the after-cure, is not less essential than the "vorcur." The local physicians recommend that the patient, after he has returned home, or gone elsewhere from Carlsbad, should take the bottled mineral water in moderate quantity, and persevere also in following the hygienic rules which were enforced during the "cure;" but the "nachcur," in its larger sense, should extend over the remainder of an individual's life, and should consist in strict avoidance of the habits which led to impaired health.

The Carlsbad season lasts from May to the end of September, and the period of cure for each invalid ranges from two or three to eight or nine weeks. Those who are wanting in vigour, or not tolerant of heat, will do well to select May and June or September for their sojourn at Carlsbad. If means and time permit absence during three or four months, it will be a good plan to get the "cure" over in the spring and early summer, and afterwards to get braced in some mountain district. For some cases the warmer months, July and August, will be suitable.

In conclusion, I would remark that a large number of invalids go to Carlsbad and other watering-places who may derive but little benefit from the morning draughts from the springs, but who yet, in strict attention to health-laws, as regards diet, exercise, rest, and recreation, in fresh scenes and associations, in the escape from the anxieties and duties of everyday life, may succeed in finding the *basilisk*, the search after which forms the subject of an amusing and instructive episode in Voltaire's admirable tale "Zadig."—*Lancet, Aug.* 2, 1873, *p.* 145.

24.—OXIDE OF ZINC IN THE TREATMENT OF DIARRHŒA.

By Dr. William Berry, House Surgeon to the Lancaster Infirmary.

[This remedy was first recommended in diarrhœa by Dr. Brakenridge, of Edinburgh.]

I have every reason to be satisfied with it as a remedy for diarrhœa in children, especially in those in which the cause appears to be some irritation of the nerve-centres presiding over the alimentary canal. In adults I have found it useful in cases of lienteric diarrhœa, but not so beneficial as the aromatic chalk powder of the pharmacopœia in ordinary cases.

I think with Dr. Brakenridge that in the majority of cases of diarrhœa in children—though not in all—the nervous system plays an important part; especially in those children who are teething, and in children of the poorer classes who are ill fed and badly clothed.

Whether we accept the theory of Dr. Brakenridge as to the cause of diarrhœa in children or not, there can be no doubt that the beneficial effects derived from the remedy are due to its tonic and astringent properties, I am inclined for my own part to think that its antispasmodic properties have little to do with its efficacy.

That infantile diarrhœa is in a great measure due to a debilitated state of the nervous system and to a hyperæmic condition of the mucous membrane of the bowels, is proved by the readiness with which it is relieved by oxide of zinc.

In the diarrhœa of teething children, and those whose digestion is at fault, the frequency of the evacuations is at once checked, and the character of the motions altered.

The remedy produces nausea in some cases unless a little food be administered just before it, but in many cases no nausea is produced although this point is not attended to.

Some cases are then related, and we take from these the following prescription used in the case of a child one year old:—℞. Zinci oxidi gr. 32; mucilaginis; syrup. simpl. āā ʒ ij); aqua ad ℥ ij. S. ʒ j 3tis horis.—*Practitioner, Nov.* 1873, *p.* 356.

25.—FÆCAL ACCUMULATION.

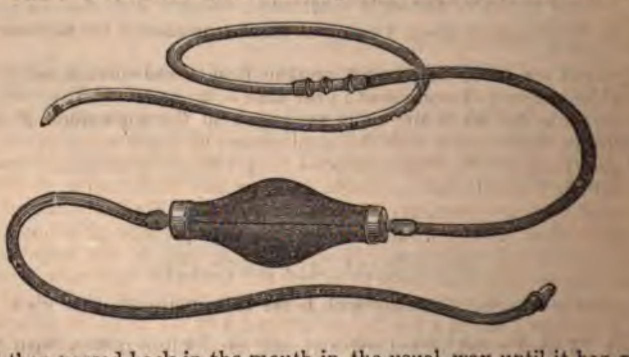
By Dr. Thomas Cole, Physician to the Royal United Hospital, Bath.

Mr. S., aged 60, had been more or less ill for two years, with gradually increasing distension of the abdomen, accompanied by some emaciation, loss of appetite, much flatus, and confined bowels. On examination, I detected an immense tumour filling the whole of the abdomen, except a small portion of the left lumbar region, where there was tympanitis. It passed upwards under the ribs, downwards into the pelvis, was perfectly smooth, very hard, and quite immovable; over a small part, to the right of the umbilicus, was indistinct resonance; and somewhat below this, I felt a coil of small intestine between the tumour and abdominal wall, evidently filled with fæcal matter. By firmly pressing the finger against the swelling, at different parts, I found that a slight indentation was made, though with difficulty. The superficial abdominal veins were much distended. The rectum was full of large hard lumps. My diagnosis was fæcal accumulation to an extent I never saw or heard of before. Large injections of warm water daily for three weeks, aided by small doses of belladonna and nux vomica, brought away a most astonishing quantity of fæcal matter, and resulted in the total disappearance of the tumour, and the restoration of the patient to perfect health. I may add, that homœopathy had had a lengthened trial in this case.—*British Med. Journal*, *Nov.* 29, 1873, *p.* 630.

26.—AN IMPROVED SYPHON STOMACH-PUMP.

By Dr. Louis H. Tosswill, Exeter.

For some time past I have felt convinced that the stomach-pump now in use is far from a perfect apparatus. It is costly, not particularly portable, and, above all, a dangerous instrument; cases being known where the stomach-tube has been pushed through the coats of the stomach or œsophagus. I wish therefore to bring before the notice of the profession a stomach pump on a novel principle, which has been made for me by Messrs. Arnold and Sons, the well-known instrument makers in West Smithfield. The advantages I claim for my instrument are—freedom from danger, cheapness, and increased portability. It can be easily carried in the coat-pocket, its cost does not exceed 15s., and it is almost impossible to perforate the coats of the stomach or œsophagus by means of it, be the operator ever so clumsy or ignorant. The instrument consists of an india-rubber tube, about half an inch in diameter and some four feet in length, provided with a ball of the same material, which is three or four inches in diameter, and situated about eighteen inches from one end of it. In fact, it is almost a *fac-simile* of one of Higginson's enema syringes, but without a valve. To the shorter length of tube the stomach-tube proper is attached by a simple contrivance. This stomach-tube is nothing more nor less than an enlarged Thompson's india-rubber catheter with two large oval holes situated near the extremity. Let us suppose the instrument to be used in a case of poisoning where it is desired to wash out the stomach as speedily as possible. The patient being seated in a chair, or reclining on a couch, the stomach-tube is oiled and then passed back in the mouth in the usual way until it has reached the upper part of the pharynx. Its passage down the œsophagus, as in the case of Thompson's catheter in the urethra, is effected by twisting it round and round in the hand until it has reached the stomach. I need scarcely point out



how much less pain this soft flexible tube must cause than the hard, somewhat inelastic tube generally used. The rest of the instrument is now adjusted to the stomach-tube (the work of a second), and then a jug of water being obtained, the end of the tube beyond the ball is introduced into it. The operator with one hand pinching the india-rubber tube somewhere between the ball and the patient's mouth, with the other hand compresses the elastic ball, and thus forces out some of the air contained in it, which bubbles up through the water in the jug. When this has been repeated two or three times all the air is forced out, and the ball becomes filled with water. If the jug be now raised a foot or two above the patient's stomach, and the pinching of the tube discontinued, the instrument acts at once as a syphon, and the water flows from the jug in a continuous stream into the patient's stomach. When enough has been introduced, the tube is pinched somewhere between the ball and the patient's mouth, and the flow of water into the stomach is at once stopped; the water being retained in the ball by the atmospheric pressure. If the end of the tube be now taken out of the jug, and put into a basin below the level of the patient's stomach, upon the pinching of the tube being discontinued, the contents of the stomach will at once flow out into the basin in a continuous stream, the instrument acting of course again as a syphon, only in the reverse way to that previously. When it is believed that the stomach is nearly empty, the tube is pinched between the ball and the basin, the end of the tube put back into the jug, and the whole thing repeated over again. The object of pinching the tube when the end of it is being shifted from the jug to the basin, and *vice versâ*, is of course to retain the water in it by means of the atmospheric pressure, thus enabling it to act as a syphon. In a case of opium poisoning I have lately used this instrument, and by means of it thoroughly washed out the patient's stomach in a very brief space of time. In case the holes at the end of the stomach-tube become clogged with food, they can be easily freed by pinching the tube between the ball and the jug; compression of the ball will then force out the fluid contained in it, and thus clear the holes. In conclusion, I would draw special attention to the low price at which this instrument can be obtained. I do not think I am wrong in asserting that the great majority of general practitioners do not possess a stomach-pump. This is chiefly due, I believe, to the high price of the instrument hitherto used. This objection being now removed, there can no longer be any excuse for any practitioner being unprovided with an instrument, the want of which may materially tend to lessen the chances of recovery in a case of poisoning.—*Medical Times and Gazette, Feb.* 14, 1874, *p.* 199.

27.—ON THE TREATMENT OF THREADWORM.

By Dr. T. Spencer Cobbold, Lecturer on Parasitic Diseases at the Middlesex Hospital.

[We cannot expect in the curative treatment of threadworm a success equal to that which ought to be obtained in the case of tapeworm. The worst of cases, however, may be overcome by perseverance in the application of appropriate remedies in combination with the employment of hygienic measures.]

I have ransacked the Pharmacopœia for permanently effective remedies, but I have satisfied myself that no single drug, or any combination of drugs, can be employed with any certainty of perfect success. You cannot find any remedial agent that exerts what may be called a specially poisonous or specific action upon the threadworm. However, in the case of the large roundworms, that is, the ascarides properly so called, the action of santonine is almost that of a genuine specific. It is, indeed, a valuable lumbricifuge; but, as an oxyurifuge, I confess I am at a loss to understand why its action is so much extolled abroad. I have used it extensively, and can only report unfavourably of its action in oxyurides. On the other hand, all the larger kinds of nematode worms are readily poisoned by it.

As to the time-honoured employment of injections of infusion of quassia, or of lime-water enemata, or of others containing salt, castor-oil, santonine, and what not besides, all that can be said of them, in the case of adults, is, that they undoubtedly afford partial relief; but, in view of contributing to a permanent cure, they avail nothing. Even the employment of long elastic syringe-tubes will not enable patients to reach the true seat of the helminths; and one cannot but perceive the inconvenience, to say nothing of the danger, of recommending their use. I see by a statement, reproduced in a recent number of the London Medical Record, that Professor Mosler recommends the employment of large enemata for the treatment of entozoa in general, and for the expulsion of oxyurides in particular. In the case of imperfectly expelled tapeworms (which he believes to possess the power of anchoring themselves afresh in the colon after expulsion from the small intestine), he even recommends the use of milk-and-water enemata up to the amount of three pints. I will only digress so far from the matter immediately before us, as to say that I have repeatedly expelled heads of tapeworms from the colon without giving injections; and I must say that I cannot bring myself to believe in the supposition of any true re-anchorage occurring in cases where the head of the worm has been dislodged from the smaller bowel. In this connection, you will not have forgotten the case of the young daughter of a physician, where I had only the upper part of the head itself left to operate on, and yet I succeeded in dislodging it. The four suckers were so slightly connected, that they separated from each other in the water of the phial during gentle agitation. The case was one, the treatment of which had previously baffled several experienced medical practitioners.

Notwithstanding recent investigations, and the clearly enunciated conclusions based on them, I find that all our ordinary manuals, handbooks, vade-mecums, and such like generally useful treatises, continue to encourage the old and erroneous notion that the threadworms reside in the rectum and sigmoid flexure of the colon. Thus, even a writer in the Lancet of last year (Feb. 10th, 1872, p. 185), says:—"As oxyures infest the lower bowel, they are best reached by enemata." Of course, if this were the whole truth, the injections of quassia and salt, recommended by Mr. Date, would alone suffice to get rid of the parasites. The fact is, however, that the entire length of the colon is the territory inhabited by the threadworm, the cæcum itself constituting the parasites' true head-quarters. Knowing this, therefore, to be the case, and likewise understanding, as we now do, how these entozoa originally gain access to their human bearers, it is clear that we are in a position to treat the threadworm helminthiasis on thoroughly rational principles. Injections, as I have said, cannot be conveniently employed to wash out the contents of the arch of the colon and cæcum, and therefore we must, in the main, rely upon the operation of medicines taken by the mouth. From the very first, I have insisted upon this mode of procedure, employing injections only for the purpose of dislodging such oxyurides as have been driven down to the lower bowel. When it comes to the question of the choice of drugs, you will find that a great variety of medicinal agents may be used with good effect. Some seem to answer better in particular cases than in others. Speaking generally, I give a preference to active saline cathartics repeated for several days in succession, followed by the use of cold water enemata. Small doses of chloric ether and sulphate of iron are eminently serviceable additions, and the same may be said of aloes, with or without assafœtida. In cases where these drugs are objected to, I have employed various active mineral waters with good results. Bitters of all kinds are useful, and patients who object to salines will swallow any reasonable amount of the infusion of gentian and other pure vegetable tonics. As a rule, oxyurifuges should be administered in the form of copious draughts, and the oftener they are repeated, for a short interval, the more effective will they prove in the end. The rapid passage of the drugs through the bowel will, if several times repeated, carry most of the parasites sufficiently low down to be within reach of the clysters; and the prevention of the return of the parasites to the upper part of the colon is one of the practical points worth looking to. The employment, therefore, of enemata, after the exhibition of remedies by the mouth, cannot fail to prove beneficial.

I am free to admit that, do what we will, some cases prove obstinate and apparently incurable. How are you to account for this? Is it really true that the disorder may baffle your best efforts? In one sense it may be that you can never succeed; in another sense, you may have been successful without knowing it. What is the probable explanation? I think it is as follows. Real cures are oftener effected than is commonly supposed. Thus, many patients will say that they have not seen a threadworm for the space of six weeks, or even for three months, when, all of a sudden, their enemies return. In these cases, I believe that fresh invasions have taken place, the patients having in the interval swallowed new germs, or more strictly eggs, derived from oxyurides infesting either themselves or some other persons. Obviously, therefore, if you accept my assurances on this score, it is part and parcel of adequate treatment to recommend such prophylactic measures as shall be likely to reduce the liability of re-infection within the lowest possible limits. Clearly, it avails little to effect a perfect cure, if, within the space of a few weeks, and time after time, the disorder be contracted afresh.—*British Medical Journal, Feb.* 7, 1874. *p.* 167.

DISEASES OF THE URINARY ORGANS.

28.—ALBUMINURIA IN CASES OF VASCULAR BRONCHOCELE AND EXOPHTHALMOS.

By Dr. J. WARBURTON BEGBIE, Edinburgh.

[Albuminuria and Bright's disease are not synonymous terms, for there are many cases of the former which are not owing to the existence of the latter. The present paper is intended to draw attention to the presence of albumen in the urine, as a notable feature in some cases of vascular bronchocele and exophthalmos].

My attention having been called to the association of an albuminous condition of the urine with vascular bronchocele and exophthalmos, in the first instance, owing to the existence, in certain cases, of œdema of the feet and ankles, I became satisfied that the association is by no means of infrequent occurrence. I have found albuminuria in a considerable number of the cases which have lately fallen under observation, and it is more than likely that it may have existed in other instances, although unrecognised from causes to which reference will be made. Albuminuria has existed in persons of both sexes suffering from this disease, and has been more common in cases of female than male patients; but when the much greater frequency of vascular bronchocele and exophthalmos among women than men is kept in view, the symptom has been present in a larger proportion of males than females. In some it has been an evanescent symptom, lasting only for a short time, and when so, only present in limited degree. In others, the albuminuria has been very considerable—it has even been excessive, and it has lasted for weeks, indeed for months—while the other notable symptoms of the complex malady continued, and only disappeared as the latter became relieved or removed. Œdema of the lower limbs, although in the first instance calling attention to the condition of the urine, has not been observed to bear any constant relation to the albuminuria; on the contrary, œdema, and sometimes considerable anasarca of the legs, have been present without any appearance of albumen in the urine; and albumen, when present, has generally existed without any form of dropsical swelling. In the most notable cases of albuminuria in connection with vascular bronchocele and exophthalmos, dropsy has not been present.

In prosecuting my inquiry on this subject, a very interesting circumstance became manifest, namely, that the albuminuria was in certain cases limited to the period of digestion—present immediately after a meal, and absent when the person fasted. I had in one case been not a little puzzled by noticing the strange variety presented by the urine within very short periods—the albumen present in considerable quantity one day, and absent the next—present in the urine of the forenoon, and not to be detected in that passed before dinner. By obtaining repeatedly specimens of the urine in this case, and in one or two others, I was able to satisfy myself that in this disease the albuminuria is apt to possess the remarkable character of only occurring during or immediately after the digestion of the food.

This appears to me to be a most interesting feature, and it is also in various ways a very important one. The existence of albumen might readily escape detection if the physician relied on the results of one examination, as he is not unapt to do; and even when taking more than usual care, it is quite possible that in several specimens of the urine furnished by the same patient for examination it might be found that no albumen existed. Conceive a patient so affected consulting a medical man before breakfast or luncheon; his urine, carefully examined, found to be non-albuminous; but, from some cause or other—and we know how fickle some patients are—calling for another physician shortly after a meal, when a large precipitate of albumen occurred on applying heat as well as on the addition of a little nitric acid. Remarkable as this character of the albuminuria which occurs in cases of vascular bronchocele and exophthalmos is, there is another feature pertaining to it which is even more striking. The albumen is present in much larger quantity after breakfast than after either luncheon or dinner. In one case of this kind, which I was able to observe at intervals for a considerable time, the urine presented the following characters:—It was passed in average amount, was of healthy colour, reaction, and density. On no occasion was the density observed to fall below 1015, and it never rose above 1025; the average density was that of health, 1020. Very occasionally this urine deposited lythates, and from time to time contained a slight excess of earthy phosphates. Sugar was never present. Albumen existed in this urine daily for upwards of a twelvemonth, but only at certain times of the day, and these times were readily found to be shortly after meals. After breakfast, however, the amount of the albumen was invariably greater than after luncheon, dinner, or an evening meal.

It is surely a satisfactory consideration that a condition of excessive albuminuria—the urine becoming nearly solid on the application of heat and addition of nitric acid—may, after all, not indicate the existence of any structural change in the kidney. Of course, in connection with the albuminuria, the presence or absence of certain other important features must, under such circumstances, be taken into account. Apart from the intermittent character of the coagulability, the facts that the quantity and density and colour of the secretion did not deviate from the healthy standard, and still more, that diligent and repeated examination by the microscope failed to detect the vestige of a cast of any kind, were to be regarded as the proofs of the renal derangement being functional and not organic. Still, there is occasion for reiterating the assurance that albuminuria is not Bright's disease, and for pointing out that, when unconnected with the presence of blood or pus in it, there may be even a highly coagulable condition of the urine, due to causes which are wholly independent of any structural change in the renal substance. Such, I am persuaded, may confidently be affirmed of the albuminuria which is apt to occur in cases of vascular bronchocele and exophthalmos.

Some little time after my attention had been called to the peculiar features of the albuminuria which is incident to cases of vascular bronchocele and exophthalmos, and to which reference has now been briefly made, a very instructive instance of the malady fell under my notice in the person of a medical man, who had been for some years engaged in active practice in the south of England. Calling one day for advice, the gentleman in question told me that he was the subject of Bright's disease, and feared that little or nothing could be done for his relief.

He certainly looked ill, was thin and sallow in appearance, and evidently deeply depressed in spirits. Having, however, noticed, as he entered my room for the first time, that he possessed the prominent eyes, with peculiar staring *expression* so characteristic of vascular bronchocele and exophthalmos, I ventured—in reliance on my previous observations—to offer the comforting suggestion, that possibly, if not probably, the presence of albumen in his urine might be due to causes which were capable of being removed, and did not indicate the existence of any serious disease. This remark he received with politeness, but with very evident incredulity, mentioning that his condition had already been condemned by medical authority, that he did not expect to be cured, but only felt justified in expecting a little prolongation of life with greatly impaired health. On carefully examining this gentleman, a bronchocele of considerable size, soft, and pulsating, of whose existence he had been unaware, was discovered. His pulse was small, and as frequent as 140 per minute. The urine, on its earliest examination, while the patient was fasting, and between 1 and 2 P.M., was, to his own surprise, found to be free from albumen, its density 1020, and of acid reaction. Subsequent examinations of the urine determined its decided coagulability after meals, and its freedom from albumen while abstinence from food was practised. This gentleman was exceedingly nervous, and very desponding. Under treatment a considerable improvement took place. As he had occasion to pass through London, I begged him to see Dr. Johnson, and it is to him Dr. Johnson alludes in a letter of date 27th October, 1872, from which I now quote:—"There is a striking resemblance between his case and that of the Scotch clergyman whom you were so good as to send to me. Dr. ——— is extremely feeble and nervous, and I fear that the prognosis is bad. I quite agree with you, that a long sea voyage would be the best course for him, but he seemed unwilling to do anything that would separate him from his family." I have not seen this gentleman for some time, but his progress can be traced in the correspondence I have had with him. On the 28th of March 1873, he wrote:—"I have gone very comfortably through the winter, considering all things. I am much stouter than when you saw me, and much stronger. I do a good deal of walking in the course of the day. My heart still beats very rapidly, but its action is not so irregular as formerly. Whether albuminuria exists, I cannot say, as I never test for it, and try to banish the thought of it altogether from my mind. The goître is decidedly less in size, but is still visible."

I obtained two specimens of urine passed on the 10th of April, 1873. That voided before food was taken, contained deposit of lithates, and was absolutely free from albumen. That passed an hour after breakfast had a density of 1016, and contained a very faint trace of albumen. The letter which accompanied the specimens of urine will exhibit the peculiar nervousness under which my friend still laboured. "I send you," he wrote, "two little bottles with urine which I passed to day. Please do not send me a bad report, as it will only frighten me. I dread the thought of renal disease so much, that I try to banish the very existence of kidneys from my thoughts. That I shall look with anxiety for your next letter, and yet dread its arrival, your knowledge of my nervous condition will assure you." Happily, I had no occasion to send "a bad report," while, in consideration of his highly sensitive and nervous state, I wrote over the seal of my communication, "good news."

Since the summer of 1873, this gentleman's condition has steadily improved. On the 28th October he wrote:—"I am glad to be able to tell you that my wife was confined on the 27th ultimo, and has done very well indeed. For myself, the only trouble I now have is occasional palpitation." Again, on the 8th December:—"My general health is now very good. I am as fat as ever I was, and my eyes have lost that unnatural stare. There is still a slight enlargement of the thyroid gland, and sometimes—though rarely—my kidneys act very little. Digitalis and iron remedy this. My heart still beats fast." My last communciation, of date 17th January, 1874, gave a most satisfactory account of his progress, and was written a few days before he embarked as surgeon of a ship sailing with the royal mails for Madeira, Ascension, and the Cape of Good Hope. Before obtaining this appointment, my friend was obliged to go before the medical officer of the General Post-Office for examination as to personal and professional fitness. In this communication he says:—"I find no remedy relieve me so much as bromide of potassium, in 20-grain doses, thrice daily. Aconite and digitalis do not relieve me much."

Having indicated the nature of the albuminuria which is found in certain cases of vascular bronchocele and exophthalmos, it now becomes necessary to inquire a little more fully into its pathology. Albumen is, in all probability, not a constituent of healthy urine. It has, indeed, been stated by Dr. Gigon of Angoulême, that albumen exists in normal urine, and can be thrown down by chloroform. Becquerel, Aran, and Parkes have satisfied themselves that the precipitate which is produced by the addition of chloroform to the urine is not albumen, but a mixture of chloroform, mucus, and organic substances. Albumen, however, is so frequently present in the urine, and occurs under so vast a variety of circumstances, that it becomes a matter of very great importance to determine its clinical significance. The existence of blood, pus, or spermatic fluid in the urine renders it coagulable; but it is scarcely necessary to state that in the cases of vascular bronchocele and exophthalmos to which I have referred, there were none of these conditions. Again, the excessive use of a diet composed chiefly or entirely of albuminous matter, such as eggs, has been found by various observers—among others, Barreswil, Hammond, and Brown-Séquard—to produce albumen in the urine. Barreswil, after taking ten eggs, passed albuminous urine for twenty-four hours. There can be little doubt that, in some persons peculiarly constituted, the partaking of certain articles of food difficult of digestion by them, produces albuminuria for a time. Of this nature was the case of a student mentioned by Sir Robert Christison, in whom a large amount of cheese or pastry produced albumen in the urine. Apart, however, from errors of diet, as Dr. Parkes has stated, temporary albuminuria will occur in persons with very slight disease. Beneke, when suffering from dyspepsia, noticed albumen in his own urine four times in four weeks. Clemens, Rayer, Martin Solon, and many other physicians, have made similar observations. It may be admitted, then, that albumen, although not an ingredient of healthy urine, may occur in the urine of healthy persons, or of persons whose disorder of health is, at the time of its presence, very slight. Attention has recently been called by Dr. George Johnson to the occurrence of albuminuria in healthy persons after bathing in cold water. Again, albuminuria is apt to occur in relation to a great many disorders which are not essentially connected with structural change in the kidneys. Not to dwell upon pregnancy, as a condition of the system with which albuminuria is associated, there is the puerperal state. Again, there is a large number of febrile and inflammatory diseases of the urine in which albumen very often occurs. Among these may be mentioned scarlet fever, measles, erysipelas, small-pox, diphtheria, typhus and typhoid fever, cholera, &c., and of inflammatory diseases, pneumonia. Over and above these relationships there are various forms of visceral disease—of disease affecting the heart, liver, and lungs—in the urine of which albumen appears; and the physician, in his observation of such cases, is on the outlook for its occurrence, and ascribes it, when it does come, to the general impediment to the circulation which the following diseases—(I name them as illustrative examples only) dilatation of the heart, cirrhosis of the liver, emphysema of the lungs—produce; for, owing to these the renal circulation necessarily suffers. In such diseases, the albuminuria is almost invariably associated with a diminished secretion of urine. It is not necessarily so in the albuminuria of vascular bronchocele and exophthalmos,—the quantity is generally unaffected, and so are the other characters of the urine.

Dr. Roberts of Manchester has very cleverly pointed out that in endeavouring to determine whether the presence of albumen in the urine be dependent upon the existence of organic disease of the kidneys or not, the question in each individual case must be considered in connexion with the three following points:—1. The temporary or persistent duration of the albuminuria; 2. The quantity of the albumen present, and the occurrence and character of a deposit of renal derivatives; 3. The presence or absence of any disease outside the kidneys which will account for the albuminuria.

Now, viewing the albuminuria of vascular bronchocele and exophthalmos under these aspects, the following observations may be made:—1. The albuminuria is temporary; for according to Dr. Parkes's definition of that condition, it has totally disappeared while the patient is under observation; but, instead of lasting a few days or weeks, as holds true of most instances of *temporary* albuminuria, properly so called, it has lasted for many months, indeed for a year. Intermittent or remittent albuminuria would be a better signification than temporary, for the albuminuria of vascular bronchocele and exophthalmos, but, better still, because more definite albuminuria occurring during or after digestion; 2. The amount of albumen which is present in the urine when the cause of its manifestation is independent of organic or inflammatory disease is usually small; very often it is not more than a mere trace. The amount of albumen in the urine when passive congestion of the kidneys results from cardiac or other visceral disease may indeed be considerable, but I do not remember ever to have seen the urine under such circumstances very highly coagulable; very highly or excessively albuminous has, however, been the character of the urine in at least one example of vascular bronchocele and exophthalmos, the recovery in which has been complete. Again, in the temporary albuminuria of other maladies, there is generally some deviation, often notable, from the normal condition of the urine in other respects; the quantity, density, and reaction of the urine are often affected; and there is the presence of lithates in excess, or an undue amount of earthy phosphates. Not so, generally at least, in the cases of vascular bronchocele and exophthalmos which I have seen. With the exception of its containing albumen in considerable or large amount, the urine has been healthy. Neither has there been in the latter any deposit derived from the kidneys. In this respect, indeed, there is the interesting fact of a copious presence of albumen in the urine, without any trace of casts of one kind or another, and without any renal epithelium or blood. Further, the form of albuminuria which we are now considering, differs from any other form hitherto described, in being limited to the period of digestion of the food. 3. The presence of disease apart from the kidneys is of course conspicuous in vascular bronchocele and exophthalmos, while the peculiar morbid condition of the nervous system and of the bloodvessels in that disease, as well as the spanæmia which exists, must, I think, be taken into consideration in our endeavour to determine the pathology of the albuminuria, which we now know to be in some way or other associated with it.

In vascular bronchocele and exophthalmos, there is always present much disturbance of the nervous system—the sufferers from this disease are invariably highly nervous—they are often hysterical. The primary disorder of the circulation, both cardiac and vascular, is of the nature which we associate with derangement of the nervous system. The organs and parts of the body in which the local manifestations of disturbance are seated, are organs and parts freely supplied with bloodvessels and blood,—the thyroid gland, the spleen, which, although not invariably, is often affected, and the deep ocular tissues. To these must be added the kidney. From the failure of due nervous influence, the small vessels, and, it may be presumed, the capillaries, in the thyroid gland, and the deep-seated orbital vessels, become dilated, and the circulation through them in consequence interfered with. We can infer from the consideration of the essential nature of the renal circulation that if an obstruction to the return of blood through the inter-tubular capillaries and veins exists, either from an obstruction in the heart or lungs, or from a disordered state of the vessels themselves—a condition which I believe to exist in vascular bronchocele and exophthalmos—favoured by the more or less watery state of the blood itself, there will occur a transudation of serum, carrying with it albumen, through the walls of the Malpighian capillaries into the tubes, and thus the urine will be rendered coagulable.

But in order to explain the limitation of the albuminuria to the period during and after digestion of the food, it is necessary to regard the increased afflux of blood which then takes place, as leading to an altered physical relation between the blood and the walls of the vessels, and likewise determining an engorgement of the Malpighian capillaries, while the loss of tonicity in the different vessels is thus rendered temporarily more injurious. In other words, the renal circulation, in its comparatively tranquil condition, is unaffected by the disordered state of the capillaries and small vessels; but, when excited by the stimulus of a recent meal, it is unequal to the task, and the resulting interference determines the albuminuria.

But, further, there is something in the character of this albuminuria to ally t with the albuminuria of indigestion, to the occurrence of which a brief reference has been made. Sufferers from vascular bronchocele and exophthalmos have frequently an inordinate appetite and craving for food. They have bulimia; and, in the case I have shortly recorded, this symptom was notably present. A large meal taken hurriedly is not unlikely to influence, as a remote cause, the production of the albuminuria. I think Dr. Johnson's explanation of the greater amount of albumen in the urine after breakfast than dinner, may be correct—the same explanation had occurred to my own mind—that the earlier meal taken after a long fast gets into the blood vessels quickly, and in consequence leads to a greater disturbance of the renal circulation. Besides, hot tea and coffee, with eggs, consumed at breakfast, may be presumed to be articles more likely than others to furnish to the blood the offending material.

I have not attempted to exhaust this interesting subject, but have for the present limited myself to pointing out the occurrence of a form of albuminuria which, so far as I am aware, has not in its details been previously described by any observer, namely, albuminuria occurring during and after digestion in cases of vascular bronchocele and exophthalmos.—*Edinburgh Medical Journal, April*, 1874, *p.* 880.

29.—ON THE TREATMENT OF DIABETES MELLITUS WITH CARBOLIC ACID.

By the EDITOR of the MEDICAL TIMES and GAZETTE.

Dr. W. Ebstein and Dr. Julius Müller, of Breslau, have published in the Berliner Klin. Woch. of Dec. 8, 1873, an account of some observations on carbolic acid in diabetes, and though their facts are at present too few to warrant any decided opinion on the subject, yet they are worth recording, in order that others may repeat their experiments on a larger scale. These authors were led to make use of carbolic acid from *à priori* reasoning. Starting from the theory that many cases of diabetes arise from an increase in the ferment which converts amyloid substance into sugar in the liver, they conceived that substances which are known to arrest fermentation would be likely to diminish the formation of sugar. Prout, Griesinger, and Julius Vogel had all previously made trial of creasote (a body chemically allied to carbolic acid) in diabetes, and found it inert. Carbolic acid has several advantages over creasote as a drug. Its composition is definite, its odour pleasanter, and it can be taken in much larger doses. The carbolic acid was given dissolved in peppermint water, strength 1 gramme (= grains 15) to 300 grammes. This quantity was taken in three days—that is, about six to seven tablespoonfuls per diem.

The first patient was a working dentist, aged 46, who had shown signs of diabetes since October, 1872, after some family trouble. There was no hereditary taint, and he had had no previous illness. He had served seven years as a soldier. He was strongly built, but had in six weeks lost thirty-four pounds weight. Sexual power was much diminished. He came under treatment in February, 1873. His urine was on the average eight litres in the twenty-four hours; specific gravity, 1032; amount of sugar, 2·86 per cent. On March 4, after taking altogether two grammes of the acid, sugar had disappeared from the urine, and its specific gravity was 1013. His weight gradually increased from 173 to 185 lbs., and he ceased to attend at the end of July, all sugar being still absent, though there had been traces occasionally present in the interval. He returned on October 29, complaining of his old symptoms, and was found to have about 2·25 per cent. sugar again present, but it disappeared as before on recommencing the carbolic acid. The patient had a mixed diet, and never a purely animal one, during the whole course of his treatment.

A second patient, a man aged 54, had had a mild form of diabetes, eleven ears, the sugar varying from 2·5 to 5 per cent. in the last few years. The isease was not hereditary. One sister died of some mental affection. He ad been eight times at Carlsbad, with marked temporary diminution of the ercentage of sugar, but soon after leaving it, it rose again to its former eight. In May, 1873, he went as usual to Carlsbad, his urine then containg 5·75 per cent. sugar, and he left it in six weeks with only 1·43 per cent. Ie was immediately placed on the carbolic acid treatment, and the dose gralually raised to half a gramme per diem, and on September 2 the sugar had ntirely disappeared. A previous trial of the acid before the visit to Carlsbad ad entirely failed. This patient habitually adopted an animal diet. The uthors give details of a third case in a man, aged 23, in whom the acid had no perceptible effect. The only case, therefore, on which positive stress can be laid is the first, and there the effects appear undoubtedly referable to the acid. The analogy of other drugs—for example, of bromide of potassium in epilepsy, the efficacy of which varies so much in different classes of cases—would lead us not to expect carbolic acid to act equally in all diabetic patients; but Drs. Ebstein and Müller certainly deserve the credit of showing that it is worthy of a more extended trial.—*Medical Times and Gazette*, *Jan.* 10, 1874, p. 38.

SURGERY.

FRACTURES, DISLOCATIONS, AMPUTATIONS, AND DISEASES OF BONES, JOINTS, ETC.

30.—ON NERVOUS MIMICRY OF DISEASES OF JOINTS.

By Sir JAMES PAGET, Bart., F R.S., Consulting Surgeon to St. Bartholomew's Hospital.

As to swelling of the whole or part of a joint, its absence may be nearly enough to prove that a joint in which there are intense pain or other signs of acute disease, or which has been long painful, or in any way seemed long diseased, is only nervous. Inflammation of a joint, either very acute or of long standing, can hardly be found without visible or tangible exudation in the joint-cavity, or in the textures bounding it. But there are many cases in which you cannot apply this rule for diagnosis. A joint shrunken after long disease may relapse into inflammation without renewed swelling, till perhaps a residual abscess appears. A hip-joint may be acutely inflamed without any discernible swelling; so, less often, may be a shoulder, the exudation being too little to be felt. And, making more difficulty, swelling is sometimes evident in a merely nervous joint; not indeed considerable swelling, but enough to make a mimicry of real disease much more close. You may often see this in the loose tissue by the sides of ligamentum patellæ. The swelling sometimes appears due to such slight exudation as may ensue in any neuralgic part; like the puffiness that may come on in facial neuralgia, or the swelling and congestion of the conjunctiva in some cases of orbital neuralgia. Such swelling is commonly transient and capricious, and the fallacy may be detected by observing that, at its greatest degree, it is not, even after long time or many repetitions, nearly proportionate to the pain or duration of the disease. For a joint which is intensely painful, with acute inflammation, or with ulceration of cartilages, should be, if not at first, yet soon after, considerably and always swollen.

In other cases, swelling of a nervous joint may be due to accidental conditions. For instance, repeated blisterings or repeated paintings with iodine will give, for a time, such thickening and puffing of the subcutaneous tissue about a joint as may be very deceptive when added to the other signs imitating inflammation of the joint. So, again, I have seen such a condition in a very marked degree produced by the long-continued use of ice about a painful joint.

On the whole, then, the absence of swelling makes it very unlikely that a joint is really diseased; so does the presence of only a trivial swelling when the nervous and muscular signs of disease are acute or of long standing; and when swelling exists it must not be counted as adding to the probability of real disease, unless it persists and is independent of such accidents as I have already named. And remember that a sensation of swelling is not unfrequently complained of when no swelling whatever exists. It is just one of the erroneous sensations to which nervous persons are prone, as they are to sensations of unnatural fulness and of weight for which there is no substantial reason. Such a mere complaint of swelling will not deceive you if you compare the suspected joint with its fellow.

But, after all, the sign most to be relied on for diagnosis between real and nervous disease of joints is the temperature. It is so impoartnt to estimate it accurately that I cannot too strongly urge you to be always studying it. You should feel with a broad surface of your hand every joint very watchfully, comparing each that is supposed to be diseased with its fellow supposed or known to be healthy, you learn, as you certainly may, to detect even a small difference of temperature in even a small part of a joint.

When you have learnt to feel accurately the heat of a joint, you may safely rely on it for some certainties in diagnosis. A joint that feels all over cold, or cool, or not above its natural temperature, is not an inflamed joint: whatever may be the other signs of inflammation in it, it is not inflamed; you may rely on this. In the hip and shoulder, however, this test of temperature is not easily applicable. The thickness of various textures covering those joints is too great for additional temperature to be transmitted through them. But in the knee and elbow and other smaller joints, even those of the fingers and of the tarsus and carpus, the test is easily applied and sure; and remember lways, in using it, that in most persons, the front of the knee-joint, and in a ess degree, the back of the elbow, are habitually cool to the touch—cooler han the arm and leg; and that in many the feet are rarely warm in health. These local variations, however, can lead to no confusion if you follow the rule —which, for other reasons as well as this, ought to be never neglected—of omparing every suspected joint with its fellow. Besides, always compare the emperature of the joint with that of the rest of the limb, for the rest of a imb may be, through disease or long defective nutrition, cold; and if one oint in it be always not cold, though it may not be fairly called hot, this may be through its being inflamed. I repeat, then, a joint which is not above its natural temperature is not an inflamed joint. But observe, besides, some rules within this rule.

A joint when first exposed for examination may feel over-warm. This may be due to its having been very warmly wrapped, or thickly covered with water-dressing, or in some other way hindered from cooling to its natural temperature. Wait, therefore, and observe whether, after a few minutes' exposure to cool air, the joint has become of the same temperature as its fellow or as the est of the limb. If it have, you may be very nearly sure it is not inflamed; r your suspicions that it is inflamed may increase with the time during which ts cooling is delayed. Frequently only a part of a joint is inflamed, and in his case the morbidly high temperature can be felt only over the inflamed art. This limitation of a sign of inflammation which one might suppose easily iffusible is a striking fact. It is the same with the swelling, which, especially n scrofulous inflammations of joints is often limited to a single portion.

Not rarely, when you feel a joint quite cool, the patient will tell you that it , nevertheless, at times very hot or burning hot, and that with this heat it wells and becomes red. Such cases are common among those in which the onsequences of sprains and other injuries are very prolonged in nervous persons, or even healthy persons whose joints have been too long treated with old douches or too long kept at rest. These joints are commonly reported as etting very hot every evening, and as being subject to painful heats, tingling nd burning. In any such case you may decide that the joint is not inflamed. f it were, it should be over-warm all day and all night. The occasional heat only due to flushing, such as some nervous people have in their faces after eir meals, or such as women commonly have at the time of ceasing to menruate. I call it flushing, not blushing, for it is not associated with any menl state; indeed it may seem strange that among all the nervous people with ints that are the constant objects of their attention, one never sees blushing f the skin over them when they are being looked at. All such turgescence nd transient heat are consistent with complete absence of organic disease. oubtless the same thing may happen in inflamed parts; they are sometimes otter than at other times, more swollen and more painful, having periods of acerbation; but then they are never of natural temperature—they are always more or less too hot. It is very different with the mimicries of inflamation in joints; here the over-heat is only at times, at night or at some nearly gular hour, or after exercise or fatigue. A joint which is cold by day and ot by night is not an inflamed joint—that is certain.

The certainty of the diagnosis based on coldness is, if possible, increased by coincident duskiness of the skin—by its having the dull purplish tint which is commonly called blue or a dull pink. Such colours may be seen at joints long inflamed, but in these cases they are associated with over-heat; when they are associated with coldness, they are characteristic of anything rather than inflammation. Besides, with inflammation the colours of over-fulness of blood-vessels are only at the inflamed parts; with congestion in cold nervous joints they are commonly much more widely diffused.

Lastly, as to fever associated with a supposed inflammation of a joint. It may help you to diagnosis in some few cases, chiefly in those in which the pain is very severe, or in which there are other signs like any of those of acute inflammation. For no very acute inflammation of a considerable joint can exist without fever; and therefore, when a patient's general temperature is normal, you may be very doubtful, to say the least, whether an intensely painful joint is inflamed. But the reverse is not true; the temperature may be frequently or habitually high, though a suspected joint be only neuralgic or in some mimicry of acute disease. For the patient may be tuberculous, or with some casual illness, may have a high temperature, or he may be recently convalescent from acute fever. Moreover, a very slight degree of inflammation in a joint in a very nervous person may be associated with a disproportionately high general temperature. This is in accordance with a general rule already mentioned—that in those with alert and mobile nervous systems a slight local inflammation may produce or be associated with a great increase of general temperature. In similar persons, fatigue or passing excitement will raise the temperature to 100°, or, I think, to 101°; and in their convalescence from acute illness—a condition in which nervous mimicry, as well as real joint disease, is apt to appear—they have widely variable temperature. You must therefore hold, as a general rule, that in very nervous persons, the temperature must be studied many times, and with circumspection, before it is counted as an addition to the evidences of their having real joint disease. Within the last few days I have been very nearly deceived in such a case. A gentleman, about thirty, had what he described as frightful paroxysmal pain in one knee—pain such as might have been in the most acute inflammation of the joint; and there were some heat, some swelling, and a general temperature of 101°. These things had "come of themselves" within three or four days, and the patient looked very ill. Of course he was treated cautiously; and all subsided so quickly as to make it sure that no serious, if indeed any, organic disease had existed. It was a nervous mimicry ensuing upon excessive and unsuccessful work.—*Lancet, Nov.* 22, 1873, *p.* 728.

31.—NECROSIS OF THE OS CALCIS; REMOVAL OF SEQUESTRUM BY BLOODLESS METHOD; REMARKS.

By Sir W. Fergusson, Bart

The bloodless method is especially valuable in operations like the subjoined, where the manipulations have to be carried on in tissues in close proximity to such important structures as the ankle-joint. The absence of hemorrhage allows of a complete inspection of the diseased part, so that the surgeon can readily ascertain what he is cutting. For the notes of the following case, the interest of which is enhanced by the addition of the remarks made by Sir Wm. Fergusson, we are indebted to Mr. William Rose, surgical registrar.

F. G., a clerk, aged twenty-seven, was admitted Nov. 6th, 1873, with disease about the right ankle-joint. In January last, from no assignable cause, a slight red swelling appeared on the outer and back part of the right heel, which caused much pain and made the patient lame. The swelling increased in size, and burst in May, and discharged a quantity of offensive matter. Since this he has been laid up, and there has been a constant discharge. The pain has been less, and only occasional. He has always enjoyed good health, and *there is no* history of syphilis.

On admission, there was a sinus on the outer side of the right heel, an inch and a quarter behind and below the external malleolus. The opening was surrounded by fungoid granulations, and a probe passed in through it led directly to rough bone at the posterior part of the os calcis, which was evidently diseased. The ankle-joint was quite healthy; no impairment of motion.

Nov. 8th.—Chloroform was administered, and the elastic bandage and india-rubber piping were applied according to Esmarch's method for temporarily arresting the circulation. Sir William then enlarged the sinus with a probe-pointed bistoury, and, after using the cutting pliers, elevator, and sequestrum forceps, removed an irregular piece of necrosed bone about an inch in diameter, representing the whole of the posterior part of the os calcis, which was lying comparatively loose in a cavity partly formed of new bone. After removing the india-rubber tubing there was only slight hemorrhage, which soon ceased; and the wound was filled with oiled lint and covered with water-dressing.

Sir William remarked that necrosis of such a bone as the os calcis was not so common as caries, although it was not unusual to find a small piece of necrosed bone lying in a carious cavity. It was rather remarkable that, although the whole of the posterior part of the bone had necrosed, there seemed no interference with the attachment of the tendo Achillis. Probably the deposit of new osseous matter thrown out had been sufficient for its support.

Nov. 14th.—No constitutional disturbance; wound looking healthy. The cavity is rapidly filling up.

Dec. 6th.—Wound nearly healed.

20th.—Will leave hospital in a day or two.

In making some remarks on bloodless operations, Sir William began by saying that the method of temporarily suspending the circulation, which had just been adopted, was that specially devised by Professor Esmarch, but that the principle was not new; that a somewhat similar process had been carried out in India years ago in cases of removal of large scrotal tumours; and also that it was the frequent practice of surgeons in this country, when about to amputate a limb, to hold it up above the rest of the body for some time before the operation to allow a great portion of the blood to gravitate out of it. He then observed that this was quite a revolution of the practice of the older surgeons, who would have been horrified at such a proceeding as this, since they were in the habit of taking ten, twenty, or thirty ounces of blood from a patient before his undergoing any capital operation. But at the same time Sir William thought that some arguments might be reasonably brought against the uniform adoption of this method; that though there were many cases in which patients could not afford to lose a drop of blood, yet there were others—for instance, in those of amputation in the thigh—where the sudden addition of so large a quantity of blood as that contained in the limb to the rest of the circulation might be more injurious than the loss of some part of it with the limb when removed. He further observed that this bloodless method had been tried by his colleagues with no bad results at present, but that there had not been sufficient time to note whether there was any difference in the progress of wounds after its use. In operations for necrosis especially, this method greatly facilitated the manipulations of the surgeon. Previously the hemorrhage from the vascular structures cut through so obscured the part that the sense of touch was the only guide to the operator; but now the surgeon could see the sequestrum, ascertain its extent, and easily distinguish it from its new bony casing. —*Lancet, Jan.* 3, 1874, *p.* 10.

32.—TWO CASES OF DISLOCATION OF THE HIP REDUCED BY THE AMERICAN MODE OF MANIPULATION.

By Dr. Hector C. Cameron, Edinburgh.

[The author has recently met with the two following cases.]

Case 1. *Dislocation on to dorsum ilii of seventeen days' standing. Reduction at first attempt.*—William Lynn, æt. 16, miner, was driving a horse harnessed to some "hutches," in a coal pit, on the 5th September, 1867. The horse ran off, and he, holding on to the reins, was drawn along by it. Being at last brought on to his knees, the hutches came up behind and struck him on the lower part of the back. When he rose he was unable to stand, experienced pain in the left hip joint, and his "legs clung together the same as if they had been tied with a rope." About two hours after the accident he was seen by a medical man, who recognised a dislocation, and, having produced extension by pulling on the leg, while he kept one of his feet pressed against the perineum, he stated, according to the patient's account, that he believed he had succeeded in effecting reduction; explaining, that the shortening and inversion, which continued to be a source of alarm to the patient and his friends, would in all probability pass off. Thirteen or fourteen days after the accident, the patient still suffering great pain, the doctor returned with a friend. Having administered chloroform, and got the assistance of "two laborers who were working on the road," they produced violent and long-continued extension of the limb, while steady counter-extension was maintained on the axillæ. They left after many attempts, and without succeeding in their object. After a day or two of great pain, he was admitted into the Royal Infirmary. I saw him after the house-surgeon in charge had made an unsuccessful attempt at reduction under chloroform. The head of the femur was on the dorsum ilii, and all the symptoms occurring under such circumstances were clearly marked. Chloroform having been fully administered, I flexed the leg on the thigh, and the thigh on the abdomen, abducted the limb fully, and rotated outwards. During this last movement, I felt the tearing through of adhesions, and the head of the bone at once returned to the acetabulum. On extending the limb, it was seen to be in good position. The patient made a good recovery, and went to the Convalescent Home three weeks afterwards.

Case 2. *Recent dislocation of the hip into the thyroid foramen. Reduction.*—Thomas Yuill, æt. 42, quarryman, was admitted on the 25th April last into a ward of the Royal Infirmary, of which I was in temporary charge.

He had been struck a short time previously by the "jib of a crane," but could not give a very accurate account of the accident. He complained of great pain in his right hip and thigh, and a very slight examination served to show that the head of the femur was in the obturator foramen. There was slight lengthening and marked abduction of the limb; while the knee was slightly flexed, and the prominence of the trochanter was conspicuous by its absence. There was a distinct fulness in the obturator region, although, as the patient was a very muscular subject, the head of the bone could not be defined by the hand. Reduction was effected under chloroform in the following manner:—I flexed the limb as thoroughly as possible, slightly abducted, and then rotated inwards firmly, at the same time abducting the limb and bringing it towards the floor. This resulted in transferring the head of the bone from the thyroid foramen—not to the acetabulum, but to the sacro-sciatic notch. Flexion, abduction and rotation outwards (which so readily transfers the head of the bone usually to the acetabulum in the case of dislocations upwards and backwards), sent it back to the thyroid foramen. A careful repetition of the first set of movements put it again in the sciatic notch; and again, as before, in trying to reduce it from that position, I sent it back to the thyroid foramen. Having again removed it to the sciatic notch, I put my foot in the perineum, and made extension aid my movements, when it slipped at once into the acetabulum. The time occupied was very short, for the head of the bone was changed from the one position to the other with provoking facility. The patient made a good recovery.

The first of these cases illustrates the ease with which a dislocation of the hip may be reduced when extension, fairly and intelligently tried both with and without chloroform, has failed to attain the desired result.

The second case is interesting, chiefly as pointing out a mishap which seems apt to occur in manipulating the thyroid dislocation; namely, the removal of the head of the bone not into the acetabulum, but upwards and backwards into the great ischiatic notch or on to the dorsum ilii. In a paper by Dr. Thomas M. Markoe on "Reduction of dislocated femur by *manipulation*," in the New York Journal of Medicine for January, 1855, this is well shown in several of the cases related. In Case IV. the original dislocation was into the sciatic notch, and Dr. Markoe says: "The effect of the first attempt was to throw the head on to the obturator foramen, making the limb longer than the other, and producing the deformity characteristic of that dislocation. From this point, by a slight alteration of the movement, the head could be made to slip back to its original position. Between these two points it could be made to play backwards and forwards, but could not enter its socket." Much the same thing is described in several other cases. In all the cases of thyroid dislocation, it would seem that extreme flexion was employed and rotation was found to succeed, whether made outwards or inwards. It seems as if too great flexion is the cause of the bone slipping from the one position to the other without entering the acetabulum. At all events, this is the opinion expressed by Bigelow and Hamilton, and in the case related above, I flexed the limb to the fullest possible extent.

Bigelow writes in regard to Dr. Markoe's case of thyroid dislocation: "These cases are instructive as showing that the head of the bone is directed towards the socket when the Y ligament is wound upon the shaft by rotation, *whether inward or outward*, and they correspond to the results of my own experiments, made before I had read them. . . . It may be superfluous to add, that in Markoe's case, inward rotation would probably have reduced the bone, had the thigh been less flexed." Hamilton says: "In one example mentioned by Markoe, it is pretty evident that the head of the femur was thrown into the ischiatic notch, by having flexed the thigh too much, so that 'the knee touched the thorax.' Indeed, it is questionable whether it will be best ever to bring the thigh much, if at all, above a right angle with the body, since any further flexion can only throw the head below the acetabulum, when, in fact, it is already too low." In backward dislocations, on the other hand, the flexion should be firm and complete.—*Glasgow Med. Jour., Aug.*, 1873, *p* 444.

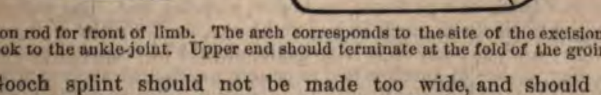
33.—OBSERVATIONS ON EXCISION OF THE KNEE JOINT, AND ITS AFTER-TREATMENT BY MEANS OF DR. P. H. WATSON'S PLASTER-SPLINT.

By Henry J. Tyrrell, Esq., one of the Surgeons to the Mater Misericordiæ Hospital, Dublin.

[Mr. Tyrrell strongly recommends the splint which forms the subject of this paper as the most convenient, the safest, and by far the most comfortable splint for the patient, and also as the one requiring least after-care on the part of the surgeon. Dr. Watson, in his work on "Excision of the Knee-Joint," published by MacLachlan & Stewart, Edinburgh, gives the following description of his apparatus for the after-treatment.]

It consists essentially of two parts—1, a suspension rod made of iron, about the size of No. 5 of trade wire gauge; 2, a modelled Gooch (scored) splint, long enough to extend from the tuberosity of the ischium to beyond the heel.

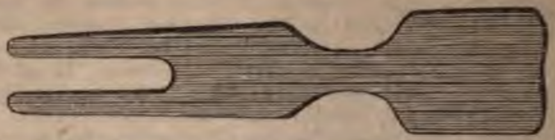
The suspension rod extends from the groin to the extremities of the toes, and is bent to the outline of the limb, departing from it only in the situation of the excision, where it forms a bow or arch. To the upper surface of the rod are attached one or more hooks, by which suspension is effected.



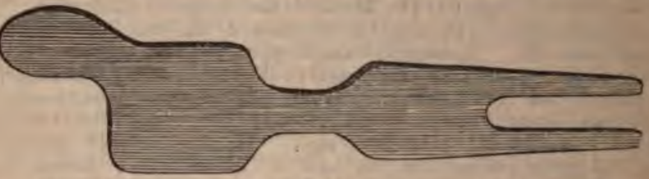
Suspension rod for front of limb. The arch corresponds to the site of the excision. The suspension-hook to the ankle-joint. Upper end should terminate at the fold of the groin.

The Gooch splint should not be made too wide, and should certainly not surround the thigh and leg more than two-thirds of their circumference. It should be scooped away laterally, at the point corresponding to the tendo-

Achillis and heel. The inferior extremity of the splint is thus of a horse-shoe or stirrup-shape, and admits of the ankle and foot being supported by the lateral horns of the splint, as they fold on each side of the malleoli, without the risk of inordinate pressure being made upon the tuberosity of the os calcis.



Posterior "Gooch" splint of ordinary form, hollowed out in each side, on the level of the operation wound, and cut out at the lower part in a horse-shoe or stirrup form, so as to relieve the heel and tendo-Achillis from pressure.



Alternative form of "Gooch" splint for excision, hollowed out as in Fig. 2. The rounded part above adapted to the os innominatum, and secured by means of strips of adhesive plaster. (*This latter splint I have not used.*)

In application, the limb is first laid and carefully adjusted upon the posterior splint, which should preliminarily be padded with lint, and covered with gutta-percha tissue, or hot paraffin, in the situation which corresponds to the site of the operation. The iron rod is then placed in front, and folded lint laid between it and the limb at the groin (where the rod terminates above) at the upper part of the tibia, and at the bend of the ankle. The whole is then rendered immovable by means either of plaster-of-Paris applied by the hand, of a consistence like thick cream, or of paraffin, which, having been rendered temporarily liquid by heat, is applied by a large paint-brush. When the application has solidified, the patient may then be removed to bed, and the limb suspended from the running pulley of a Salter's swinging cradle, or from the roof bar of the common iron wire cradle, employed to support the weight of the bed clothes. I do not regard it as a matter of indifference whether plaster-of-Paris or paraffin is used. Each has its advantages. The plaster-of-Paris is firmer and not liable to be affected by heat like paraffin, but it has the disadvantage of permitting soakage of discharge, it takes longer to consolidate, and when consolidated is less easily chipped through, by means of a bandage-shears, than the paraffin apparatus. I have, of late, in the treatment of excisions and of compound fractures, employed the plaster-of-Paris as the substantial substratum, and thereafter applied the paraffin over it, as soon as consolidation and drying have become complete, so as to secure its greater toughness, and its complete impermeability to fluids of any kind—whether blood, or serum, or pus—from the line of incision, or water from the wet dressings which may be applied during the healing of the wound. The foot and limb should not be much raised above the level of the mattress upon which the patient lies, as this is apt to lead to an awkward position of the limb when the patient first begins to move about. When in bed, the sacrum and hips must be protected from all risks of inordinate pressure by means of a large square corrugated air-cushion, completely covered with a blanket or draw-sheet. This will be foundto be most evenly and comfortably supported upon a firm hair mattress, laid either on a thick hair palliasse, or better, a spring bed. The patient should be encouraged to sit up as much in bed as he can, even within a day or two of the operation; and, as soon as it is possible, should be removed out of the bed during the day, either to another bed or couch, or, should such not be attainable, to a mattress laid on the floor. In sitting up, the air-pillow, placed beneath upon the seat, will be found a great comfort, as it admits of easy relief to the irksomeness of maintaining nearly the same sitting posture for a period of several hours.

Case 1.—Mary D., aged fourteen years, was admitted into the Mater Misericordiæ Hospital on the 1st of March, 1873, for disease of the right knee-joint.

For the last two years she had been in various hospitals, at different times; and, her friends not being willing to allow her leg to be amputated, she left, without deriving any benefit from treatment.

On admission the knee was found to be much enlarged, white in colour, and when at rest she did not complain of much pain; but when the heel was percussed, or the leg rotated, she complained bitterly. There were three sinuses discharging pus—one opposite the external condyle, two corresponding to the inner side of the head of the tibia. A probe passed through the openings entered the joint, and gave the sensation of grating against roughened bone. On rotation of the leg, and placing the hand over the knee, it became apparent that the cartilages were gone, and the ends of the bones bare, irregular and rough. The leg hung like the handle of a flail, and was much atrophied. The little girl was very thin and exhausted from long confinement, but there was no evidence of any visceral disease. She stated that three years before she fell and hurt her knee, and that since she had been a cripple.

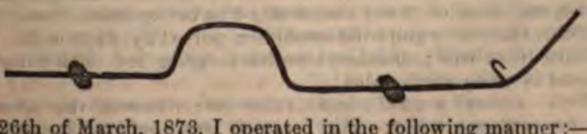
She was ordered to remain in bed, a splint was applied to the leg, and every attention paid to her general health; and as it was evident an operation would be required, I ordered her 15♏ of the tincture of perchloride of iron three times daily (a practice I invariably adopt when time permits before performing any serious operation, as I believe, to say the least of it, it tends to lessen very much the liability to erysipelas and pyæmia).

Looking on the case as one of primary disease of the synovial membrane, believing the bones to be but little affected, and the girl's health tolerably good (also she was most unwilling to lose her leg, and begged of me not to cut it off), I determined to excise it.

Knowing how difficult it has proved, in all the cases of the excision of the knee that I have seen treated in Dublin, to keep the parts in position with the usual box splint—how irksome and constrained the patient's position must of necessity be—how unfavourable such a position is to a speedy convalescence—how it must necessarily render the operation more dangerous to life—I determined, if possible, to avoid using it; and having had considerable practice in the use of plaster-of-Paris in the treatment of simple and compound fractures and diseased joints, I was struck with the simplicity of Dr. Watson's apparatus, and made up my mind to give it a trial. Before doing so, however, I tested it in the following manner:—I gave Dr. Watson's pamphlet to Mr. Corcoran, of the firm of M‘Adam and Corcoran, instrument makers, and desired him to measure my patient's leg and thigh, and to make the scored splint and suspension rods according to his directions. Having procured the splints and rod, I applied them to the diseased leg, and allowed the apparatus to remain on for two days. It proved, so far, a decided success; my patient expressed herself pleased with it; it gave no pain, allowed her, when it was suspended by a piece of bandage from an ordinary bed-cradle, to turn and shift her position with the greatest ease and comfort.

As the suspension rod moved a little from side to side, I directed the instrument maker to make another for me, adding two small flanges of tin, each about two inches in diameter, which (I hope to be pardoned for saying) is, I think, an improvement, as it entirely prevents *wabbling*.

The flanges, being thin, are easily bent to correspond with the convexity of the leg and thigh.



On the 26th of March, 1873, I operated in the following manner:—

The patient having been put fully under the influence of ether, a tourniquet applied, and an assistant having taken charge of the sound leg, I entered the point of a strong scalpel above and behind the internal condyle of the femur, pushed it down to the bone, and then carried the incision downwards in a curved direction, severing the ligamentum patellæ, and then upwards and outwards to the upper and back part of the external condyle, where it terminated exactly opposite to where it commenced. I then dissected up the flap with the patella contained in it. I then removed the patella, and next, having divided the lateral ligaments, an assistant having forcibly bent the leg, I carefully cut across the crucial ligaments, and caused the condyles of the femur to project from the wound. I next cleared the condyloid surface of the femur for the saw, *as I well knew if I removed the whole epiphyseal line, arrest of growth should follow.* I carefully avoided doing so by making a horizontal section from before backwards, commencing in front *below* the upper margin of the trochlear surface of the femur. I then removed a thin slice of bone from the tibia, and having clipped away with a strong curved scissors all the thickened synovial membrane, I twisted four vessels, sponged out the wound with a solution of chloride of zinc (10 grs. to the oz.), and finally, having inserted two *pledgets of tenax* (rolled into plugs about four inches long, and as thick as the little finger) into the most depending angles of the wound, the limb was straightened, and I commenced to apply the apparatus in the following manner:—I first carefully wiped away all blood from the thigh and leg, and thoroughly dried the surface. I next applied a thin flannel roller from the toes to the groin (except for six inches at the knee), and then adjusted the limb on the posterior splint, which was previously well padded with fine cotton wadding, particularly in the centre corresponding to the popliteal space, and opposite the two malleoli. A piece of gutta-percha tissue was laid on over the cotton wadding in the popliteal space. The iron suspension rod was next applied in front, and a little wadding placed here and there under it made it lie accurately. A number of coarse muslin bandages, each three inches broad and six yards long, having been previously prepared by rubbing plaster-of-Paris into them, and rolling them up dry, were immersed in water, and quickly applied from the toes to the bow of the iron rod, and then under the knee and up the thigh to the groin. The bandages were applied three-fold, and a little fluid plaster afterwards, laid on with the hand, strengthened and at the same time rendered the whole apparatus more artistic looking. The edges of the wound were united everywhere but at the angles (here, as before mentioned, two plugs, or, more properly, two most efficient drainage tubes, were inserted) by four carbolized gut sutures. The splint in the neighbourhood of the knee was then well painted with fluid paraffin, and a large pledget of tenax *covered* with muslin was placed on the wound. The ends of the lateral drainage tubes were placed in the tenax over the wound, and a bandage, pretty tightly applied round the knee, under the iron bow, kept all secure. The patient was then removed to her bed, previously prepared by placing a firm hair mattress on a spring-bed, and the limb was suspended by a piece of bandage passed from the lower hook to the roof of a wire bed cradle—the heel being raised one inch from the bed. When she fully recovered from the anæsthetic effects of the ether, she became very restless—in fact, she became quite drunk and difficult to restrain. She sat up in bed, and wanted to get up altogether. Towards evening she became more tranquil, took some strong beef-tea, and a glass of wine, and at ten o'clock p.m. she had a draught containing 15 ♏ of Battley's sedative and 15 grs. of chloral hydrate. Immediately after taking the draught she fell into a tranquil sleep, and did not awake till six o'clock the following morning.

March 27th. Is a little hysterical, but otherwise perfectly comfortable; not much pain in the wound: pulse 95. Ordered to take beef-tea, milk, and arrowroot biscuits, and 4 oz. of wine; the anodyne to be repeated if required.

March 28th. Did not require the anodyne; pulse 92; slept well.

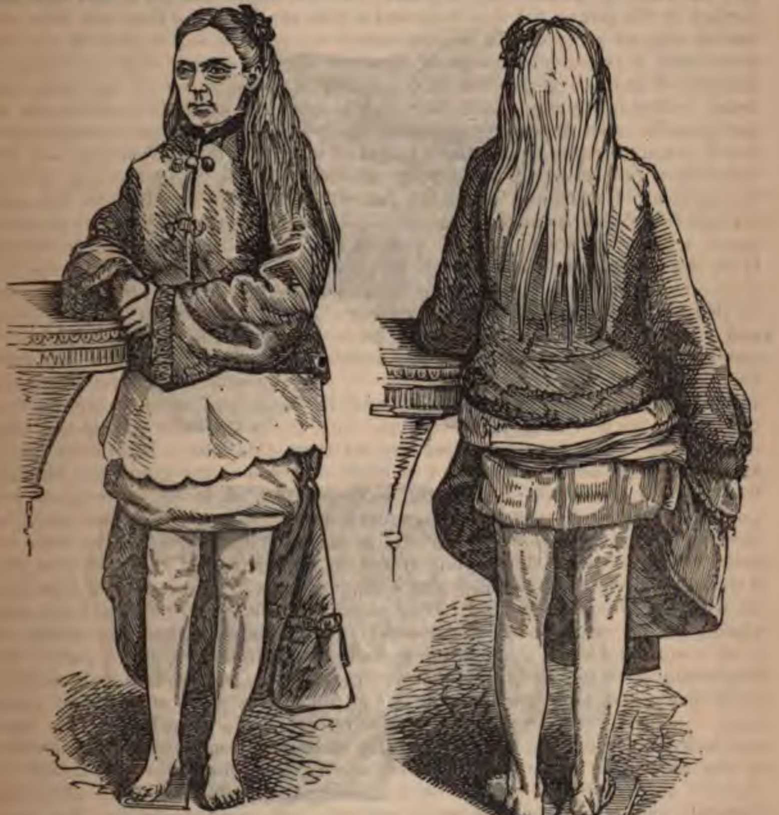
March 29th. Slept well; took her breakfast sitting up; settled her hair herself; says she is quite comfortable.

March 30th. Passed a good night; pulse 88; removed the dressing, and drew out the drainage tubes. The dressing was perfectly saturated with a sanguineo-serous discharge. Except where the drainage tubes were inserted the wound is quite healed.

(The wood engravings are taken from photographs executed on Nov. 20, and they speak for themselves.)

Before applying the tenax dressing again I softened a piece of paraffin with my fingers sufficiently to permit me to insert it like a wedge between the skin and the plaster bandage at the angles of the wound. This sufficiently prevented any discharge from trickling into the popliteal space.

April 10th. Every second day the tenax dressing was renewed since last report, and before re-applying it the whole exposed surface of the knee was sprayed with a weak solution of carbolic acid (10 grs. to the oz.). There is a moderate discharge of pus from the inner angle; the outer angle almost healed. The gut sutures have come away in the usual way by absorption of the portions inserted into the living tissues. She sits up every day, moves about from one side of the bed to the other. Several surgeons who have seen the case are quite pleased with the apparatus, and Dr. Ladly, from the United States, remarked that the only part of her person she could not injure by falling out of bed was her resected knee.



May 21st. Removed the splint to-day for the first time; union perfect; slight mobility; operation wound long since healed. The openings of two of the sinuses present before the operation not quite healed. Re-applied the apparatus more lightly; cut off the portion of the splint projecting beyond the foot; allowed to move about with crutches.

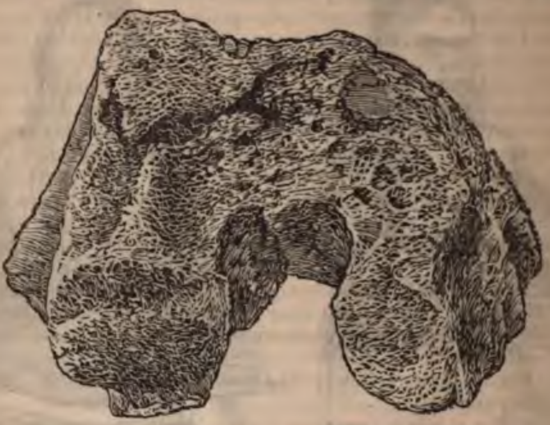
June 14th. Walks with one crutch; spends most of her time in the pleasure-grounds of the hospital.

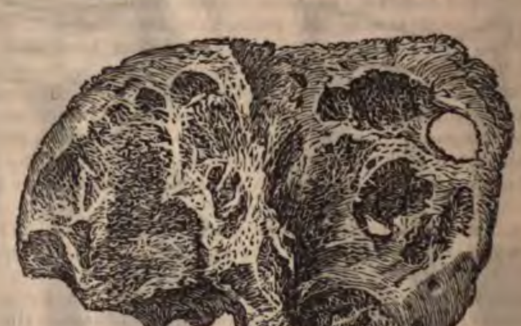
Nov. 20th. Splints all removed; perfect bony union; she walks and runs with perfect ease. A cork heel in the inside of her boot, ¾-inch thick, prevents limping, and as she walks or runs no one would suppose she had been subject to so severe an operation.

The wood-engravings, natural size of the portions of the bones removed, show accurately the amount of disease present, and the peculiar worm-eaten appearance of the articulating surfaces of the femur and tibia. The cartilage was also removed from the patella, but otherwise it was not diseased.

Case 2.—Thomas C., aged ten years, a deaf mute from St. Vincent's Asylum for the Deaf and Dumb, was admitted into the Mater Misericordiæ Hospital on the 10th of March, 1873, under the care of Mr. Tyrrell.

On admission we were informed that the boy had been labouring under disease of the knee-joint for two years. The affection did not arise from accident. The disease was at no time attended with much pain, and until six months ago he was able to get about with a crutch. Since then he preferred remaining in bed. The medical officer of the institution had employed various remedies, but without benefit.





On examination the joint was found to be very much swollen, globular, elastic to the touch, painful when pressed on, and at three points fluctuation could be felt. The boy was small for his age; had been always delicate and dull; his head was large and misshapen. He had a slight cough, but there was no evidence of organic disease of the lungs, kidneys, or any of the other viscera. The case was evidently one of pulpy degeneration of the synovial membrane in an advanced stage, and I recommended amputation; this, however, the boy's father would not hear of.

Under these circumstances I gave a trial to rest and Scott's bandage locally, and the administration of cod-liver-oil and iron internally. This treatment was continued for a month without benefit. Three abscesses formed on the knee and burst externally, and on examination with a probe were found to communicate with the joint. Amputation was again proposed, but would not be permitted. Although I looked upon the case as one unsuitable for excision, owing to the enormous thickness and diseased state of the synovial membrane and the general unhealthy state of the boy, I gave him the chance, as I had no other alternative.

On the 25th of April I operated, as in the first case. In reflecting the soft parts to expose the bones, a quantity of thick, gelatinous fluid escaped. The synovial membrane was fully one and a half inch in thickness in some parts. It was of a yellowish brown colour, soft, and easily broken down. The cartilages were softened and easily scraped off the bones. The bones were a little expanded, but otherwise not perceptibly diseased. The patella was literally buried in the synovial membrane. I removed, with an ordinary saw, the extremities of the femur and tibia, as before described, and also dissected out the patella, and then, with a strong curved scissors, clipped away as much of the diseased soft parts as I could; much, however, was left. The wound was sponged with a solution of chloride of zinc. No vessels required to be twisted or ligatured. The immovable apparatus was applied, as in the first case, and the patient removed to bed, and the leg swung to a cradle. As soon as the little boy recovered perfectly from the ether, fifteen drops of Battley's solution were given.

April 26th. Passed a good night; seems quite cheerful, and moves about in his bed with ease.

June 14th. Everything going on well; the same dressing was used as in the other case. He hooks his leg up and lays it down on the bed himself when he is desired to do so by signs. The operation wound nearly healed; the sinuses show no disposition to heal.

July 18th. Removed the splint for the first time. No union; position perfect. Re-applied apparatus.

August 10th. For the last few days he has declined much in health. Appetite failing; he sweats at night; pulse 110; no cough; urine albuminous, specific gravity 1·010. He was removed to another ward, and shifted every day from his bed to a sofa placed in the open air.

August 17th. Complains very much of his head, the light gives him annoyance, and he excludes it by burying his forehead in his pillow, and drawing the sheet over his head. The poor little dumb fellow excites great compassion from his inability to make known his wants. On the 20th he had a fit; his sphincters became relaxed; he lay in a semi-comatose state for three days, and then died.

The knee was the only part we were allowed to examine. The ends of the bones were in good position, but not the faintest attempt at union had taken place.

Although the result of this case was unsatisfactory, still, for my present purpose (viz., to prove how efficient and comfortable the immovable apparatus of Dr. Watson is), it answers, perhaps, even better than a successful one.

The case was an unpromising one from the beginning. The operation was not one I would have selected had I liberty to choose. Still, from the day the operation was performed till the patient died (three months), the leg gave him no trouble; when suspended from the cradle he could move freely from one side of the bed to the other, he could hook it up and lay it down himself without trouble or pain. There was no tenderness at any time since the operation over the sacrum or nates; and I believe I can, with truth, assert that with any other splint he would not have lived so long—nay, more, I do not consider the operation to have hastened his death; but, on the contrary, to have retarded it; inasmuch as he suffered less inconvenience, was less constrained in his movements, more master of his own actions, was in every way better able to assist himself after the operation than for months before. He died, not from the operation, but in spite of it.

Before I conclude this paper I shall refer to the use of tenax as a dressing, and to my mode of using it. When I introduced it first, about eighteen months ago, in the Mater Misericordiæ Hospital, it was found occasionally to give rise to extreme irritation when applied to a granulating surface, or to the skin of a delicate female (as after amputation of the breast); to obviate this I have, for the last twelve months, first placed over the wound or surface to be dressed *a piece of thin washed muslin*, and then the tenax. This mode of dressing answers admirably, and is in constant use in the hospital. It prevents the fibres of the tenax from irritating; and when you wish to change the dressing you do so with great ease and cleanliness by raising the muslin, and with it the superimposed tenax saturated with the discharge. Ordinary lint is the very worst application that can be applied to a sore or suppurating wound, its want of capillarity prevents any discharge that may be present from flowing away. After amputations, resections, and, indeed, all operations, I invariably use the tenax in the manner described, and, as a rule, do not remove the first dressings for four days.—*Dublin Journal of Medical Science, Feb.* 1874, *p.* 97.

34.—CASE OF PRIMARY EXCISION OF THE ANKLE-JOINT; WITH OBSERVATIONS.

By Henry Lee, Esq., Surgeon to St. George's Hospital.

The author describes what he believes to be the only case in which complete primary section of the ankle-joint has been performed; and advocates the plan, both in primary and secondary excisions of the joint, of dislocating the tibia and fibula *outward* so as to allow of the articulating surface of the tibia being removed with comparatively little disturbance to the surrounding parts. The articulating surface of the artragalus is also more easily removed in this way than by dislocating the bones of the leg inward, as has commonly been attempted in secondary excisions of the joint. In many of the so-called excisions of the ankle, the extremities of the tibia and fibula have alone been removed, and the articular surface of the astragalus has been left either partially or altogether. In other cases, where a complete secondary resection of the joint has been performed, the bones have been divided by a thin saw whilst they have remained *in situ;* a proceeding, according to the author, involving considerable disturbance and risk to the surrounding parts. In other cases, again, an incision has been made on the outside of the joint, and a dislocation of the bones attempted; but this cannot be satisfactorily accomplished so long as the internal malleolus is left. The plan advocated, therefore, is to remove the internal malleolus first, and then the tibia and fibula may be dislocated outward through the external wound with great facility, and without interfering with any important structures. Such a mode of operating has not, he believes, been hitherto described. The patient was exhibited at the meeting.—*Lancet, Feb.* 7, 1874, *p.* 198.

35.—IRON-WIRE FOLDING FRACTURE BOX.

By Dr. G. W. McNalty, Surgeon, Army Medical Department.

Having experienced great difficulty in the Franco-German war in dressing gunshot wounds of the leg without disturbing the wounded extremity, particularly in cases of compound fracture accompanied with considerable discharge, I have been led to devise an apparatus which I trust will prove of service.

The apparatus, which may be termed an iron-wire folding fracture box, is made of galvanised iron to prevent rusting, and consists essentially of a back piece, sides, and foot-rest. The back piece is 22 in. long, 5¾ in. broad at the upper, and 4½ in. broad at the lower end, and has an oval-shaped opening to accommodate the tendo-Achillis and heel. The sides are not so broad but are of the same length as the back piece; they are joined to the latter by two hinges on each side, and are kept applied to the limb by a webbing buckled on the thick wire at the upper edges. The foot rest is 10 in. long and 3¼ in. broad, and can be firmly secured at different angles by iron loops which fix on wires projecting from two small wings, thus preventing any movement either backwards or forwards. At the end of the back piece is a wire support, to raise the foot so as to prevent pressure on the heel. When not required it can be turned up, and fits into the space between the two wings.

The advantages are these:—Bandages, with the exception of the one for the foot, are rendered unnecessary. The wound can be pressed by letting down the sides unless it be confined to the back of the limb, and in most cases can be seen without disturbing the apparatus. The foot-rest remains fixed when the sides are let down; this is a most important point, as it is well known that one of the greatest difficulties in the treatment of fractured leg is to prevent either inversion or eversion of the foot. The sides and foot-rest are sufficiently high to keep off the pressure of the bed-clothes. The apparatus is convenient when it is necessary to move the patient; it is also simple in construction, durable, moderate in price, light, cool, easily kept clean, equally applicable to the right and left extremity, and allows the leg to be placed on the side. All the component parts being fastened together, it will always be found complete.

The object I have specially kept in view is to make it suitable for military practice, or on occasions in civil life where a large number of persons may be injured at the same time, as in colliery accidents, &c. It is therefore constructed to pack into the smallest possible compass, the sides folding behind the back piece, the wings on one another, and the foot-rest lying flat when unfastened. The weight without pads is 2 lbs. 8 oz. Messrs. Weiss and Sons are the makers.—*Lancet, Nov.* 8, 1873, *p.* 660.

36.—A SELF-ADJUSTING SLING ARM-SPLINT FOR PATIENTS CONFINED TO BED.

By H. Hammond Smith, Esq., House Surgeon to the Middlesex Hospital.

Recognising the value of Mr. Callender's sling-splint for the arm, the authorities of this hospital at once determined to adopt it; but it was found that the apparatus of Mr. Callender was not suited for the beds at Middlesex Hospital. To remove this objection, the senior house-surgeon, Mr. H. Hammond Smith, has introduced several modifications, by which the splint may be used with almost any bed.

The apparatus consists of a wrought-iron support, a sling, and a counterweight. The support is made of an iron bar bent at a right angle, the vertical part of which is round and works in a socket fixed by hooks or screws to the bed-head, so that it can be moved to right or left readily by the patient when necessary. At the angle of the support is a small iron pulley over which the cord of the sling runs, and on the horizontal arm is another pulley, which can be moved backwards or forwards at pleasure.

The sling is made of strong canvas, with four holes on each side, and supported by four cords with hooks, which can be so placed in the holes of the sling as to give the arm or hand any angle or position. These sling-cords are attached to the single cord which passes over the pulleys, and has at its other extremity a bag containing a shot, so that the weight may be increased or diminished as occasion requires.

The advantages of this sling are, that the patient need not constantly have his arm in one position, that the position may be altered without assistance, and that it can be applied to almost any bed. It has been used with great success in cases of cellulitis of the arm and hand, in amputation about the hand, and in one case of compound fracture of the humerus.—*Lancet, Feb.* 14, 1874, *p.* 232.

37.—A NEW APPARATUS FOR THE TREATMENT OF FRACTURES OF THE PATELLA.

By Dr. W. J. Wheeler, Surgeon to the City of Dublin Hospital.

The treatment of fractures of the patella being one of the most interesting and practical subjects connected with surgery, and being one which has occupied the attention of the profession of late years, and especially in Dublin, I thought it might not be uninteresting to bring under your notice this evening an apparatus which I believe to be in every way suitable for the treatment of such fractures, and more particularly have I been induced to do so from the knowledge that almost every method which has yet been introduced in the hope of obtaining perfect or bony union has met with objections on some justifiable ground, and has not been followed by the satisfactory results anticipated by sanguine advocates. The cases of this accident which have come under my care and observation have led me to an anxious consideration of the various apparatus suggested, and to the conviction that their defects are obviated in the splint now before you, although I have not yet had an opportunity of testing it practically in its present form. It would be unnecessary for me, before such a society as this, to enter into a discussion on the causes and varieties of this accident; suffice it to say that it may occur directly or indirectly, either from violence directly applied or from muscular action. The fracture may be transverse (which is most commonly met with) or stellate, or vertical, multiple, simple, or compound. The transverse most usually occurs by muscular violence, a sudden action of the quadriceps extensor femoris under effort to prevent the body falling backwards (being the common cause), the knee at the time being slightly bent; occasionally however, transverse fractures are the result of direct violence. The diagnosis of this fracture is not difficult—the history of the case, the loss of power, the bulging of the synovial sac; the distinct separation of the bones, which in the transverse fracture is sometimes very great, depending partially on the amount of muscular action at the time of the accident, and afterwards by the effusion which takes place into the knee-joint. As to the character of the effusion, which is said by some to be serous, by others hemorrhagic, for my own part I am inclined to think it the former.

Before describing my splint, I must mention some of the appliances that have from time to time been employed in the treatment of this accident, in order that the merits I claim for the apparatus before you may be more clearly understood.

The first method I would mention is the plan adopted by Sir A. Cooper, namely, a leather strap or collar, buckled round the thigh above the broken bone, another strap passing under the middle of the foot, the leg being extended and the foot considerably raised, the strap is brought up to each side of the patella and buckled to the band or collar applied to the lower part of the thigh.

The next I would mention is Malgaigne's hooks, which are composed of four claws drawn together by a screw, which act on the upper and lower fragments, and Malgaigne states that the advantage of them is that the edges of the fragments are brought into direct apposition, and that they are not tilted forwards.

The next is Mr. Wood's splint, extending from the tuber ischii to below the foot, having two hooks attached; it is placed on the posterior part of the limb.

Another plan is that recommended by Mr. Butcher; he uses a forked splint on the anterior part of the thigh, firmly bandaged to the limb, the forked part being towards the upper fragment and pressing it towards the lower.

There are several other appliances, such as india-rubber bands above and below the patella, straps of plaister, pads above and below the fragments, and others, too numerous to mention.

In the apparatus now on the table, is a hollow wooden splint, 4¾ inches wide, and extending from above the middle of the thigh to the sole of the foot, at which point a footboard is attached by means of a hinge; this splint having two transverse bars, is fitted into a long box-splint, the sides of which are forty inches long and six inches in depth, in which it travels horizontally. That portion of the splint on which the limb rests can be elevated or depressed as required, by means of perpendicular slots cut through the sides of the box-splint. Thus, the splint can be adjusted to suit a long or short leg, the limb can be elevated or lowered at pleasure, and the foot placed at any angle. This hollow splint is fixed in position by means of thumb-screws, which fit into the transverse bars before mentioned; two semilunar pieces of metal, softly padded, are fixed one above the other below the fractured patella by means of leather straps which pass round the limb, the leg is secured to the splint by means of two broad web straps, one round the calf, the other at the ankle; the foot can be bandaged to the footboard; a roller with each adjustment is fitted in the box-splint below the footboard. From this roller start four cords, which passing through brass sheaves, are attached, two to the upper and two to the lower metal pads by means of chains and light hooks. The roller is turned by means of a key, and acting on the cords, causes the metal pads simultaneously to approach each other, thereby bringing the fragments into apposition; the rack is covered by a brass box, which can be locked, so that the adjustment of the splint cannot be interfered with by the patient.

The advantages of this apparatus will be more briefly detailed by comparison. To those who advocate Malgaigne's hooks, I claim all the advantages without the objections; my splint provides for the position of the limb, Malgaigne makes no such provision; this apparatus will procure perfect coaptation of the fragments without penetrating the soft parts, without the pain and irritation frequently caused by the hooks. In several cases treated in London by Malgaigne's method, erysipelatous inflammation has endangered both the limb and life of the patient. It is superior to Sir A. Cooper's method, by drawing on both fragments, and can make well-maintained traction of the lower as well as on the upper fragment, if necessary—and over both the plans just mentioned, by its being able to exert greater or lesser force on each fragment, as the case may require. It would be tiresome were I to enter into a comparison with all the other contrivances that have been used for this fracture.

In conclusion, I will only add, that not only is this splint suitable for the fracture for which I have introduced it, but I believe also for fractures of the thigh or fractures of the leg.—*Medical Press and Circular, Dec.* 31, 1873, *p.* 586.

38.—FRACTURE OF THE CLAVICLE TREATED BY PLACING THE ARM BEHIND THE BACK.

M. Broca recently treated a case of fracture of the left clavicle, in which the fracture was near the middle of the bone; was oblique, from above downwards and from without inwards, the fragments riding one another considerably. After numerous plans of treatment had failed to reduce the fragments, M. Broca adopted a suggestion made last year by Dr. Michel to the Sociéte de Chir., and which is reported in the Jour. de Méd. et Chir. Prat. He placed the arm in a semiflexed position behind the back, where it was retained by a bandage for eighteen days, with the effect of completely adjusting the fractured surfaces and producing an excellent cure. For a few days longer a sling was used. The patient, a man of considerable nerve, complained of the inconvenience and pain of the method for only the first twenty-four hours.—*Medical Press and Circular, Dec.* 24, 1873, *p.* 574.

39.—ON EXSANGUINATION OF LIMBS BEFORE OPERATIONS, AND ON TORSION OF ARTERIES.

By the Editor of the Lancet.

It may be fairly asked whether something like the attainable limits of even conservative surgery have not now been reached. Within the last few years mild and compromising measures have been successfully substituted for bold and radical operations: limbs formerly condemned have been saved by the comparatively small sacrifice of a joint, and cases which in the opinion of the older surgeons would have been considered hopeless are now treated with the confident assumption of a prospect of recovery. Quite recently surgical conservatism has gained still greater conquests; for, by the adoption of a method of operating for which we are chiefly indebted to the active intelligence of Professor Esmarch, a joint may now be excised, or even a limb removed, with little more than the mere sight of blood. During the present year, also, another method of performing bloodless operations has been revived by Professor Dittel, who has removed large tumours, and even amputated the thigh, by means of gradual strangulation by an elastic ligature. What will be the verdict finally passed on these procedures, time alone will show; but, so far, it appears that the exsanguination of a limb before operation is so simple, and in many cases so advantageous, that it must become a constant and universal practice. It is doubtful, however, whether the second plan of operating will find favour with the majority of practical surgeons. It is evident that, before it receives full recognition, its pretensions must be more clearly defined, and its powers more accurately determined. It is difficult to imagine that the excision of a breast or the amputation of a limb by the tedious process of strangulation will be generally preferred to the more certain and more speedy use of the knife. But for the removal of small growths, or for the removal of tumours in situations too dangerous for the employment of a cutting instrument, a strangulating ligature may prove to be of the greatest service, and free from danger.

Intimately associated with the subject of bloodless operations is that of the arrest of primary arterial hemorrhage. Practically, four methods are in use—namely, ligature by whipcord or silk, ligature by catgut or other animal substance, torsion, and acupressure. Much has been said by advocates and opponents for and against each each of these methods. Some, firmly wedded to this plan or to that, will never allow any merit to a rival. The question at issue, however, is not solely the arrest of the hemorrhage, but includes also the subject of the primary union of wounds. It has been asserted that the ordinary ligature, which is acknowledged on all hands to be a most efficient mode of securing a bleeding vessel, is attended with the serious disadvantages that many days must elapse before its separation, and that the irritating thread and the distal fragment of the ligatured vessel by their presence act as foreign bodies to prevent the speedy union of the wound, and thereby increase the dangers of secondary hemorrhage and other disastrous results of operation. On the other hand, the advocates of the use of a ligature made of some animal substance, and the advocates of torsion or of acupressure, have each alleged that these evils do not occur with the employment of their [favourite method. The reintroduction of ligatures made of some animal substance said to be capable of absorption, and the use of torsion of arteries, are mainly due to the efforts of those who practice antiseptic surgery; who argue that when the vessel is secured with a substance such as catgut, the ligature becomes organised and incorporated with the surrounding tissues, so that no dead fragment is left in the wound to retard its union.

A similar advantage has been claimed for the torsion of bleeding vessels. The principles of torsion are based on a knowledge of the anatomy and physiology of arteries. The arteries contain in their walls a muscular tissue, which contracts and retracts whenever the vessel is divided, the more so if it is rudely lacerated. This muscular tissue, which exists proportionately in the greatest degree in the small vessels and in a smaller degree in the large ones, is ruptured when an artery is twisted. But the unequal distribution of muscular tissue is a serious objection to the exclusive application of torsion. Another objection is to be found in the fact that the arteries are liable to be extensively diseased, so that the muscular tissue is changed or degenerated in such a manner that it no longer fulfils its normal functions. If, therefore, arteries so affected be twisted, there is a danger lest the process necessary for the temporary and permanent closure of the vessels shall not take place. Torsion is, *moreover*, not adapted for cases where the artery cannot be sufficiently with*drawn to* allow of free twisting, and is useless where hemorrhage occurs from the side of one vessel, as when a branch has been divided close to the trunk. It appears, therefore, that torsion requires for its complete and safe employment a healthy subject, sound arteries, good instruments, and a persevering operator whose acuteness of vision is very great, for the difficulties of torsion are largely increased if the sight be at all defective; it will, indeed, generally be found that, as surgeons advance in years and become presbyopic, they discard torsion for the more simple procedure of the ligature. While, then, the principles of torsion are, that by twisting the vessel the muscular coat is ruptured and becomes contracted, and that its roughened edges act as foreign bodies, which whip out the fibrin, and on which lymph is subsequently deposited, it seems practically to be a matter of small importance whether the end of the vessel is twisted off, or whether only a few turns are made. But the question in dispute is whether the bruised and twisted end sloughs off or retains its vitality. The truth is, that much depends on the size of the vessel and the manner in which torsion is performed, for if the piece have not been severely bruised it may preserve its vitality; but if it have been twisted too frequently, or if the tissue be much broken, there can be no doubt that the end does often slough off and act as a foreign body.

Notwithstanding the strenuous advocacy of its ingenious originator, acupressure has not gained much favour with English surgeons, and it may indeed be fairly doubted whether it is a means deserving of implicit confidence. There is no proof that the use of the needle produces division of the muscular coats, which experiment and experience have shown to be a very important factor. It is no proof to say that it is frequently successful, for at least it is liable to failure for the reason just stated; and it is not safer than the ligature or torsion, over which it cannot be truly said to possess any advantage sufficiently great to compensate for the risk incurred by its use.—*Lancet, Dec.* 20, 1873, *p.* 883.

40.—AMPUTATION AT THE HIP-JOINT BY ESMARCH'S METHOD.

By Dr. Charles J. Gibb, Consulting Surgeon to the Newcastle Infirmary.

Having occasion last week to amputate the left leg of a youth eighteen years of age at the hip-joint for malignant disease of the femur, I did so by Esmarch's method, and the result was so satisfactory that I desire to record the case, more especially as, so far as I am aware, this is the first time amputation at the hip-joint has been performed by the bloodless method. The tumour was a very large one, and the limb œdematous. The elastic bandage was applied from the toes to the perineum, a good-sized pad of linen placed over the aorta, just below the umbilicus, and seven or eight turns of elastic tubing made over it. Not a drop of arterial blood was lost till the ligature was loosened, and then only one vessel spouted, all the others having been secured. When the femoral vessels were divided,about two ounces of venous blood escaped. No inconvenience was experienced from the application of the tubing tightly round the belly, and after the operation neither the lad's countenance nor his pulse indicated that any great shock had been sustained.

I am indebted to Dr. Page, the house-surgeon to the Newcastle Infirmary, for suggesting to me to try Esmarch's plan in this case.—*Lancet, Jan.* 31, 1874, *p.* 181.

41.—EXTENSIVE DISEASE OF KNEE-JOINT; AMPUTATION THROUGH THE THIGH; BLOODLESS OPERATION; RECOVERY.

Case under the care of J. Kellet Smith, Esq., Stanley Hospital, Liverpool.

[The patient was thirty-nine years of age, and had suffered from knee-joint disease for four years.]

Although the case seemed very unsuitable for the employment of Esmarch's bloodless method, it was determined at any rate to try it, and at first it was thought that excision of the knee might be performed.

On Nov. 7th the patient was put under chloroform in the ward to allow of his being removed to the operating room. Upon his arrival there the chloroform was discontinued, and ether used instead. The limb was tightly bandaged, from the toes to the upper third of the thigh, with elastic bandage about four inches in width, after which a stout elastic cord was worked spirally over all until it reached the last turn of the bandage, when it was fastened, and the bandage removed. An incision was then made on the inner side to fully expose the knee-joint, which was found to be so extensively diseased that amputation was decided upon. The incision made on the inner side of the limb was continued forwards, embracing the patella, and brought to the outer side, thus making the anterior flap. The knife was then passed behind the femur, commencing at the same point as the anterior, was carried forwards for a short distance, then abruptly downwards, thus making a short posterior flap, about one-fourth the length of the anterior. After the arteries, which were very much enlarged, had been tied with carbolized catgut, and the part well washed with carbolic-acid lotion, the flaps were brought together and secured. A piece of spiral wire drainage-tubing was passed through the part, and made to project about an inch on each side; some pieces of lint soaked in carbolic oil were then put on the part, and the patient removed. Upon making the first incision on the inner side of the knee a large amount of pus, and about an ounce of clot escaped, but during the rest of the operation there was no blood lost. The patient complained of no pain, such as was supposed would be caused by the sudden gorging of the vessels with blood, upon the removal of the cord.

The convalescence was rapid, there being no bad symptom throughout, and the patient has regained a florid, healthy look; he has also increased in weight.

At this operation, in spite of the excessive œdema, the elastic compression and bandaging most effectually succeeded in making a bloodless operation, and although the pressure was kept up for half an hour there was no sloughing. In a fortnight firm union had taken place in the deep parts of the stump. —*Lancet*, *Dec.* 20, 1873, *p.* 878.

42.—ON BLOODLESS OPERATIONS AS ILLUSTRATED BY THE USE OF THE GALVANIC CAUTERY.

By Thomas Bryant, Esq., Surgeon to Guy's Hospital.

[The principle of Esmarch's practice is not new, having been acted upon in Guy's Hospital for many years, more particularly by Mr. Hilton. The extension of the principle, and the exact mode of its application contain, however, much novelty, and Professor Esmarch has made it a practical success.]

There are, however, associated with the method under consideration some disadvantages of no mean importance, and the greatest of these is the prolonged and general oozing of blood from the tissues which invariably follows the removal of the tourniquet. I do not mean the arterial bleeding that takes place from the arteries, large or small, for such can be effectually controlled by well-applied torsion, but I mean the capillary oozing of blood from the general surface of the wound; the soft parts, as it were, on the removal of the constricting band, filling like a sponge and oozing blood in a passive way; the capillaries, that have been entirely deprived of their blood by the elastic pressure, apparently losing their contractile power for a time and in this way giving rise to a passive hemorrhage; this general oozing converting what might have been a bloodless operation, so far as the mere cutting was concerned, into an operation as a whole of a totally different character, and tempting me to think that it would be wise to describe the operations performed by Esmarch's plan as examples of bloodless operating, reserving the term bloodless operations to those performed without the loss of blood from the beginning to the end.

How far, again, this secondary oozing of blood may interfere with or prevent the immediate or primary union of a wound is not yet proved. That it must do so in a degree is palpable to all, for you must know that oozing to any extent between the edges of a wound or between flaps necessarily prevents rapid repair.

Care is likewise called for in the application of the elastic band, for too tight compression must do harm. Langenbeck has already recorded three instances in which, by this pressure applied to the upper extremity, some paralysis of the median nerve followed.

In aged subjects, and in others in whom the vessels are diseased, the practice does not commend itself, for in such the complete obstruction to the flow of blood into the tissues is likely to be followed by sloughing or want of action. The same result is likely to arise when the carbolic-acid spray is employed after Lister's system; the influence of the spray upon bloodless tissues having an evil tendency,—at least so it has appeared from some cases which have occurred in the practice of my colleagues.

With respect to the bloodless method of operating by the elastic band, as suggested by Professor Dittel, one word must be said, although that word at present cannot be in its favour. It had its origin in the remarkable case of a girl, aged eleven, who died in March, 1872, from meningitis, which was found after death to have originated from the gradual pressure of a thin elastic cord, such as that employed to keep 'a hair-net in position; this cord, by its constant pressure, is said to have produced a circular furrow around the skull-cap, even through the bone, and in parts through the dura mater. From this case Professor Dittel was induced to apply the elastic ligature to others, and from November, 1872, to the end of 1873 he has by means of the elastic ligature cured nævi, cases of fistula in ano, and sinuous passages. He has, moreover, removed by it a cancerous breast, amputated three legs, and ligatured many arteries. He also recently reports that he has by the same means treated cases of hydrocele, phimosis, and many examples of cancerous and other tumours. In this country the practice has been taken up by Sir H. Thompson, who applied it for the removal of a breast. The success, however, of the case, as at present known, is not encouraging, the breast not coming away for twenty-eight days. The elastic ligature may be useful for the removal of pedunculated tumours—and for such cases I have employed it since 1862—and for some rare examples of anal fistula and allied cases; but for what are called capital operations I cannot at present understand its value. It possesses also one grave practical objection, and that is the fetor connected with the sloughing process; and unless its advocates can show that it possesses some equivalent advantages it will never find a place in ordinary surgical practice, although in exceptional cases it may possibly prove of use.

With these few remarks, therefore, upon what have been described as bloodless operations, the merits of which are not yet clear or proved, I propose to pass on to consider with you the clinical value of the galvanic cautery, to show how it really possesses what the other plans claim—a true bloodless character,—and to illustrate by examples the different classes of cases in which it is applicable.

It is not my intention to go far into the literature of the subject; for if I do I may get into trouble, and fail to give credit where credit is due. But I feel bound to tell you that it was Mr. Marshall, of University College Hospital, who first used a galvanic cautery for surgical purposes, although to a very limited extent; and that it is to the late Dr. Middeldorpf, of Breslau, that we are really indebted for the introduction of a splendid battery, and a set of instruments which have rendered the galvanic cautery an agent of practical value. In 1854, he introduced both, and published a monograph upon the subject. In this country, however, the practice was little known, and never followed, and it was not till 1868 that I became acquainted with it. In that year Dr. Middeldorpf sent over to Dr. Beigel a battery with a case of instruments, and through Dr. Beigel's courtesy I had an opportunity of using them. I did so from February 26th, 1868, till the end of May, having selected from my out-patients at Guy's a large number of cases upon which to employ it. From the experience thus acquired I became so convinced of the value of the instrument that I persuaded our *treasurer at* Guy's to provide one for the hospital, and since that time it has been in active use. At my suggestion, Messrs. Krohne and Sesemann, who supplied the one we have at Guy's, obtained a second for themselves, which they work for private purposes; and in this way the galvanic cautery has been introduced into London practice.

There is no necessity for me to describe the battery or the instruments that are in use. You have seen them employed too often in this theatre, for a vast variety of cases, not to know all about them: not to know that the battery is a very powerful one, and that when in good working order it will heat the largest cautery we have and keep it heated—either at a white, red, or black heat; the different degrees of heat being regulated by the number of cells employed—many cells representing a high temperature, a less number a moderate one. You know, also, that the number of cells required is much determined by the thickness of the wire or cautery-iron—a thin wire or small cautery being more readily heated, and kept heated, than a thick wire or large cautery.

The battery is a Bunsen's. They are not quite like those you have seen me use in this hospital, but they are of equal if not of greater value, and the battery is more portable. They are those which Mr. Krohne has for private use, and which he has been led to make. They represent the improvements which a long practical acquaintance with the instruments has produced.

I trust I shall be able to show to you that it is with such a battery as this, and with such instruments, that the true bloodless operations are to be performed; and that, in comparison with this method, all other so-called bloodless methods are delusions. But in saying this, I must impress upon you that to secure this result much care is called for and much discretion. The battery must be in good working order, and the instruments complete and whole: no make-shift will suffice in any way. If the battery does not work well, the required heat will not be maintained; if the instruments are imperfect, some break in the galvanic current may take place, and thus failure must ensue. As a matter of precaution, before operating, the surgeon should, therefore, test the battery and instruments that are to be employed, and in this way save trouble and prevent disappointment or, possibly, failure.

I will now proceed to discuss the different classes of cases to which the cautery is applicable; admitting at once the charm the practice possesses, and understanding to a degree how Dr. Middeldorpf, its originator, was disposed to apply it to almost every branch of surgery. I propose to consider its use under different headings, and to demonstrate its value by brief records of cases. I shall commence with the operations upon the tongue, penis, and anus; and pass on to those for the removal of tumours, the destruction of cancers, and the cure of lupus; winding up my observations upon the subject by showing its value in the treatment of nævi.

On operations on the Tongue.—There are no operations of importance that the surgeon has to perform which have been more benefited and simplified by the introduction of the galvanic cautery than those upon the tongue; for there are none in which, without its use, hemorrhage is more troublesome or dangerous, and there are none, with its use, which more satisfactorily illustrates its bloodless character. Indeed, before the introduction of the galvanic cautery or écraseur, operations on the tongue were very rarely performed. During my student's career I can recall but very few instances in which this form of operation was undertaken; and I well remember even so bold a surgeon as the late Mr. Aston Key asserting that he never undertook such a case if he could avoid it.

At the present period, however, a different tale has to be recorded; for, at most hospitals, operations on the tongue are of common occurrence—at our own I may safely say that they are much looked after; and the reason for this great change in practice is, doubtless, to be found in the improved means we have at hand for the performance of the operation, more particularly the introduction of the écraseur wire and galvanic cautery. For by the use of such instruments, carefully employed, no fear of bleeding need disturb the mind of the operator; and what was formerly a very serious measure has become comparatively a simple one.

Operations on the tongue are mostly called for on account of cancer; and, when this affection exists, the sooner it is removed the better, for there are few, if any, local cancers that are the cause of more local distress, if left alone, than that under consideration, and there are few operations for cancer that give greater comfort than those upon the tongue.

I will now proceed to consider the operation for its removal, and will say again what I have said before, that, of all ways, the removal of the tongue by the galvanic écraseur is the one to be adopted. I lay emphasis upon this opinion, not only because I believe it will be endorsed by every surgeon who has had any experience with the galvanic écraseur, but because at no very distant date a learned teacher of surgery in this metropolis, in a published lecture upon cancer of the tongue, went so far in an opposite direction as to say that, "to remove the disease, the least painful, most manageable, and most effective plan is no doubt by the application of the knife or scissors." In his experience this opinion was doubtless correct; but such an opinion, I venture to suggest, could only have been given in ignorance of the value of the galvanic or even of the wire écraseur.

The operation.—For the removal of a cancerous nodule of the tongue, or of a cancerous tongue wholly or in part, the first thing a surgeon has to do is to isolate the part to be removed; and this can usually be effected by the introduction of long pins, ivory pegs, or curved needles in handles beneath the base of the growth, in the way I have illustrated in my work on the Practice of Surgery, page 262; and in doing this the surgeon had better go wide of the disease. Having isolated the growth by this means, fixed the mouth open by a gag, and had the tongue drawn forward as far as possible, either by means of tongue forceps or, what is far better, a whipcord ligature passed through the tip of the organ, the loop of the galvanic écraseur is to be passed round the base of the disease behind the pins, and gradually tightened; the connexion between the poles of the battery being made as soon as the wire loop has been adjusted, but not before. The process of tightening and cauterising may then be carried out; and, when performed successfully, the part to be removed will quickly fall off, without the loss of a drop of blood. In this process of tightening and cauterising much care is called for. In the first place, the wire that is employed should be thick or twisted. I believe the twisted platinum wire is better than the thick. This wire should not be heated beyond a red heat, and the redness ought to be of a dull kind. But, above all, the process of tightening should be very slowly performed, the wire of the écraseur being screwed home only as it becomes loose by cutting through the tissues; any force may break it, and thus give rise to difficulties, or cut through the tissues too rapidly, and give rise to bleeding. Whenever bleeding follows the operation that has been described it is from one of two things—the wire cautery has been used at too great a temperature, or has been screwed up too rapidly. The surgeon had better take a few minutes longer at this operation than fail in obtaining its good effects.—*Lancet, Feb.* 28, 1874, *p.* 289.

43.—BLOODLESS OPERATIONS.

By Dr. Gesualdo Clementi, Naples.

[Dr. Clementi in lately visiting the different hospitals in London, observed the frequent use of the elastic bandage to prevent hemorrhage in cases of amputation, or other operations performed on the extremities. He desires to bring to the notice of English surgeons the fact that both these methods had been practised and publicly described for several years in Italy.]

Dr. Silvestri published in 1871, in the Gazzetta Medica Italiana, (anno xiv., p. 397), that in cases of amputation he constricted the limb tightly above the part that was to be cut by means of an elastic band, the ends of which were provided with hooks so that they could be fixed without the aid of an assistant. Professor Vanzetti has latterly modified the procedure of Grandesso-Silvestri by bandaging the limb preliminarily. In a book published before Professor Esmarch read his communication to the yearly meeting of German Surgeons at

Berlin, Vanzetti (Cenni storici sulla Universita di Padua, p. 163, 1873), wrote as follows:—"Following the example and instructions of the author himself (Silvestri), instead of using the tourniquet in amputations, we are accustomed to constrict the limb by means of an India-rubber band, and with so good results that ordinarily the stump does not lose a drop of blood. Before applying the elastic band, it is our custom to bandage the limb very tightly from its extremity to the point of amputation, and to keep it in an elevated position for several minutes in order that it shall retain as small a quantity as possible of venous blood."

It will be thus seen that Esmarch has only the merit of employing more generally a method already described and practised in Italy.

Billroth in the Wien. Mediz. Wochenschrift of July last, in regard to Esmarch's method remarks: "I read in a work of Vanzetti's that this procedure has been described in 1871 by Dr. Grandesso-Silvestri, and that since that time this method has been employed successfully in the Clinique at Padua."

In regard to the use of the elastic ligature in amputation of the mamma, and all the other applications of it made by Professor Dittel, in consequence, as he says, of an "accidental case" (Allgem. Wien. Mediz. Zeitung, Nos. 7 and 8, 1873) I ought to state that Dr. Grandesso-Silvestri, in 1862, published in the above cited Gazzetta Medica Italiana an account of operations performed by means of the elastic ligature, and expounded scientifically the principles through which he had been led to adopt the method. I cannot do better than quote what he then wrote:—"In reflecting on the force which this substance (gum-elastic) exercises continuously and incessantly in order to return to its natural dimensions, I thought that it might be applied to further uses, and principally to cutting the tissues without producing dangerous hemorrhages. In fact, a ring of gum-elastic applied in a state of extension does of itself what an inextensible cord does when regulated by a screw-écraseur. Although slight, the pressure exerted by this substance is sufficient to obliterate the small capillary vessels, and to deprive the part with which it is in contact of the material elements of nutrition, and cause it to mortify by its incessant action. . . . . When this first thin layer has mortified, the elastic ligature exerts its power on the next, and continues thus to cut uniformly all the soft tissues. If in this march of destruction it meets a large vessel, before cutting it the internal surfaces are so applied against each other as to interrupt the current of blood, and before the coats are cut through obliteration will have certainly taken place."

After having thus so well explained the *modus operandi* of the elastic ligature on the tissues, the author relates a case of nævus, of a vaginal polypus, and of a scirrhus of the mamma operated on with excellent success.

In the publication of 1871 (*loc. cit.*) he remarks that he has employed with good results the elastic ligature; (1) in occluding arteries after amputation; (2) in excision of varicose veins, angiom, and very hard fibroma of the uterus; also, from a single instance that he met with, he believes that the bones can be severed by the incessant and progressive action of the gum-elastic.

That, after his publication, the other surgeons in Italy employed his method is very clearly proved by what Vanzetti, Clinical Professor of Surgery at Padua, has written in the book I have quoted above, and which was published before Professor Dittel made his communication to the Medical Society of Vienna (Feb. 14, 1873).

Vanzetti writes: "In order to spare as much as possible bloody operations, where it is practicable, instead of the knife the elastic ligature is employed: and that since 1862, when this method, rational and not rarely very convenient, was introduced into surgical practice and published by Dr. Grandesso-Silvestri, pediculated tumours where they can be constricted at the base, hemorrhoidal tumours, prolapse and fistula in ano, nævus, and fistulous sinuses, frequently, as has been exemplified in the practice of the inventor, and in this Clinique, find in this method a sure and bloodless remedy."—*Lancet, Jan.* 3, 1874, *p.* 35.

44.—ESMARCH'S BANDAGES FOR BLOODLESS OPERATIONS.

By SAMPSON GAMGEE, Esq., Surgeon to the Queen's Hospital, Birmingham.

Impressed by the results recorded by Professor Humphry of Cambridge, I resolved (29th Nov. ult.) to test the value of Esmarch's bandages for bloodless operations. The patient, a youth aged nineteen, was suffering from acute caries of the middle third of the left femur. Chloroform having been administered and the heel well raised, I bandaged tightly, with india-rubber bandages two inches wide, from the roots of the toes, over the heel, to a point two inches above the intended seat of operation. While an assistant held his finger on the uppermost turn of bandage, the limb immediately above it was very firmly encircled with several turns of india-rubber pipe, about the thickness of the index-finger. The ends of the pipe, which was two yards long, having been firmly tied together, the elastic bandage was uncoiled, leaving the limb shrunken and waxy pale. A three-inch incision in the middle line down to the femur, free use of the gouge and chisel in the removal of a quantity of carious bone, an incision into the boggy spot inside the knee, and the passage of a drainage-tube through the opening thus made from the incision over the middle third of the bone, were the successive steps of the absolutely bloodless operation. The scanty, pale, serous exudation was barely sufficient to impart a light-yellow stain to a white handkerchief. The elastic cord once freed and allowed to uncoil, pink suffusion of the previously dead-looking limb was the work of a few seconds. Blood now trickled from the wound, but certainly not more freely than usual after such an operation. Subsequent progress has been most satisfactory. The lad is easy and feeding, the femur is reduced to nearly normal size, the discharge lessening, and the wound rapidly granulating.

In addressing the clinical class immediately after the operation, it was impossible to conceal the enthusiastic admiration of the proceeding counselled by the German professor. Only the other day one of our leading surgeons spoke as if operative surgery had reached the utmost bounds of perfection. Esmarch's perfectly simple and efficient contrivance, which any surgeon can employ with success, renders possible the utmost economy of the vital powers, and scientific precision in the performance of many surgical operations. As the lad lay on the table in deep sleep, with the long drainage-tube stretching between the pale and spotless wounds through the substance of the thigh, it was impossible to suppress grateful admiration for the triple triumph of international surgery. This country and America divide the conspicuous honour inseparable from the discovery of anæsthesia. Chassaignac's system of drainage, to which justice has scarcely been done, is one of the greatest contributions, amongst the many, with which Frenchmen have enriched surgical art. Now, the Professor of Surgery at Kiel has introduced a method of procedure calculated to influence most beneficially, in the conjoined interest of science and humanity, a large domain of surgical practice.

The success attained in the foregoing case has led me to adopt the method in a case of excision of the knee, with the assistance of my friend and colleague, Mr. J. F. West. The proceeding was bloodless, though the hemorrhage was rather free when the ligature was removed from the middle of the thigh. The little arteries, which spirted out freely, were twisted or ligatured by our resident surgeon, Mr. Gilbert Smith, who also applied the usual apparatus to secure immobility. The case is proceeding (ten days after the operation) quite satisfactorily, with exceptional freedom from pain.

It often happens, in our profession as in other departments of human knowledge, though in varying degree, that the alluring charm of novelty seduces judgment, and soundness is sacrificed to fashion. I am always averse to magnifying the importance of new processes and remedies; but with the fullest reservation in favour of the appellate rights of experience, I look upon Professor Esmarch's process for bloodless operations as one likely to aid judicious surgeons in doing good.—*Lancet*, *Dec.* 20, 1873, *p.* 874.

45.—BLOODLESS SURGERY OF THE EXTREMITIES.

By J. E. Kelly, Esq., Surgeon to Jervis-street Hospital, Dublin.

Within the past fortnight I have applied Esmarch's method to four surgical cases. This coincidence induces me to detail some facts directly bearing upon the question of "Bloodless Surgery," together with a few points worthy of consideration.

Whether we concede to Esmarch the honour of an invention, or merely the adaptation of a rarely applied and undeveloped principle, is of little moment to those who derive most benefit from an expedient which we must regard as constituting one of the most striking advances in modern surgery. Simplicity and easy application are perhaps the strongest recommendations of a method, the novelty of which will be my excuse for briefly describing it. A strong elastic bandage is applied in the ordinary manner to the extremity, firmly and slowly; a piece of india-rubber tubing of the diameter of the thumb is wound very tightly round the limb immediately above the margin of the bandage, which is then removed. The preparations are complete, and the limb is perfectly bloodless until the tubing is removed. The facility of operating under these circumstances can hardly be realized. The structures have a semi-transparency resembling that resulting from preservation in turpentine or glycerine, and as may be expected, they are picked out and distinguished with remarkable facility. The operations upon which Esmarch's method confers the greatest advantage are those in which much blood is ordinarily lost, or those performed under circumstances which prevent the operator from providing efficient aid. Amongst the former, resection, and explorations, either for foreign bodies or where preliminary investigations are judicious, may be specially noticed. These preliminary measures, so conservative in their aim and results, have often been frustrated by the danger or dread of the consequent hemorrhage. Now we are enabled to pursue them bloodlessly. In provincial and in military practice a surgeon must frequently operate without skilled assistants or perhaps without any. Here again the advantages of the method are indisputable.

Against the obvious advantages of the expedient we must in justice weigh some well-founded objections. While advocating, and in some instances practising bloodless surgery, I regard it as being unnecessary in many cases, and in others as only to be used with great caution. In cases where collections of septic matter exist Esmarch warns us against the danger of favouring by pressure its entrance into the circulation. The absence of bleeding points during the operation may cause us to neglect to secure some vessels which will subsequently be troublesome. This occurred to a suggestive extent in the second of the accompanying cases, but the hemorrhage was quickly checked by the application of ice. As observed in the amputations performed in Guy's and other hospitals, the edges of the incisions may slough. I noticed this in my first case only, and my observations on it and the succeeding cases induce me to regard the sloughing as proportionate to, and dependent on the duration of the pressure by the elastic bandage, and not to the bloodless condition of the tissues. In the first of the series, being anxious to do full justice to the method, I applied the bandage in the ward before the removal of the patient to the operation theatre with the view of making the limb perfectly exsanguineous. When the patient was chloroformed the caoutchouc tubing was applied, and the bandage after being on for about ten minutes, was removed. The result was perfect; so bloodless were the parts that in my subsequent operations I allowed the bandage to remain on but for a few minutes, and in my last, immediately after the limb was entirely enveloped, I applied the tubing and removed the bandage. This operation was completely bloodless, and along the line of the incision not a trace of sloughing could be observed; in some parts even there was an effort at union by adhesion, but the condition of the deeper structures would not admit of such a rapid closure. I am inclined to regard sloughing as unimportant in these cases, which are really benefited by the method. In amputations and other operations, which can be completed in a few minutes, I cannot recognise the necessity of departing from the old and efficient method of controlling hemorrhage by *direct* arterial compression. Hence I question the importance of a contingency hich can be avoided. In military and provincial practice, where, as already :marked, the surgeon must amputate without sufficient skilled assistants, he is ee to select between the immediate danger of hemorrhage and the remote dislvantage of sloughing of the flaps; obervations are as yet deficient as to the conitutional effects of the expedient. The quantity of blood lost during operation frequently a most desirable antiphlogistic. By Esmarch's method, for good r for evil, this is prevented, and severe inflammation may follow the operation; owever, in such cases the surgeon can conveniently resort to phlebotomy, or ther antiphlogistic treatment, as suggested by his judgment. Again, in disise or predisposition to congestion in any of the important viscera, as the lungs r brain, we may well hesitate before we throw the entire amount of blood conined in a limb, either temporarily or permanently, into the general circulation. Ve must also remember the possibility that this quantity of blood may embariss the pulmonary circulation and aid in developing the fatal effects of the anæsietic. These are points which can only be settled by experience, and they apear to me to be worthy of the consideration of the profession :—

Case 1.—*Necrosis of the Entire Tibia.*—Thos.——, æt. 17, disease of eight ionths' duration. Commenced with violent pain about the tuberosity of the ibia. His legs became flexed, and an abscess formed which pointed and burst bout the tenth day. A series of these occurred along the front of the leg. Adiitted November 10th with symptoms indicating necrosis of the entire shaft of he bone. Operation on November 15th : Commenced by an incision reaching early from the knee to the ankle. The new osseous case was three-quarters f an inch thick and was opened with a chisel, at the expense of much labour, to he same extent as the primary incision. The bloodlessness of the operation iay be estimated by the fact that when the sequestra were removed the cavity id not contain a drop of blood. The case is progressing most favourably and he cavity is lined by healthy granulations. The edges of the incision sloughed n their entire extent, but only to a limited degree.

Case 2.—*Necrosis of Lower End of Tibia.*—Joseph——, æt. 15, disease of wo years' duration. Commenced by a pain in the ankle, and an abscess ormed above the articulation. The bone became expanded at its lower third, nd a succession of abscesses formed on the anterior and inner surface of the eg. Admitted October 29th, with symptoms of necrosis of the lower end f the tibia. After some general treatment the operation was performed on Vovember 13th. A portion of expanded bone was chiselled out, and a few mall sequestra were removed from the centre of the mass. This case also progresses favourably. Some smart hemorrhage occurred about two hours after peration, but it was easily suppressed by the application of ice.

Case 3.—*Necrosis of the Shaft of the Humerus.*—Margaret——, æt. 15, isease of eight months' duration. Gives the history of rheumatic fever, for which she received hospital treatment; abscesses formed at various points long the arm; admitted November 20th, and operation on the 25th November. A long incision on the outer side of the arm, and the new osseous case, which vas barely developed, was cleared of the sequestra. The special interest in this peration is owing to the application of the tubing above the arm, or round he clavicle and the neck of the scapula. Notwithstanding the protection which the vessels receive from the bones in the neighbourhood of the last tage of the subclavian artery, this operation was as bloodless as any of the preceding cases.

Case 4.—*Anchylosis of Elbow Joint; Resection.*—Anne——, æt. 23, disease of four years' duration, resulting from an injury to the joint. Admitted November 28th. Operation, sub-periosteal (November 29th), performed by means of an external incision. The osseous mass was cut through with a forceps, the end of the bone turned out and sawn off. The case progresses most favourably. The wound was dressed before the tubing was removed, and during, or after the operation, or at any time, a single drop of blood was not visible.—*Medical Press and Circular, Dec.* 17, 1873, *p.* 541.

46.—ON A CASE ILLUSTRATING ESMARCH'S METHOD OF PREVENTING LOSS OF BLOOD DURING SURGICAL OPERATIONS.

By Henry Arnott, Esq., Assistant Surgeon to St. Thomas's Hospital.

[The following is an abstract of a paper read before the Clinical Society of London.]

It happened to be probably the first operation of any magnitude in which this ingenious suggestion of Professor Esmarch was employed in England, although Mr. MacCormac had already used it in some cases of removal of necrosed bone; and the author therefore brought forward the case as a peg on which to hang a discussion on the merits of the proceeding by the hospital surgeons present. The case was an ordinary example of old disease of the knee, in which it was determined, on exploring the joint under chloroform, to excise it at once, the patient having refused to submit to amputation. The limb was tightly bandaged with elastic webbing from the toes to the thigh, and before the removal of the bandage, a stout elastic cord was twisted firmly round the limb above the bandage and fastened with hooks. The compression was kept up for more than thirty minutes, with the effect of rendering the operation absolutely bloodless until the elastic tourniquet was removed. Convalescence was slow, but presented nothing unusual which could be attributed to the pressure employed during the operation. The points dwelt upon by the author in commenting on the case, had reference to—1. The possibility of sloughing following the use of the constricting band, and the length of time during which the pressure might be safely maintained; 2. The effect on the circulation of the compressed limb which ensues on removal of the elastic cord; 3. The difficulty of using the method without chloroform; and 4. Under what circumstances the method might be wisely avoided. In briefly remarking on these heads, it was suggested that much of the pain and sudden engorgement of the vessels with blood might be prevented by gradually relaxing the constriction band; and, in enumerating the cases in which the employment of the method might be attended with danger, reference was made to instances of septic abscess, the clot-occluded veins about a compound fracture, and limbs already gangrenous. It was also suggested that a possible risk of apoplexy was incurred by suddenly overfilling the circulatory system of aged people with brittle arteries; and that, although generally beneficial in town practice, the visceral congestions caused by the subsequent plethora in cases of large amputations in robust subjects might seriously interfere with recovery.—Mr. Eastes remarked that, in seven cases of amputations under this method, recently reported in Guy's Hospital, the edges of the flaps subsequently sloughed in five; but, in all five cases the carbolic acid spray was also used. There was, therefore, not only a deprivation of blood, but also a considerable cooling of the tissues. Doubtless, the conjoined conditions are sufficiently powerful to produce sloughing, although the elastic bandage alone does not generally appear to be chargeable with such untoward result. In cases of necrosis, &c., where a precise view of the parts included in the operation is necessary, Esmarch's method is most useful, as the wound is absolutely bloodless.—Mr. Heath had for a long time practised a method by which the limbs may be deprived of blood. The limb being raised, the house-surgeon draws his hand gently along the surface, so as to force back the blood to the trunk. A tourniquet is next applied, and a card placed beneath the screw of the instrument. After the operation, the tourniquet is gradually loosened, when the blood returns slowly to the limb. One great advantage of this plan is, that every surgeon already possesses all that is requisite for its accomplishment.—Mr. Clover said that, with an ordinary bandage and band-tourniquet, bloodless operations may be performed, as he had years ago pointed out. It had been objected to all such procedures that, practically, a little too much blood might be forced into the general circulation; respecting Esmarch's method, especially, it should be remembered that the elastic cord is apt to break readily, unless it be new. He thought that a spiral bandage is more efficacious than the hand of an assistan in forcing blood out of a limb.—Mr. MacCormac noticed Mr. Heath's remark— that he had not seen Esmarch's method practised. In its simplicity and readiess of application, it is superior to all other plans for restraining hemorrhage; ven one drop of blood need not be lost. It facilitates and shortens operaons, as sponging is not required. The bandage had been partially adopted efore, but the method of Esmarch has now come into general use. Further xperience must be looked for, that we may be open to all the advantages and isadvantages of the procedure. Possibly, in certain cases, a moderate loss of lood may even be beneficial. The amount of pressure required to stop the rterial current in a limb is much less than is usually supposed. The sudden ntrance of blood to the limb after the operation is very agonising; a slow eturn, by gradual loosening of the band, is much better. In the operation or necrosis, the living bone seems dry, as well as the dead parts; why is this? The bandage cannot compress and force the blood out of the bone. It is well o apply the bandage about two minutes before the patient is quite chloroformed; and, at the time of beginning the operation, tie the cord which acts as a tourniquet around the upper part of the limb.—Mr. Barwell considered that, of the two methods of Clover and Esmarch, that of the latter empties the limb most thoroughly of blood. In a case in which he had adopted Esmarch's plan, he had, however, found, after the operation, a good deal of oozing for twenty-four hours, principally from the bone. In another case, a case of Syme's operation, the heel was left out of the constricting bandage; upon that part being cut into, the blood at once escaped; it had not diffused itself generally through the bloodless limb. The healing process had been rapid in Mr. Barwell's cases. He looked upon the bone as a kind of closed vessel with a small hole, from which blood cannot escape, unless fresh blood enters the limb.—Dr. Koch (of Munich) had seen some of Esmarch's own cases, viz., an excision of the knee, an operation for necrosis of the tibia, and a little operation upon the finger. The limb was dry, and the bone was dry in each case; and Esmarch thinks that the dryness of the bone is due to the fact, that the empty veins draw or suck the blood from the bone. Esmarch has never seen sloughing of the edges of the flaps, nor any bad results in his cases.—Mr. A. Willett said that nothing, since the introduction of chloroform, had added so much to the comfort of the operating surgeon as Esmarch's ingenious procedure, which lends a great charm to operations. The bone is always absolutely free from blood. After section of the bone in excision of the knee, and whilst the cord was still *in situ*, he had seen the cut surface of the bone become covered with a current-jelly kind of clot, one-third of an inch thick. It was, of course, removed before the bones were adjusted, but even then a little more blood escaped. In only one of many cases had he seen sloughing follow the operation, which was amputation of the leg in an elderly patient for an ulcer which had extended completely round the limb. The anterior flap sloughed, and the patient sank from pyæmia. After the application of the bandage, Mr. Willett had seen the Italian tourniquet applied; but he did not think it answered so well as an elastic tube or cord in restraining hemorrhage during the operation.—Mr. H. Morris had frequently known Mr. Hilton (of Guy's) order an ordinary bandage to be applied from the foot upwards half an hour before an operation upon the limb; the tourniquet being also then loosely placed over the femoral artery, and tightened just at the commencement of the operation, immediately before the removal of the bandage; but, by that method, never was such immunity from bleeding secured as by Esmarch's plan. He had sometimes, however, seen such considerable hemorrhage follow operations (Esmarch's), where the cord was quickly removed, that it did away with much of the benefit claimed for the procedure. He considered a combination of plans good; such as the elastic bandage with Pettit's tourniquet, or some other plan by which the blood can be gradually re-admitted to the limb. He could corroborate the fact that, in five of seven cases of amputation at Guy's Hospital, sloughing of the flaps had occurred, and that Lister's carbolic spray, which cools the tissues, had been used in all five cases. Mr. Morris had seen death occur in an old man four weeks after a Syme's operation under Esmarch's plan. There was never any attempt of union of the flaps. In old people, the driving of the blood from the limb is likely to prevent subsequent healing. He thought Mr. Arnott's case was one in which the procedure should not have been adopted; there was pus in the joint; the capsule might have been ruptured, and the pus forced into the blood-vessels.—Mr. **Barwell** described the different methods by which the cord might be fastened.—Mr. **Arnott** did not think the carbolic acid spray had anything to do with the subsequent sloughing of the edges of the wound; he doubted whether it cooled down the limb. He thought that one very important point is the *newness* of the cord; old vulcanised material becomes very brittle. Gentlemen in the country who could not frequently renew their apparatus should remember this fact. He considered that an elastic tube tied round the limb is the best method for the compression of the arteries; small pressure under this plan will more effectually stop the current of blood than a larger pressure from the old tourniquet.—*British Med. Journal, Dec.* 6, 1873, *p.* 664.

47.—THE ELASTIC LIGATURE.

By Professor Dittel, of Vienna.

[When the surgeon has to undertake operations in cavities and canals which are out of sight, or which are so narrow, that it is extremely difficult, if not impossible, to use cutting instruments within them, the adoption of a plan of operation which obviates all risk of hemorrhage is exceedingly desirable.]

In November, 1872, I was consulted by a rickety woman, who had her first child with her, five months old. It had, on its right temporal region, a roundish vascular growth, having a base from 2 to 2½ centimètres (about four-fifths of an inch to an inch) in diameter. I carried a strong insect-pin through the base, and also two others, one on each side—three pins in all being thus introduced through the tumour—and over these I twisted turns of waxed thread in the way described above. As usual, I had to correct the shape of many pins which had become bent, before the affair was in the state which was desired. After some days, the circular ligature had cut into the part, and lay, with the needles, imbedded in the suppurating furrow. I should now have drawn the ligature tighter, to make the falling-off of the tumour more sure. There then occurred to me the history of a girl, aged eleven, who had fallen a victim to the refined wickedness or the extreme carelessness of an unkind stepmother. This child, Marie Kramer, was admitted into hospital on March 5, 1872. She had an extremely neglected appearance. Her hair, dirty and full of vermin, was fastened in a net; her face was pale, and her look timid. She answered questions imperfectly and with hesitation, manifesting, evidently, that she felt herself in fear. Her statement that she had not removed her hair-net for about a fortnight, in consequence of the order of her mother, was not absolutely believed. As she complained of head-ache, the head was more carefully examined; and I found, in the part corresponding to the edge of the net, a suppurating furrow, at the bottom of which, after carefully washing away the purulent scabs, I discovered the fine elastic thread with which the hair-net had been fastened. This tread was visible in some parts; in others it was deeply imbedded and overgrown with granulations; and in some parts it lay deep in the corroded bones, especially the occipital and right parietal bones, where it had penetrated as far as the inner table. Although the furrow very soon granulated under ordinary treatment, symptoms of meningitis appeared, and she died on March 21.

The necropsy showed not only a high degree of general anæmia and meningitis, but also sloughing of the dura mater at the points where the loss of substance in the bone was greatest. The furrow in the soft parts corresponded with a furrow in the bone below the occipital tuberosity, reaching on each side over the tuberosities of the frontal bone, and thus forming a complete circular furrow in the skull. The furrow formed an almost complete chasm in the bones, so that the portion of skull lying above the furrow was connected with the bones below it only by means of remaining bridges, the whole length of which was ten or twelve centimètres—the circumference of the head at the part being forty-two centimètres.

From this act of base wickedness (for I found on inquiry, that the step-mother had not allowed the child to loosen the hair-net), I learned that an elastic cord is excellent for use in the division of tissues. And now, instead of tightening the thread in the case of my little patient above-mentioned, I applied a caoutchouc drainage-tube all round the pins. Eight days later the mother brought in the child, which had borne this ligature much more easily than the thread. The vascular growth had fallen off; and in its place was a healthy granulating surface, the circumference of which was already beginning to be covered with a cicatricial membrane. This result led me to make further researches with the drainage-tubes; and I have since then used them in the treatment of nævus, fistula ani, prolapsus ani, sinuses, cancer of the breast, and in the ligature of arteries.

The proceeding is easy, but it requires a certain care and precision, which may soon be acquired by patience and attention. The operator must avoid giving unnecessary pain through pulling the cord too tight and dragging on the part, by having the part to be tied supported or held up by an assistant. The ligature must, of course, be drawn tight, and tied with two knots. The pain produced by the tying is altogether not great, in many cases very trifling, and scarcely ever lasts more than an hour. It is self-evident that the depth of the furrow produced depends on the degree to which the cord is tightened, and on the resistance of the tissues. It is probable that the ligature may be applied less tightly than I have done from fear of failure, as the pressure is continuous until the elastic cord has regained its former length. The division of the part is effected by the continuous pressure of the cord, which compresses the vessels and lymphatics until the vessels are plugged, and the access of nutritive material is completely interrupted. The substance of India-rubber has the peculiarity of not producing suppuration; and hence, while the compressed parts disappear, a granulating furrow is left, and after the part has fallen off, a healthy luxuriant granulating surface remains. As the tied part falls off, the ligature springs away from the granulations. The ligature then forms a closed ring, the aperture of which is so narrow that a probe or needle can scarcely enter it. The process of ligature or division lasts from three to fifteen days, according to the thickness of the pedicle and the density of the tissues; in a case of cancer of the breast, the duration of treatment was fifteen days. I have never detected febrile symptoms, even when the surface was extensive and the pedicle broad.

The mode of proceeding I have varied according to the peculiarities of the task to be performed.

1. In nævus, after transfixing it with needles as for ordinary ligature, I have the tumour fixed by an assistant during the tying. A single circular ligature is sufficient. In the two cases in which I used this treatment, the nævus fell off in eight days.

2. In fistula ani or sinuses about the rectum, the elastic ligature has especial advantages where the inner opening of the fistula lies high up or the sinus extends far. In the case of sinus, an inner opening (into the rectum) is first made by means of a trocar. The trocar being withdrawn, the elastic thread is introduced through the cannula and drawn out through the rectum. This proceeding is rendered more easy by first introducing a metallic thread into the rectum through the cannula, seizing it with the finger or forceps, drawing it through the anus, and removing the cannula. The outer end of the wire is now fastened to the small elastic tubing by means of a waxed thread. In this way the elastic cord is very easily drawn through, if the index finger of the left hand can be passed up the rectum as far as the opening. Both ends of the ligature are drawn upon, and tied rather tight. The bridge of intestine with the sphincter is generally cut through in three or four days, and the patient finds the ligature, contracted into a ring, lying in his bed. In complete fistula ani, the metal wire is carried into the intestine along the groove of a director, and the use of the trocar is unnecessary.

3. In prolapsus ani, the protruding fold is seized with hooks or polypus-forceps, as for the application of the ordinary ligature, and drawn down a little. In order that the fold may not escape from the forceps while the ligature is being applied, an assistant must press against the fold after it is seized, a

second must stretch the cord, and a third must fix between his fingers a small portion of the cord corresponding to the fold, so that it remains only for the operator to tie the ligature. The ligature falls off in three or four days.

4. In sinuses I have used the elastic ligature many times; and will briefly relate the following case. Herr Sch., residing in Odeonstrasse, aged twenty-two, very anæmic, had been suffering twelve weeks from inflammation of the left inguinal glands, following typhus. On Dec. 5, 1872, in consultation with his ordinary doctor, Dr. Hamburger, I opened an abscess; and on Dec. 7, as the glands were greatly swollen and suppuration had extended down to their lower border, I repeated the operation. There remained a sinus passing below the glands for about two inches. On December 25 I introduced through the sinus a ligature which included the superjacent skin and all the glands. On the second day, the portion of the bridge of skin that was left was only two lines long, the swelling of the glands was reduced, and healthy granulations were present. The fever disappeared; the ligature fell off on the sixth day. The patient completely recovered.

In a second operation in which I applied the elastic ligature, the bridge of skin was four inches long. The result was equally favourable.

5. In tumours, the elastic ligature has its application when they are pedunculated, or when they can be isolated, or when it is not possible or necessary to save the skin. It is thus inapplicable in diffuse infiltration. I have had only one opportunity of applying the elastic ligature to a tumour. The patient was Frau H., aged seventy-four, and at her age neither I nor another surgeon would have undertaken the risk of a bloody operation. But, having already ascertained that the elastic ligature did not produce any fever, and as the old lady earnestly desired that the tumour—a fibrous cancer of the right breast—should be removed, I concluded to make this first attempt. The cancer had at its base a vertical diameter of about four inches, and a transverse diameter of five inches, and was movable. I passed a Fleurand's trocar through, beneath the tumour, in the direction of the vertical diameter; and, having withdrawn the trocar I introduced through the cannula two waxed threads, and by means of them drew through two pieces of small India-rubber tubing; the cannula was then withdrawn, leaving the tubes. I now, while an assistant drew forward the tumour, embraced each half in the corresponding portion of tube, and tied the ends of the tubing firmly in a double knot. This was done in the out-patient department on January 9, 1873. On the second day I had the patient taken into ward No. 81, as she was rather anxious on account of some pain. The furrow procured by the ligature became deeper daily before our eyes, leaving a granulating surface, while the tumour correspondingly collapsed, sloughed, and fell off on the tenth day. During the whole time the patient had no fever, only an increase of thirst on the eighth day. The old lady was right well pleased with the result.

6. I have applied the elastic ligature to arteries in the following cases.

a. To the popliteal artery, on the occasion of amputating the left thigh after Gritti's method. The ligature was thrown off on the sixteenth day.

b. To the anterior tibial artery in two cases of amputation of the left leg, the ligature falling off on the seventh day; and once in a Pirogoff's amputation.

c. To small branches of the anterior and posterior tibial and peroneal arteries. I have thus ascertained, that by means of the elastic ligature the flow of blood from the divided arteries may be completely arrested and their final closure perfectly attained; and further, that this ligature does not provoke suppuration. But with regard to the question whether it may be retained and encapsuled without mischief, so as to favour healing by the first intention, I have made no experiments. In the cases in which I applied it, the wounds were not closed, except in the Pirogoff's operation, where the ligature was soon thrown off.

The mode of applying the ligature to arteries requires some improvement. I have so far modified it, that the elastic thread is first applied over the end of the catch-forceps, and, when it is tied, is pushed from the instrument by an assistant, with the nail of his forefinger. The ligature then springs over the artery that is held; but sometimes it misses at first. It is probable that this ligature does not require to be tied very tight, but only just so much as is sufcient to compress the lumen of the artery, so as to allow the formation of a lug, and its organisation and definite union with the walls of the vessel, so as o completely close the artery. That the ligature remained sixteen days in the ase of Gritti's operation, must be ascribed to the fact that I connected two ligtures one with the other, so that they held on like the links of a chain. It ras interesting to see the two mouths of the ends of the tube projecting from he granulations, without any trace of pus around them.

I will, in conclusion, answer by anticipation the question which will doubtess be raised. Why devise a new ligature, it will be said, when we already ossess other means of strangulation, such as the écraseur, the constrictor, the latinum wire, and ligatures of various materials?

The écraseur and constrictor act indeed more quickly, but they produce nuch pain; they leave a contused wound, which, under all the chances which ttend the process, must throw off the dead parts and granulate, to say nothing f the roughness of the proceeding, repulsive both to surgeon and to patient.

The platinum wire requires, in the first place, the possession of an expensive galvano-caustic apparatus, which is therefore beyond the reach of the ordinary practitioner; and the apparatus is so complicated, that not unfrequently, in consequence of a portion being out of order, even those who are skilled in its use are placed in difficulty. Moreover, the platinum wire does not secure against hemorrhage, and leaves a wound covered with an eschar, which is only replaced by a clean granulating wound after a rather long process of separation.

The ordinary threads have scarcely one advantage over the elastic ligature, but many disadvantages. Thread is not elastic, and thus their pressure is not constant. It acts on tissues just as deeply as it has been at first drawn together, and perhaps somewhat deeper if indirect pressure be entertained. When the first layers have been divided, the ligature becomes loose, and the tying must be repeated, at least in many cases, as in extensive sinuses and fistula ni. Thus, pain is again produced, with some bleeding from the furrow, and, erhaps, an unexpectedly rapid tying of the remainder of the tumour to avoid e danger of hemorrhage. Besides this, pus is poured out in the furrow and und the threads, and encrusts the edges, so that it becomes difficult to keep the rt clean and to remove the ligature.

The elastic ligature, on the other hand, when once applied, acts gently and ntinuously by its elasticity, until the tissue that has been strangulated falls , and leaves no suppuration. The shortness of the time in which the operan is completed also shows a balance in favour of the elastic ligature, and it not much more expensive than good thread. I have no doubt that the use the ligature will be found capable of extension to other cases, such as castran and the removal of lymphatic glands.

On account of the tendency of India-rubber to favour granulation, I have lateapplied it to chronic ulcers on the feet, and to other wound-surfaces. I cover ese with pieces of caoutchouc, which I press down gently by means of calicondages, thus at the same time excluding the air. I may say that my own servations on each case, and the patients themselves, agree that healing vances more rapidly. We generally see a small ring about one or two millières broad, which at its periphery exhibits daily a new transformation into atricial tissue. This observation is still far from being complete; it will be e subject of a future critical communication by my assistant, Dr. Mayerhofer. *London Medical Record*, *Dec.* 3, 1873, *p.* 755.

.—REMARKS ON THE METHOD OF REMOVING GROWTHS, &c., BY THE ELASTIC LIGATURE.

y Sir Henry Thompson, Surgeon Extraordinary to the King of the Belgians, and Surgeon to University College Hospital.

The following brief paper contains Sir Henry Thompson's experience upon the se of the elastic ligature in the removal of a tumour of the breast. The whole of the right breast was removed, and the patient made a satisfactory recovery.)

The progress of the case was unsatisfactory at first, owing to two circumstances. The first was an attack of erysipelas, prolonged rather than severe, which appeared two or three days after the application of the ligature, and which affected the right arm, shoulder, and most of the back.

At the end of eight or ten days the ligature was evidently not embracing the still undivided portion so firmly as it ought to have done. I therefore made it tighter, which was very easily done by pulling out the loop from the wound as far as possible without much hurting the patient, and tying round it a fine ligature, so as to diminish, perhaps by one half, the encircling noose. Certainly the mass was larger than I at first suspected it to be; and it is not to be overlooked that the whole breast was of course extirpated.

The cord on one side gave way about the second day; I therefore did not re-apply it until after the other half of the breast had been divided; and on this occasion the process was more rapid, and no more erysipelas appeared as the result of the fresh pressure from the ligature on the skin. The whole mass was removed about ten or twelve days since, and nothing can be better than the appearance of the wound now, which began to diminish rapidly after the tumour came away.

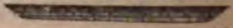
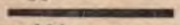
I am satisfied that the fine india-rubber tube which I used, and which was the only material then attainable, was not sufficiently strong. I have therefore had some *solid* cord manufactured for me which is much more powerful, and which I believe will divide the tissues in at least one-half the time which was occupied in this case by the tube. Its superiority is very manifest, for, being much stronger, it may be drawn at least twice as tight as the tube, and the consequence is that not only is the tension on the tumour greater, but the cord itself by the same process becomes finer, and therefore cuts more rapidly. In fact, there is no comparison between the two.

A small quantity of this cord could not be made, as no such product is required for any other purpose, and I have, therefore, deposited the whole with Messrs. Weiss and Son, from whom it can be obtained.

I also think it must be better, as Prof. Dittel now does, to apply the ligature to one-half of the breast at a time, the lower side, and not to use the second for the upper-half until the first has separated. This was what actually did occur in my case, only the upper ligature, having remained some thirty-six hours or so before giving way, strangled the whole mass, and produced a far more offensive result than would have occurred had only one been at first employed.

No doubt the principal objection to the plan is the smell which necessarily arises from the sphacelated portion. This was much controlled by constant irrigation with carbolic acid and water, from a bottle placed above the level of the patient's head; a small current flowing through a tube, the lower extremity of which rested on the breast as the patient lay in bed, the part being isolated by means of a macintosh cloth, and the solution running into a receptable below. All this was admirably arranged and attended to by the house-surgeon, Mr Buckston Browne.

No doubt a breast is a rather severe test for the ligature, although I do not hesitate to anticipate a very much better result with a more powerful cord. For the removal of the testicle, and for division of fistula in ano, I think it will be found admirable.

The cord itself ought to be more accurately described. Its size before use is the following: 
When applied it should be strained until it is a mere thread—say like this: 

It would be very easy to devise a simple apparatus to tighten it subsequently, but it is so easy to accomplish this by pulling it out if loose, and tying a bit of fine ligature round the portion so drawn out, that it seems unnecessary to employ any other means. But with the new cord it is very probable that no such readjustment will be required.—*Lancet, Jan.* 3, 1874, *p.* 3.

.—CASE OF FATTY TUMOUR OF THE FOREARM REMOVED BY THE BLOODLESS OPERATION.

Under the care of Mr. Cowell, at the Westminster Hospital.

The subject of this operation was a girl thirteen years of age. She presented elastic tumour situated over the flexor muscles of the left forearm. It was al in shape, and measured five inches in length by two inches and a half in idth. An ordinary bandage, which Mr. Cowell seems to prefer to an elastic e, was applied to the arm in the ordinary way, and the solid elastic cord rendered the operation absolutely bloodless. The ordinary bandage does not mpty the tissues so completely as the elastic, and the limb does not appear so lanched. The incision was made near and parallel to the median vein, which ccupied a position over the long diameter of the tumour, and was more or ess isolated during the rolling back of the skin. Mr. Cowell remarked that his bloodless method was a great convenience to the operator, as it made him ndependent of sponges during the process, but he questioned whether the ounds generally heal so quickly. He considered it necessary not to be in too reat a hurry to close the wound after this method, but to wait a few minutes ntil the engorged condition of the surface and edges of the wound had subsided efore applying the sutures, a matter which becomes of more importance after he larger operations, such as excision of the knee. In an instance of the later operation, the application of cold water, which contained a small proportion f carbolic acid, for five minutes was most satisfactory, and the wound rapidly ealed.

In this case the sutures were applied at once, and either from the separation f the surfaces of the wound by oozing, or in consequence of the partial isotion of the median vein, the wound did not heal by first intention. The ound granulated in twenty days, when the patient was discharged cured.—*ancet*, *Dec.* 27, 1873, *p.* 899.

.—REMOVAL OF EPITHELIOMA OF THE LOWER LIP BY THE ELASTIC LIGATURE.

By Dr. H. Stanley Gale, Manchester.

The patient was a gentleman aged sixty-four, who said that "four years ago mall wart appeared on his lower lip, which he scratched; this caused a sore, hich gradually spread until very nearly the whole of the lower lip was affect." There was little doubt as to the nature of the complaint, although a rgeon whom he had consulted lately, salivated him and kept him under the uence of mercury for several weeks. He then came to me, and the first ing he said was, "Mind, no knife work for me." On examination I found implication of any glands, while the patient's general health was good, and spirits high. He consented at once to the ligature. I transfixed the lip in e centre, at a spot which was quite clear of the disease, with a narrow knife; d passing through the hole thus made two pieces of india-rubber cord about e-sixth of an inch thick, had no difficulty in tying a knot at each extremity the lip. The whole of the diseased surface was thus enclosed in much the me way as piles are tied after transfixion. The result was most successful. two or three days the smell became troublesome, and I found powdered rcoal the best remedy on the whole, as it kept the parts more dry, and nce less disagreeable, than most other deodorants. On the thirteenth day e-half was detached, and on the fourteenth the second half. The wound left s a mere line, and presented a healthy-looking surface. In six days more e patient was well and able to eat his meals in the usual manner, while before e operation he could only take liquids through a tube and solids with the reatest difficulty. The patient stated that the pain was but little if any greater han that arising from the disease, which at times had been most acute.

There cannot be a doubt that this mode of operating will be of the greatest service, and must come into general use in those cases suited to its employment. During the whole time I kept him under the influence of morphia, by hypodermic injections of one-eighth of a grain night and morning. I believe this will enable many to bear the mode of operating by ligature who otherwise could not endure the pain.—*Lancet, Jan.* 17, 1874, *p.* 90.

51.—ON THE VALUE OF THE GALVANIC CAUTERY AS A CAUSTIC.

By THOMAS BRYANT, Esq., Surgeon to Guy's Hospital.

In the cases I am now about to relate, the value of the galvanic cautery as a caustic will be illustrated; and I think I shall be able to satisfy you that, as such, for the treatment of lupus, epithelial or skin cancers, and some forms of nævi, it possesses advantages such as are not equalled by any other means the surgeon has at his command. It is applied with facility, certainty, and success; and it has rendered the treatment of these hitherto troublesome affections comparatively a simple matter; for in many of the cases I am now about to relate, one application of the galvano-caustic was sufficient to set up a new action in the part, and heal a lupus, destroy a cancer, or cure a nævus.

Cases of Lupus cured by the Galvanic Cautery.—Case 1.—*Lupus of face of four years' standing; cautery; cure.*—Caroline E., aged twelve, came under my care on Jan. 4th, 1872, with a large patch of lupus covering the left cheek and extending from the left ear to the nose and chin. It had commenced four years previously as a tubercle. The tubercle at that time was cut out, but the wound never healed; indeed, it ulcerated, the ulceration spreading gradually in spite of all treatment. On Jan. 5th, with the patient under chloroform, I applied the galvanic cautery to the whole surface of the sore, and covered the parts up with a piece of gutta-percha and cotton wool. A healthy action followed this operation, and a steady cure, the patient leaving the hospital, well, on Jan. 29th.

Case 2.—*Lupus of face of four years' standing: cautery; cure.* (Reported by Mr. Manby.)—Eliza T., aged thirty, came under Mr. Bryant's care in November 1868, with a lupus covering the whole nose and upper lip. It had been gradually spreading for four years in spite of treatment. On November 6th Mr. Bryant cauterised the whole surface freely with the porcelain cautery. In thirteen days the scab fell off, leaving a healthy, healing surface. By Dec. 3d, or the twenty-seventh day after the operation, the part had completely healed.

Case 3.—*Lupus of twenty years' standing cured by the application of the galvanic cautery.*—Mary W., aged thirty-eight, came to me on April 22nd, 1868, with a patch of lupus, the size of a florin, on her nose. It had existed for twenty years, and no kind of treatment had been of any service. I destroyed the whole mass at one sitting with the porcelain cautery, and charred it into a hard leathery eschar. On May 12th, when I saw her again, the surface had nearly healed.

Case 4.—*Lupus on the nose and cheek; galvanic cautery; cure.*—Mrs. H., aged sixty-five, of Fairford, Gloucester, consulted me in July, 1873, for a lupus involving the nose and cheek. It had been of some years' standing, and had spread in spite of treatment. On July 9th I thoroughly destroyed the growth by the galvanic cautery, and when the eschar came away a healthy surface appeared, which rapidly healed. Her medical attendant, Mr. Cornwall, of Fairford, reported to me in November that she was quite well.

Cases of Epithelial Cancer cured by the Cautery.—Case 1.—*Epithelial cancer of the ala of the nose; galvanic cautery; cure.*—Honora S., aged fifty, came under my care in February, 1868, with an epithelial cancer, of the size of a sixpence, on the ala of the nose. I destroyed it completely by one free application of the galvanic cautery, and a good sound cicatrix was the result—

Case 2.—*Cancer of nose of four years' standing ; galvanic cautery ; cure.*—Mary——, aged sixty-four, came under my care on Jan. 8th, 1871, with a cancer nvolving the ala of her nose. It had commenced four years before, and, in spite of treatment, had spread. I applied the cautery to its surface and destroyed it, a rapid recovery taking place.

Case 3.—*Epithelial cancer of the nose of four years' duration ; galvanic cau-ery ; recovery.* (Reported by Mr. Ticehurst.)—Sarah B., aged fifty-three, who had had an epithelial cancer on her nose for five years and every form of reatment applied, came to Mr. Bryant in October, 1868. The growth was near he right ala, and was about the size of a sixpence. He freely destroyed it by means of the galvanic cautery; on the third day the dry eschar fell off, and he wound healed in a week. Nine months afterwards some recurrence on the pot took place, which was treated in the same way and with a like result.

Case 4.—*Epithelial cancer of the nose and eyelids ; galvanic cautery ; cure.*—On Feb. 26th, 1868, Mrs. P., aged fifty-five, applied to me with an epithelial ancer that involved the bridge of the nose and the inner angle of the eye. It had been growing for a year and a half. I destroyed it completely with the galvanic cautery, and a good cure ensued.

In this case on account of the position of the cancer, it would have been difficult to destroy the disease with safety in any other way.

Case 5.—*Epithelial cancer of the scrotum (sweep's) ; removal by cautery ; recovery.*—George H., a sweep, aged fifty-eight, was admitted into Guy's Hospital, under Mr. Bryant's care, on July 8th, 1868, with a large cancer of the scrotum, the size of a crown. On Oct. 30th, Mr. Bryant, with the patient under the influence of chloroform, destroyed the whole with the galvanic cautery. No bleeding attended the operation; and after the operation the man said he was in less pain than he had been before. On the seventh day the eschar came away, and the wound rapidly healed.

Case 6.—*Epithelial cancer of the scrotum (sweep's) ; destruction with the cautery ; cure.*—James H., aged thirty-five, a sweep, came under my care in April, 1868, with an epithelial cancer, the size of a crown-piece, upon the scrotum. I destroyed it completely with the galvanic cautery, and when the eschar came way a healthy wound existed, except at one spot. This was touched again y the cautery on April 22nd, and by April 29th, the parts had healed.

Case 7.—*Fungating sore over malar bone of seven years' standing ; cure after the application of the galvanic cautery.*—William H., aged sixteen, came under my care in March, 1868, with a peculiar fungating sore over the left malar bone of seven years' standing, which had followed the bite of a horse. It was about he size of a shilling, and had irregular thickened edges. I destroyed it completely with the galvanic cautery on March 12th, and by April 1st the wound had healed. On April 20th a soft pliable cicatrix existed.

On the treatment of Nævi by the Galvanic Cautery.—That nævi may be destroyed and cured by means of the galvanic cautery must be such a familiar fact to you, gentlemen, who have followed my practice at this hospital, as to make it appear quite unnecessary for me to bring the subject before you on the present occasion, and so far as some of you are concerned you are right; but as it is through you that I want others to know the value of the practice I am now advocating, I propose to point out the forms of nævi which are likely to be benefited by the treatment, and the different ways in which it is to be applied; and I shall do this as briefly as possible.

With regard to the forms of nævi that are likely to be benefited by the practice, I would remind you that nævi are met with in three varieties: (1) the purely skin nævus, that involves the skin alone, and appears as a more or less extensive and bright vascular mark; (2) the purely subcutaneous nævus, that involves the cellular tissue placed beneath the skin, and that does not affect he integument over it; and (3) the mixed form of nævus, which affects both ssues, beginning either in the skin and spreading deeply, or in the cellular ssue and growing forwards. Of these three varieties the first and the last re the more amenable to the treatment by the galvanic cautery; the first ith a good certainty of securing a successful issue, and the last with a fair rtainty. The purely subcutaneous may be so dealt with, but in a general way these cases are better treated by excision or by subcutaneous ligature.

In the treatment of the purely skin nævus the surgeon must take care that the whole thickness of the vascular tissue is destroyed. He must burn it down by means of the platinum point, spatula, or porcelain cautery, till the whole is destroyed and carbonised. He must see that the margins of the growth are freely cauterised as well as its centre; the base of the disease as well as its surface. He must not show any timidity in the use of the burning instrument; he must use it boldly, to the complete destruction of the vascular structure. He must so cauterise the whole nævus as to turn it into a brown, non-vascular, solid eschar. Where failure has followed the use of the cautery in these cases it has been from want of attention to these points.

In the treatment of the mixed variety of nævus the same care is called for, and the same boldness of execution; but something additional is required, and that is the perforation of the subcutaneous portion of the nævus with the heated wire. The whole mass should be riddled in all directions: the platinum wire should be introduced into it vertically, transversely or obliquely; every part of its structure should be brought under the influence of the cautery, and under such circumstances the withering of the whole may with some confidence be looked for. In all the cases in which I have adopted this practice good success has been secured; when failure has followed it has been from a want of due attention to its details. The success, indeed, has been so good that in the purely subcutaneous variety of nævus I am disposed to think an equal amount of success is to be gained by an equal attention to the application of the cautery to the deeper parts. The success I have hitherto acquired by following this practice has encouraged me to give this opinion. The following cases will, however, best illustrate the subject:

Cases of Nævi cured by means of the Galvanic Cautery.—Case 1. Wm. G., aged ten weeks. Nævus the size of a sixpence involving the skin over the right frontal eminence. On March 5th, 1869, the growth was cauterised by means of the porcelain galvanic cautery. In ten days the slough came away, and a white cicatrix was left, no signs of the disease being visible.

Case 2.—Charles J. E., aged twelve months. Nævus involving the tip of the nose, alæ, and septum, with part of the upper lip. It had been vaccinated without any good effect. On Nov. 7th, 1865, Mr. Bryant cauterised the whole with the porcelain cautery. On the 15th the eschar fell off, and by the 30th the parts were well, no signs of nævus being visible.

Case 3.—Mary G., aged five months. Nævus the size of a shilling at the inner angle of the left eye, steadily growing. It had been vaccinated three times with no good effect. On October 30th, 1868, the whole surface was deeply cauterised, and burned into a dry eschar. On Nov. 12th the scab was still adherent. 26th: The scab fell off three days since; cicatrix healthy; no sign of disease to be seen.

Case 4.—Robert C., aged sixteen months. A *mixed* nævus, the size of a sixpence, over the right forehead. On Nov. 13th, 1868, it was cauterised freely, and then punctured in all directions. On Nov. 21st the scab came off, leaving a healthy scar; and by the 26th the child was cured.

Case 5.—Agnes W., aged four months. A nævus, the size of a florin, over the right side of the forehead. It was freely cauterised on Oct. 30th, 1868. On Nov. 5th, the scab was adherent, and on the 16th it came off, leaving a healthy scar. On the 26th the child was quite well. On Jan. 13th, 1869, a white movable cicatrix existed.

Case 6.—Alice C., aged four months. A *mixed* nævus, the size of a crown piece, on the side, growing rapidly. On Jan. 8th, 1869, the whole was deeply cauterised and perforated in all ways. The scab came off on the 18th, and on the 20th the parts had nearly healed. On the 28th a healthy scar existed. A month later the scar was white, and no signs of disease could be made out.

Case 7.—William S., aged five months. A nævus, the size of a shilling, on the nape of the neck, of the *mixed* variety. On Nov. 6th Mr. Bryant punctured *the swelling* in all directions with the heated galvanic cautery wire. The *growth* on the sixth day had become quite solid from inflammatory induration.

n one month it had disappeared. On Feb. 1st, 1869, beyond the scars of the unctures, no signs of the disease could be made out.

Case 8.—Emily M., aged thirteen months. A *subcutaneous* nævus, the size of alf a walnut, in the right cheek. It had been ligatured subcutaneously, with- ut any good result. On Oct. 30th, 1868, Mr. Bryant punctured the swelling reely from within the mouth with the wire of the galvanic cautery. On Nov. 2th it had become perfectly consolidated, and in six weeks the whole disease ad disappeared. In February, 1869, no evidence of the affection was visible.

Case 9.—Mary H., aged three months. A *mixed* nævus, two inches and a alf long by an inch wide, under the chin. On Oct. 30th, 1868, the whole was reely cauterised with the porcelain galvanic cautery, and then punctured in ll directions. On Nov. 12th the nævus was quite hard and solid; and on the 9th the scab came off, leaving a healthy scar, no signs of the disease being isible. On the 5th of March a suspicious spot the size of half a pea was seen nd destroyed, a good recovery taking place.

Case 10.—James M., aged one year. A *mixed* nævus, the size of a shilling, n the centre of the forehead. On Jan. 8th, 1869, the growth was perforated n all directions by the heated wire of the galvanic cautery; it subsequently onsolidated and then gradually disappeared, the child leaving London, cured, n six weeks.

Case 11.—Richard H., aged five months. A nævus of the *mixed* kind on the nterior border of the axilla. On the 16th of April, 1869, Mr. Bryant removed t by means of the galvanic écraseur, having passed beneath its base two hare- ip pins. The operation was bloodless. A rapid recovery followed.

Case 12.—Mary D., aged twenty-one months. A *mixed* nævus, the size of half a walnut, over the frontal eminence. On February 26th, 1868, destruction of the whole was effected with the galvanic cautery. On the tenth day the scab came off, leaving a sound cicatrix.

I have selected these cases to bring before you, because they were some of the earliest I had to treat, and also because notes of them were taken by one of my then dressers. They may be read as types of the very many I have dealt with since, and of which I have not kept notes. As I described to you in my former remarks, you will have observed that in the simple nævus the mark was cauterized from its surface; in the mixed variety it was cauterised nd perforated; in the subcutaneous it was perforated alone; and in one case he mixed nævus was removed as a whole with the écraseur. In all of these ases the operation was almost bloodless from first to last.

In some cases of nævi it is not necessary to destroy the whole. I have had veral in which one of the eyelids was involved down to its ciliary border. In ese I have been content with destroying half the growth, leaving the ciliary lf alone; and I have been pleased to find that the ciliary portion of the dis- se subsequently disappeared, the redness gradually going, and leaving the in white and pliable.

The same result has followed a like practice in a case in which one ear and eek were involved, the destruction of part of the growth being followed by e withering of the whole. The most active part of the growth in all these ses was selected for destruction.

In extensive nævi, with these facts before us, it is consequently wise to deal th them piecemeal—to destroy a spot the size of a shilling at different parts the growth; to cauterise the surface of the nævus when the skin is involv- , and the deeper part by perforating it when the cellular tissue is affected; d in some cases it is well to make a clean sweep of the heated wire through e base of the growth;—the object under all these circumstances being the me—to destroy the vascular tissue, and excite sufficient inflammatory action its deeper parts to cause what remains to wither. The larger and more ex- asive nævi may be thus treated with every probability of acquiring a suc- ssful result.

The cicatrices which follow the cure of these nævi by means of the cautery e singularly good. They are mostly white, and, as a rule, movable—more rticularly when the nævus is thoroughly destroyed in one operation, and the schar is allowed to dry up and drop off, the deeper parts being allowed to eal by scabbing. Why this should be I will not detain you to consider, but he clinical fact is a *good one to* bear in mind.—*Lancet*, April 4, 1874, p. 469.

52.—MATTHEWS BROTHERS' NEW "SEPTIC" CAUTERY.

The want of a small and effective lamp for heating the actual cautery is one which has long been felt. In operations such as Mr. Henry Smith's for the removal of hemorrhoids it has often been a cause of much inconvenience to heat the cautery by the ordinary bedroom fire, the bars of the grate being in many instances too high to allow the point of the instrument being inserted in the midst of a clear fire whilst the handle rests on the hearth; the alternative being to thrust the iron into the fire with the handle protruding horizontally, in which case it becomes heated in the stem as well as in the button, causing the handle to be dangerously "warm" to the operator. It may also happen that the fire in the patient's room is not a bright one, the result being that the coal adheres to the iron, from which it is with difficulty removed.

The ingenious arrangement invented for Mr. Henry Smith by Messrs. Matthews, of Portugal-street, entirely removes all these inconveniences. The Septic Lamp is on the principle of the Russian blow-pipe, by which a little methylated spirit is boiled in an upper reservoir furnished with a safety valve, and the spray of spirit vapour ignited by a little flame beneath the upper reservoir, which may be easily regulated and promptly extinguished. The jet of flame is protected by copper sides, on which the iron or irons are placed, and made hot at the button only in "one or two minutes."

The whole affair folds in a very compact little japanned box, which is furnished with a tin compartment to hold a quantity of spirit; and the whole box (which may be made to carry the cauteries also) measures on the outside 8½ in. by 3½ in.

We are extremely pleased with this ingenious arrangement; and have no doubt that in India and warm climates, where a fire is not readily obtainable, the instrument will be extensively used, as it will doubtless be here, owing to its simplicity, handiness, and moderate price.

The specimen lamp shown to us was fitted in a little bag, which contained, besides, Mr. Henry Smith's set of clamps, cauteries, &c., for hemorrhoids.—*Lancet*, *Dec.* 27, 1873, *p.* 907.

53.—ON TALIPES VALGUS.

By LAMBERT H. ORMSBY, Esq., Surgeon to the Meath Hospital, Dublin, and Demonstrator in the Royal College of Surgeons in Ireland.

The external character of this deformity may be said to be an exact contrast to varus. We find it occurring as a congenital and non-congenital affection; the congenital form, however, I may mention, is very rare, the acquired form being by far the most frequent, coming on from a varied set of causes. It goes by the common name of flat or splay-foot, as we find a certain amount of flattening of the foot in this form of distortion. In short, valgus consists in the following: The foot is very considerably everted, heel drawn up, the toes elevated, and the weight of the body is supported upon the inner margin of the foot, which is in a complete case abducted, flexed and rotated outwards; we also find the longitudinal and transverse arches of the foot nearly obliterated, and in severe cases completely so, which, as a matter of course, produces that flattened appearance which is very apparent in non-congenital cases.

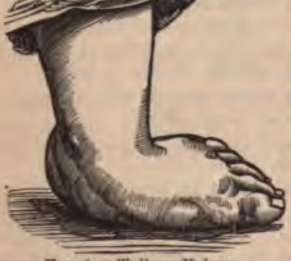


FIG. 1.—*Talipes Valgus*.

There are two modifications of this distortion, described in orthopædic works on the subject; as compound varieties, which I may mention are due—1st, either to contraction or shortening of the tendo achillis (which, when I say tendo achillis, I mean the muscles forming that tendon), producing the variety called *talipes equino-valgus*; or 2nd, to elongation of the tendo achillis, producing the variety *talipes calcaneo-valgus*.

The above subdivision, in my opinion, is very unimportant, and needs only a passing notice, as the tendo achillis is more or less engaged in every case and generally requires to be divided.

Valgus is generally produced by traumatic injuries, and, as I said before, is rarely congenital.

Mr. Adams in his work on club-foot gives a very useful table which determines very fairly the degree of frequency this deformity occurs.

Table of 764 cases of congenital club-foot showing the relative frequency of congenital deformities as follows :—

Talipes varus	688
" valgus	42
" valgus of one foot and varus of the other	15
" calcaneous	19
Total congenital cases	764

In the same table mentioned by Mr. Adams we find the cases of talipes valgus distributed as follows:

Affecting the right foot only	15
" the left foot only	10
" both feet	17
	42

We also find two different species of deformity co-existing at the same time —viz., in one foot, say the left, we may have valgus, and in the right we may have varus, and *vice versâ*.

Causes of this Deformity.—As I mentioned before, it may be either congenital or non-congenital, and, as is natural to suppose, the causes giving rise to it in both cases will be slightly different :—

1st. Co-existence of other deformities of the hip or knee.

2nd. Deficient development of the tibia and fibula.

3rd. Rickets.

4th. May co-exist with spinal curvature.

5th. Relaxation of the plantar ligaments. Weak and delicate growing girls often suffer from a slight degree of it.

6th. Tonic contraction of the peronei muscles, together with the extensor digitorum communis; in fact, in nearly every case there is more or less shortening of these muscles and elongation or relaxation of the muscles passing to the inside of the foot.

7th. Diseases of the foot—scrofulous, affecting the ankle-joint.

8th. Traumatic causes—the contraction after the healing of a wound or a severe burn.

9th. May be produced by walking bare-footed, common among the lower classes, such as flat foot.

10th. Caused by paralysis of the muscles.

Treatment divided into (1) the mechanical, and (2) operative, or (3) a combination of both.

Treatment by Tenotomy.—The tendons requiring division of course depends on the severity of the case; in slight cases we find division of the peroneus longus and brevis sufficient; in others, in addition, we have to divide the tendo achillis and extensor longus digitorum.

To Divide the Peroneus Longus.—This is easily done by placing the patient on the opposite side, and grasping the foot in one hand and making the tendon prominent. As it passes at the outside of the foot to get into the groove on the cuboid bone, insert the point of the tenotome flat, underneath the tendon, just below the bend of the ankle, then turn and cut from within outwards. When the tendon is cut through it is easily ascertained, as nothing arrests or resists the onward progress of the knife. A small pledget of lint on the puncture, a metallic splint bent to fit the deformity, a bandage applied, and the operation is completed.

To Divide the Tendons of the Extensor Digitorum Communis and Peroneus Tertius Muscles.—Place the patient on his back or opposite side, grasp the foot in such a manner as to render the tendons prominent, insert the tenotome about the centre of the ankle in front of the anterior annular ligament to the nner side of the extensor tendon, and having got the point of the knife well behind, cut from below upwards towards the skin until you feel the tendon completely divided. If you follow out these directions there is no fear of the anterior tibial, or, more properly, the dorsalis pedis artery being wounded. Then let the same pledget of lint and metallic splint be applied and kept in the same deformed position for seven or eight days, in order to allow a lymph-like organisable substance to be poured out between the divided ends of the tendons.

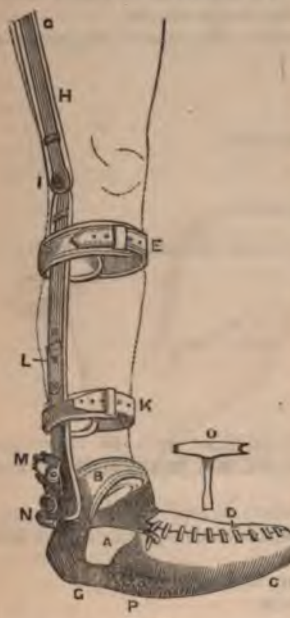


FIG. 2.

After these operative measures have been practised, I am then in the habit of finishin up with some mechanical apparatus for th permanent cure of the case. I may mentio that the instrument is generally more suitab the simpler it is in its construction, as in many cases patients like to return home at the end of the operative treatment with the intention of coming to see you occasionally to see how the case is going on, and if the instrument is very complex, the object of the appliance will in many cases, be misunderstood, and perhaps put on wrong, or applied incorrectly, and the very object you are trying to see carried out will be entirely defeated, for which reason, I say, let the instrument be simple, and apply it first yourself, and show the friends of the patient the use and utility of the appliance suggested, and then no blame, if the case ever relapse, can possibly be attached to you or your instrument.

"Scarpa's shoe" often is used for the after-treatment. Although this instrument was suggested for varus, it can with a slight modification be very easily and advantageously applied to valgus. A Scarpa's shoe made for a right foot suffering from varus will do a valgus very well of the left foot, and *vice versâ*.

In all cases of severe valgus I would suggest that the treatment be divided into two stages, as in varus, viz.: 1st, convert the talipes valgus into the equino-valgus position; and 2ndly, when you have got so far, divide the tendo achillis as in varus, and bring the foot in a short time into normal position.

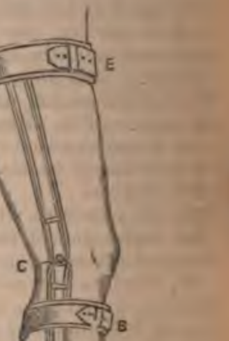


FIG. 3.

Stromeyer's shoe is also a useful appliance, and it can be used for varus or valgus by just altering the screw and pad in the footboard.

A very inexpensive and simple apparatus I am in the habit of using for cases of the kind, particularly in hospital practice—viz., an instrument precisely the same as I finish up in cases of *varus;* it consists in a boot laced up from the toes above the ankle, and a light set of irons attached to this boot, passing up at each side of the leg, and attached to the under surface of the heel, a joint in the irons corresponding to the ankle and knee, and strapped and buckled at the ankle, centre of leg, and above the knee, very much after the form of Fig. 3. This appliance I order the patient's friends to keep applied for three months, or even longer, after the tendons have been divided. In slight cases, where there is no operation required, and a slight tendency to flat foot, I order a boot with an arched sole, to induce, as it were, the plantar fascia to assume artificially the arched appearance, which, after some time, becomes permanent. In the slightest grade of valgus strips of adhesive plaster may be used with advantage, as also the gutta-percha plan, as recommended under the treatment of talipes varus. Paraffin bandages are very much used by some surgeons, also Mr. Barwell's plan, with vulcanised india-rubber bands and adhesive plaster, as also mentioned when considering the treatment of varus. Any instrument, however applied, must be worn day and night, as the permanent cure greatly depends on this point. Charriere gives two very useful appliances in his Catalogue (Figs. 2 and 3), which are very well adapted for talipes valgus and talipes equinus; they are provided with cogwheels, and a key, and ratchet-joints, and can be turned to suit nearly any distortion of the foot.

Talipes Calcaneus.—This form of club-foot is of rather rare occurrence, and not often met with; it consists, however, in a depression of the os calcis and lengthening or elongation of the tendo achillis, with contraction of the anterior muscles of the leg, viz., extensor longus digitorum, tibialis anticus, directly and extensor proprius pollicis; the plantar fascia is considerably contracted, rendering the sole of the foot concave; in some cases the person affected walks on the heel, and in advanced cases the toes are drawn directly upwards, and do not touch the ground, as in Fig. 4. This deformity is met with both as a congenital and non-congenital affection, and as the appearances and external characters of each form differ considerably, a separate description of the two varieties might be judicious.—*Medical Press and Circular, Dec.* 3, 1873, *p.* 493.

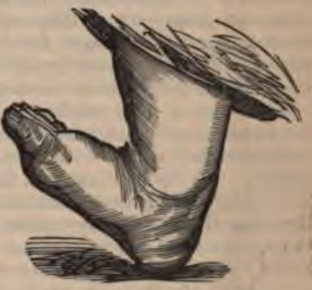


FIG. 4.—*Talipes Calcaneus.*

54.—ON HOSPITALISM, AND THE CAUSES WHICH DETERMINE THE MORTALITY AFTER AMPUTATIONS.

By JOHN ERIC ERICHSEN, Esq., Senior Surgeon to University College Hospital, and Holme Professor of Clinical Surgery in University College, London.

[There is no doubt, whatever, that the septic poison, which is the cause of hospital gangrene, septicæmia, pyæmia, and erysipelas, when once generated, is capable of transmission through the medium of the atmosphere. That it may be generated in the air by overcrowding is equally certain.]

It would, in fact, appear that the air of a ward is capable of oxidising and destroying or absorbing a certain amount of morbid emanations from the contained patients; but if these emanations are developed too rapidly or too abundantly the air becomes overcharged with septic matters, and then all the ill-effects of overcrowding at once develop themselves. The contamination of the air of a ward may thus take place even though the actual number of patients lying in it be below what it is constructed to hold, as readily as by the introduction of one single patient beyond the number that the ward is calculated to accommodate with safety. Thus it would be more correct to say that the evil effects of overcrowding, and the special form of disease it will occasion, are rather dependent on the nature than on the actual number of the cases that are contained in any given ward or building at one time. There is no evidence that an accumulation of unwounded patients to any extent can develop hospital gangrene or pyæmia, whatever other disease may thus be generated. Pyæmia could not have been generated in the Black Hole of Calcutta; but a very trifling excess of the number of open suppurating wounds beyond what any given ward is capable of holding with safety constitutes overcrowding in a surgical sense, and will invariably generate septic disease.

It is the nature of the cases, and not their number merely, that constitutes the danger of the development of surgical septic disease by so-called overcrowding. Doubtless epidemic influences favour the development of these septic outbreaks that are directly produced by overcrowding. In what these epidemic influences consist I know not. So far as my experience and observation go they are connected with very different meteorological conditions. In wet and in dry weather, in heat and cold, during prevalent easterly and westerly winds, they equally manifest themselves. The existence of these epidemic conditions, in addition to the influence of overcrowding, is evidenced by the admission from out of doors of erysipelatous cases and allied diseases, or by their appearance amongst the out-patients in unusual numbers. But the influence of different meteorological conditions on the development of epidemic septic diseases has yet to be worked out.

Now what is this condition of contamination in hospital air that causes the development of septic disease in a ward, the cubic and floor space of which are sufficient, and which is kept scrupulously clean? That is the next point for inquiry. In the well-known, I may say celebrated, lecture by Prof. Tyndall on "Dust and Disease," that distinguished philosopher points out the important fact that the atmosphere under all circumstances contains floating particles of organic matter; that the motes which dance in the sunbeam are of this character. He states that the air of our London rooms is loaded with this organic dust, nor is the country air free from the pollution; and he goes on to observe that, however much it may disguise itself from ordinary observation, "a powerful beam of electric light causes the air in which the dust is suspended to appear as a semi-solid rather than as a gas. Nobody could in the first instance, without repugnance, place the mouth at the illuminated focus of the electric beam and inhale the dirt revealed there. Nor is the disgust abolished by the reflection, that although we do not see the nastiness, we are churning it in our lungs every hour and minute of our lives. There is no respite to this contact with dirt, and the wonder is, not that we should from time to time suffer from its presence, but that so small a portion of it would appear to be deadly to man." Watts states that a minute quantity of organic matter, one grain in 200,000, is found in the present mountain air. This increases in the country at a lower level, and in large towns reaches the condition stated by Professor Tyndall in the above extract. Even in sea air, according to Dr. Rattray, in the last volume of the Medical-Chirurgical Transactions, it is not absent. This organic matter is the result, in all cases, of vegetable and animal emanations, and it necessarily varies in its composition according to the source from which it is derived. Rattray, in a most interesting paper in the volume referred to, gives an analysis of ship air, and after pointing out a variety of impurities of a gaseous character, states that the more solid impurities are of animal, vegetable, and mineral origin, from the skin, lungs, &c., of the crew; from the ship, as minute particles of wood, paint, whitewash, leather, &c.; from the bilge, containing occasionally minute microscopical animal and vegetable organisms; from the stores, particles of bread, cotton, wool, &c; and others which I need not mention. He states that it is the volatile organic matters thrown off by the skin which give the ship air its well-known and often nauseous smell. Dr. Parkes, in his admirable work on practical hygiene, gives the following account of hospital air. At page 88, he says: "I have examined the air of various barracks and military hospitals, and have detected large quantities of epithelium from the skin, and, perhaps, the mouth." At page 99, he says: "The most important class of diseases produced by impurities in the atmosphere is certainly caused by the presence of organic matters floating in the air, since under this heading come all the specific diseases. The exact condition of this organic matter is unknown; whether it is in the form of impalpable particles, or of moist or dry epithelium and pus-cells, is a point for further inquiry." And again, at page 106, he says: "The air of a sick-ward, containing as it does an immense quantity of organic matter, is well-known to be most injurious. At a certain point of impurity erysipelas and hospital gangrene appear. The occurrence of either disease is, in fact, a condemnation of the sanitary condition of the ward." Again, at page 100, he states that "erysipelas and hospital gangrene in surgical wards are often carried by dirty sponges, dressings, &c. Another mode of transference is by the passage into the atmosphere of disintegrated pus-cells and putrefying organic particles, and hence the great effect of free ventilation in military ophthalmia, in erysipelas, and hospital gangrene." Lastly Dr. Douglas Cunningham, of Calcutta, has recently found that the atmospheric dust largely consists of spores of fungi, and that the majority of these are living and capable of growth and development; and that bacterial matter exists also in dry dust, so that when this is added to putrescible fluids a rapid development of fungi and bacteria occurs. Here then we find, on the combined authority of able and recent observers, abundant evidence not only of the existence in the atmosphere of large quantities of suspended organic matters, but of animal *débris* and exfoliations, and of other animal particles, capable under favourable circumstances of generating septic diseases.

We are still in complete ignorance as to the precise nature of the septic poison that produces those various forms of disease that we recognize as being due to its influence. We know it, in fact, by its effects, but are ignorant of what it essentially consists. That there are different forms of septic disease appears more than probable. We find that the different varieties of septic diseases are not interchangeable, but are as distinct in themselves, in the symptoms they present, in the course they run, in the pathological conditions that are found, as any of the other zymotic diseases. In whatever way originating, and in whatever it may essentially consist, there can be little doubt that this septic virus is communicable from patient to patient through the medium of the organic particles of various kinds with which the atmosphere of any crowded building, be it hospital, or barrack, or man-of-war, is invariably charged. And when we reflect on the exceedingly minute, infinitesimal, in fact inappreciable, quantity of any animal virus, as that of cow-pox, small-pox, or syphilis, that is needed to communicate its own disease when applied to a fitting soil, we can easily understand how the virus of the septic diseases that occur in surgical wards may be transmitted from wound to wound, even on so slight a vehicle as atmospheric dust.

There are four of these septic diseases universally recognised by surgeons—viz., hospital gangrene, septicæmia, pyæmia, and erysipelas. Let us very briefly study the mode of development of them.

With regard to hospital gangrene I need say but little. It is universally recognised by the concurrent testimony of military surgeons that this disease originates in the first instance as a direct consequence of overcrowding of the wounded in hospitals that are insufficiently ventilated. The experience of the Franco-German war added confirmation, if any were needed, to this view of the origin of the pestilence, which, however, when once generated, is capable of indefinite propagation by contact through the medium of the fingers, instruments, sponges, and surgical appliances. The occurrence of hospital gangrene in civil hospitals is now, fortunately, extremely rare, and its development in such institutions is of itself an evidence that the sanitary condition of the building is, for the time at least, in a bad state. The repeated occurrence of hospital gangrene in a civil hospital would undoubtedly be discreditable to those who had the management of its sanitary arrangements. In University College Hospital we have now had no outbreak of hospital gangrene for more than twenty years, and I trust never again to see it here as having been developed within the walls of the hospital. It is altogether a preventible disease, and ought never to occur in any institution that is conducted on sanitary principles. The few cases that we have had of late years have been brought into the hospital from without, and, curiously enough, last summer two cases came to us, of a mild form certainly, from a convalescent institution.

Septicæmia is not of very common occurrence, but yet there is a certain percentage of deaths occurring from it. In this hospital we have about one death in the year from this cause. Its influence on the general mortality after operations is therefore but trifling, but its existence is an indication of a septic influence, and I believe that a certain number of cases of low, irritative fevers following operations and injuries partake of a septicæmic type. In fact, the gradation from ordinary traumatic fever into septicæmia is easy, and I believe not very unfrequent. Surgeons themselves suffer at times from small whiffs of this poison. Who that has been long connected with a hospital has not at times, after the examination of a sloughing cancer, or some other horribly putrescent and foul disease, felt feverish, depressed, prostrated for some hours, or a day or two, conscious of the absorption of a poisonous effluvium, which after a period of febrile depression eliminates itself from the system by an attack of offensive diarrhœa, the eruption of a pustule, or perhaps by some more distinctly localised inflammatory action, as tonsillitis, or a boil? The cause of septicæmia appears, however, to me to be somewhat obscure. It does not seem to be distinctly connected with overcrowding, but rather with the development of putrescent discharges from unhealthy or malignant ulcers. The offensive discharge from ulcerated cancer uteri is supposed by some to tend very specially to its production, and it has been a cause of death after ovariotomy when the operation has been practised in the same building in which a woman suffering from that disease is lying.

But the two most important of the septic diseases are undoubtedly pyæmia and erysipelas. These two diseases are commonly looked upon as being more or less allied, and they may be so in so far as their etiology and those epidemic influences that predispose to their occurrence are concerned, but in all other respects they differ widely.

Acute pyæmia is essentially a hospital disease. As has already been stated in the last lecture, it seldom occurs except in hospital practice; and perhaps the best proof that we have of the rarity of the occurrence of acute pyæmia out of hospital exists in the rarity of the admission of a pyæmic patient into a hospital from without whilst labouring under the disease. Almost every case of pyæmia that I have seen in hospital practice has originated within the building itself. By reference to Table B it will be seen that the amount of pyæmia varies very greatly in different London hospitals, which is an additional proof of its being dependent for its origin on conditions that are more rife in some institutions than in others. So far as University College Hospital is concerned, there appears to be at present a tolerably uniform amount of it. In three years, from July, 1870, to July, 1873, we had twenty-three cases: nine in connexion with amputations, fourteen with other operations and injuries. Of these twenty-three cases, four occurred in the latter half of 1870, eight in 1871, eight in 1872, and three in the first half of 1873. As to the mode of its development, there can be no doubt that it is the result of the exposure of wounds to an atmosphere overcharged with organic matter emanating from other patients with suppurating wounds. What I stated in the last lecture about the infection of the military hospitals with pyæmia during the Franco-German war, and the comparative freedom of the hut hospitals from this disease, points clearly to the cause. I look upon pyæmia, when of traumatic origin, as being primarily the local sceptic impregnation of a wound by atmospheric matter in a condition capable of developing changes of such a character in the wound that its fluids decompose, its surface becomes foul and sloughing, and the veins leading from it become plugged with soft clots, putrid and easily decomposable. The constitutional symptoms of this dread disease—the prostrating rigors, the profuse transudations from skin and lung saturating the bedclothes and contaminating the air around with a faint, sickly odour, the high temperature, the extreme mental depression—are all consequent on the entry into the circulation of the septic virus deposited from air upon wounds, thence absorbed into the veins, and thence transmitted through the system.

Pyæmia stands next to hospital gangrene among septic diseases of local *origin*. It is less marked in its local phenomena; it is far more developed in *its constitutional* symptoms, but though less marked locally it is most distinctly characterised. Mr. Beck, who has made the post-mortem examinations of the twenty-one fatal cases of pyæmia that have occurred in University College Hospital during the past three years with a degree of minuteness and care that leave nothing unnoted, states that in every case except one—a case of necrosis of the tibia, in which there was no open wound until a few hours before death—distinct local evidences were found. In fourteen cases the local origin of the disease was evidenced by broken-down and putrid clots in the veins leading from the part, and in six there were foul sloughing wounds. It is a remarkable fact that acute pyæmia never appears to occur in single isolated cases, but invariably in groups of two or three, not necessarily absolutely contemporaneous, but only separated by short intervals of time.

Is pyæmia contagious? The French surgeons generally believe that it is highly so. I have never seen an unequivocal instance of it. This, however, may be accounted for by the fact that whenever a case of pyæmia has developed itself in this hospital, immediate steps have been taken to guard against contagion by isolating the patient and disinfecting the ward. We act, in fact, as if its contagion were proved, although we may not in reality be in possession of this proof. There certainly appears to be an epidemic influence that favours its development. It is often coincident, though not invariably so, with outbreaks of erysipelas both in the hospital itself and out of doors. I have already said that it is necessary for the surgeon to consider the nature rather than the number of the cases contained within a surgical ward. This is well exemplified by an outbreak of pyæmia that took place in Ward 1 in January, 1871. In the early part of the month this ward contained a rather large number of severe wounds, including a sloughing cancer in the groin, an amputation of the leg, a compound fracture of the fibula with extensive laceration of the soft parts, a compound fracture of the femur, and one of the tibia. Pyæmia occurred in the case of amputation of the leg on the 19th of January, in the compound fracture of the fibula on the third of February, in the compound fracture of the femur on the 13th, and all these patients died. At no time, however, was the ward overcrowded as to the number of patients, two out of fourteen beds being empty the whole time, so that each patient had a cubic space of no less than 1800 feet of air. But the ward contained for two months an average of seven or eight open wounds, of which five were always severe, such as amputations or compound fractures. During this period there was only one case of erysipelas, and the hospital generally was free from that disease. Another instance of a similar kind occurred in December, 1872. At this period Ward 1 became again crowded with severe wounds, there being an average of nine open wounds, seven or eight of which were severe, including three amputations, and an extensive necrosis of the tibia. One of the amputation cases had pyæmia and recovered, the only instance of recovery from pyæmia during the last three years and a half. Another had pyæmia and died. On this occasion erysipelas broke out in three cases. In both these outbreaks the weather was cold.

Now in erysipelas we find various influences tend to develop the disease. Contagion, overcrowding, and epidemic influences all produce their effect. About the contagion of erysipelas there can be no question. I could adduce many instances of it, but one especially occurs to me. Though it happened many years ago, it made a deep and lasting impression upon my mind. I will read an extract from the notes I took at the time. On the 17th of January, 1851, a case of plegmonous erysipelas of the leg was accidentally brought into Ward 1. As soon as the nature of the disease was discovered the patient was removed to the erysipelas ward at the top of the building, having only remained in Ward 1 for about two hours. At this time Ward 1 was perfectly healthy, but in a few days after, a patient lying in the next bed to that into which the erysipelatous patient had been taken, and who had been operated on for necrosis of the ilium, was seized with erysipelas. On the 22nd I performed five operations on patients who were in this ward. Of these cases three were attacked by erysipelas on the 24th—viz., a case of necrosis of the tibia, one of partial amputation of the foot, and one of encysted tumour of the scalp. All these patients died. On the 24th a patient was operated on by me for strangulated femoral hernia in this ward. He was seized with symptoms of low peritonitis, doubtless of an erysipelatous character, and died on the 31st. No case of pyæmia showed itself, but all the patients in the ward who were not attacked by erysipelas had diarrhœa and severe gastro-intestinal irritation. Here were a series of lamentable catastrophes doubtless directly introduced by the accidental intrusion of an erysipelatous patient into an operation ward, the disease being then propagated by contagion, though it only remained for two hours in the ward. That erysipelas, however, may be developed in other ways there can be little question. I believe that by no means an uncommon cause of infection occurs from dissecting students. I have several times seen cases, both in hospital and private practice, distinctly referable to contamination from this cause, and I think that no student, during the time that he is engaged in dissections, should be allowed to act as a dresser in a hospital. There is a remarkable resemblance, if not an actual similarity, between puerperal peritonitis and erysipelatous inflammation of the peritoneum following operations on the organs contained in that cavity. The influence of contamination in the way I mention is well illustrated by what occurred some years ago at the Vienna Hospital, and is described by Dr. Braidwood in his work on Pyæmia. Prior to 1846 the number of deaths amongst lying-in women in this hospital was 1 in 10. In order to check this frightful mortality, Dr. Semelweiss required the students who attended these women to avoid making autopsies, and obliged them to wash their hands in a solution of chlorine before and after making vaginal examinations. The result was that the mortality in 1848 had fallen to 1 in 74.

That erysipelas is often of epidemic origin, there can be no question, but the influence of any epidemic is immensely increased by unhealthy conditions of a ward from overcrowding. Of this I will give you one illustration. In the middle of November, 1872, ward No. 1 was filled by a very severe set of cases. There was one patient who had had both legs amputated below the knees; another whose leg had been amputated through the knee; another who had a compound fracture in the ankle-joint; one with extensive necrosis of the tibia; and another with extensive laceration of the forearm. And this condition of things was kept up during three primary operations—one of the foot and the others of the thigh; these cases being admitted in December, but whilst the preceding ones were under treatment. There was no erysipelas in the ward until Nov. 15th. On that day the patient with compound fracture into the ankle was attacked. He recovered, and the erysipelas had left him by the 21st. On this day the patient with necrosis of the tibia had the sequestrum removed: he was a feeble old man; he was attacked by erysipelas on the 1st, was removed to the erysipelas ward, and died on the 7th of December. On the 4th of December the patient with the scalp wound was attacked, and was at once removed to the erysipelatous ward. During this period—namely, Dec. 7th and the 22nd—two of the other patients, the one with an amputation through the knee-joint and another, were seized with pyæmia. Only one out of the twenty-three patients with acute traumatic pyæmia recovered. During the same period two of my patients in Ward 5, both suffering from chronic wounds, one of fistula and another with a sinus in the thigh, were attacked by erysipelas. To them it is possible that the disease was conveyed by the house-surgeon or dressers. But as other cases of erysipelas occurred in the hospital at the same time, and one was admitted from out of doors, it is probable that an epidemic influence existed.—*Lancet, Jan.* 31, 1874, *p.* 151.

55.—ON THE USE OF RESIN-CLOTH, IN THE PLACE OF CERE-CLOTH, IN THE TREATMENT OF WOUNDS.

By Edward Lund, Esq., Surgeon to the Manchester Royal Infirmary.

[The carbolised gauze employed by Professor Lister is expensive, which is a matter of importance in hospital practice. There is, moreover, a difficulty in its *preparation.*]

In the paper in which he first described this dressing in the British Medical Journal (January 1871, p. 30), Professor Lister refers to a very curious property possessed by carbolic acid—namely, that it can be combined with resin, and resinous matters generally, with great facility; and, when so blended, nearly all its irritating acrid properties are neutralised, while the compound so formed retains the power of evolving an antiseptic vapour at the temperature of the body. It is well known that to touch the mucous membrane, or even the skin of the lips, with a fragment of pure carbolic acid, occasions pain and excoriation, in some subjects more than in others; but when we experiment in the same way, on a mixture of the acid with five parts of resin, we find all this acridity destroyed; the acid is still present, but it is stored up and rendered harmless by this new combination. Having verified this fact by numerous observations, I came to the conclusion that we have here a method of forming easily, without the aid of heat, an application for antiseptic purposes in surgical practice of great value. All that is required to carry out the idea is to saturate very thin calico gauze with such a mixture of resin and acid, dissolved in methylated spirit, press it powerfully, spread it out to dry quickly, so that it may become quite porous and absorbent, and it is ready for use. Yet the resin and acid, thus left upon the threads of the calico after the complete evaporation of the spirit, would be too brittle and adhesive to the skin for a wound-cover and air-filter if something else were not added to impart flexibility. The substance which I employ for this purpose is castor-oil, because it is the only fixed oil easily accessible, which is quite miscible with spirits of wine. All samples of castor-oil, by reason of adulteration, do not show this property of perfect solubility in alcohol, as stated by Pereira (Materia Medica and Therapeutics, 1865, p.251); and, therefore, I am content to use the oil, if it will unite perfectly with twice its bulk of rectified spirit. The exact composition of the solution is as follows:—Carbolic acid crystals, melted, two fluid-ounces; castor-oil, two fluid-ounces; purified resin, sixteen ounces by weight; methylated spirit, forty fluid-ounces: mix. To dissolve these ingredients easily, we must add them together in a certain order. To the resin, liquefied by heat and removed from the fire, add one-third part of the spirit; when these are well mixed, put in another third of the spirit, in which the oil has been previously dissolved; and, lastly, the acid in the remaining portion of the spirit must be slowly added to complete the mixture. The whole mass must be agitated until all the constituents are thoroughly incorporated, and afterwards passed through a muslin filter to get rid of any extraneous matters. If this plan be not adopted, the resin will concrete into a mass at the bottom of the vessel, and it will be extremely difficult afterwards to get it perfectly mixed. When thus prepared, the solution is of a dark colour, clear and free from any deposit; and it can be kept unchanged, in a well corked bottle, for a long time. It is, in fact, a kind of thick varnish. To make the resin-cloth, as I term it, for the sake of distinguishing it from the cere-cloth dressing for wounds, which I described in a paper read at the Leeds meeting of the British Medical Association in 1869, it is needful to select a very thin, cheap, porous calico, or calico-muslin, known in the trade as "mulls," which costs at wholesale price about four shillings per piece of twenty yards. This, divided into strips, each about nine inches wide and six yards long, is reduced to a convenient form for general use. The calico should be unbleached and free from stiffening, and each of the strips should be carefully folded up, so as just to lie flatly in the press, as I am about to explain.

An ordinary square tincture press may be used to press the cloth, or such a press as is sold for copying letters, to which a tin box has been adapted, so that the plates of the press can work in it; and into this box the folded calico is placed, the solution being poured over each successive layer, so as to wet perfectly every part of it. There should be an aperture at the bottom of the box, with a tap by which the superfluous fluid can be removed, collected, and used a second time. The press being brought into action, the pile of calico should be squeezed as dry as possible, all the fluid drained off, the resin-cloth taken out, laid over a few lines of string in a warm room with a good ventilation; and in an hour or two, when all traces of smell of methylated spirit have departed, the cloth may be rolled up and kept in tin cases ready for use.

It is difficult for me to state the exact cost of resin-cloth made by this process, for I have not yet bought the materials for its preparation at such prices as could be had if it were to be manufactured in large quantities; but, allowing for some slight reduction, where six or eight twenty-yard pieces of calico are bought at a time, and the solution made by the gallon, I find it comes to a fraction less than threepence per yard of average width of 44 inches. In using it as a dressing for wounds, I deal with it precisely as I would do with Mr. Lister's antiseptic gauze, for which it must be taken as a cheap and ready substitute. I generally apply ten folds of it over the face of a wound, as in an amputation, and perhaps six folds higher up the limb for some distance, and I cover it with the macintosh hat-lining, so as to distribute the serous discharges through the breadth of the resin-cloth thus covered. I have never found it to irritate the skin in any degree beyond what the oiled silk (protective), liberally used, could control, except once, when in the hurrying of preparing the resin-cloth, I had neglected to dry it thoroughly, and it was applied, still moist with the methylated spirit, the naphtha in it seeming to be the chief cause of the skin-irritation. But if this precaution be observed, I believe this resin-cloth will be found a very useful addendum to our means of treating wounds and abscesses on antiseptic principles. —*Brit. Med. Journal, Dec.* 6, 1873, *p.* 654.

56.—ON A NEW FORM OF FIXED BANDAGE.

By W. W. Wagstaffe, Esq., Assistant Surgeon and Lecturer on Anatomy at St. Thomas's Hospital.

In the course of last year my attention was drawn, by Mr. West's address upon French Surgery delivered at the Midland Medical Society, to the silica bandages used in the Paris hospitals in place of gypsum and starch, and, being then Resident Assistant-Surgeon at St. Thomas's Hospital, and able to find frequent opportunities for testing the value of new appliances, I was desirous of trying this. However, I found that no such preparation of silica for bandages had been brought into use in England, and upon inquiring of Mr. West himself, found he had been unable to succeed with the preparations obtained in England. The French directions, too, for its preparation were so useless to a surgeon that I despaired of being able to find anything at all satisfactory. However, I was fortunate enough to obtain, through my friend Mr. A. H. Smee, a small quantity of colloidal silicate of soda, and with this succeeded in making an exceedingly firm bandage. This bandage was put on over the knee in a case of knee-joint disease in July, 1873, and was removed only in November, after four months' wear, and at the end of that time it was perfectly strong, clean, and sound. This bandage was a single length arranged in three successive layers, each being covered with the silicate; and the whole quantity of the silicate used was four ounces.

The silicates of potash and soda, dissolved in an excess of caustic alkali, are now prepared in large quantity in the manufacture of soap, and can be easily obtained at a very moderate cost. Messrs. Hopkins and Williams, of 16, Cross Street, Hatton Garden, have supplied it in large quantities at 4d. a pound, and in small quantities at 6d. a pound, and it can be obtained, though inferior in character, at 2d. and 3d.,—so that it is not an expensive material. In fact, it is about half or a quarter the price of the thick gum solution used for similar bandages, and being firmer, does not require to be used in much more than half the quantity.

The plan which I have adopted has been to encase the joint or fractured limb in cotton wool, lint, or a thin flannel roller; then over this to apply a common bandage, dry; over this to paint the silica by means of a brush or sponge; and to repeat the bandage and silica, so that two or three layers of each exist. As soon as the last layer is dry, another coating of silicate is to be put on, so as to give an even surface.

The limb is to be left exposed to the air for about half an hour, but there is no fear of any of the silicate coming off after the first few minutes, and after half an hour or less (varying with the temperature) the bandage is firm enough to prevent movement. However, the bandage continues to harden for about two or three days, at the end of which time it should be quite firm; but it is usually firm enough in a few hours to insure immobility of a limb. It is sometimes advisable, as where great strength is required, to coat the splint after a day or so with fresh silicate; but where such strength is wished for, it is better to adopt a modification to which I shall refer directly. The fixed bandage, as now completed, does not contract in drying, and does not produce the inconvenience which is often seen with gum bandages—that of contracting at the margins and cutting into the skin. It forms a fixed splint of great lightness, cleanliness, and strength, and of less expense than gum. With children I generally take the precaution of varnishing over the surface with spirit varnish after a couple of days, so as to keep the bandage from the wet to which it may be exposed. The solubility of the silicate in water is of advantage, for it renders the removal of the bandage easy. Without taking the precaution of moistening it, I have seen a strong pair of scissors broken in removing one of these silica bandages from the knee.

The advantages, then, which the silica bandage possesses over the gum bandage, are—(*a*) much greater rapidity in drying, (*b*) greater cleanliness, (*c*) probably greater strength, and (*d*) greater cheapness. Its advantages as compared with the starch bandage are (*a*) its greater strength, (*b*) its greater rapidity in drying, (*c*) its greater cheapness, and (*d*) its freedom from offensiveness after a time. Compared with plaster of Paris, it is (*a*) lighter, (*b*) cleaner, but it does not set so quickly or afford quite so firm a support at first.

There is a modification to which I have alluded above as securing greater strength in this bandage. Mr. W. C. Elliott, of this Hospital, has suggested the mixing of whitening with the silicate. As much whitening is mixed with the silicate as to make a fluid of the consistence of batter. This is used in the same way as the pure silicate, and the results obtained have proved very satisfactory. In a case of dislocation of the knee from injury he adapted a fixed bandage in the following manner:—A thin layer of cotton-wool was spread evenly over the joint, and retained by a bandage; over this strips of brown paper, two inches wide, and plastered on both sides with the mixture of sodium silicate and whitening, were placed in the same manner as in strapping a knee, and the whole was plastered over with the mixture, so as to procure an even surface. This bandage remained on for eight weeks, and at the end of that time it was removed; it was still perfectly firm. The patient was able to walk about comfortably with this bandage on.

In removing the silicate and whitening bandage some difficulty may be experienced, owing to its great hardness, but, if care be taken not to have too great a thickness of the coverings, this difficulty may be avoided.

We have therefore in this silicate bandage a means for providing immobility of joints and fractures which far surpasses in convenience, lightness, cleanliness, firmness, and cheapness any of the means hitherto made use of. If a limb be covered with cotton-wool, lint, wool, a worsted stocking, or any other soft protecting material, a surgeon may make use of strips of linen, bandage, or paper saturated with silicate of soda mixed or not with a salt of lime, such as chalk, whitening, or plaster of Paris, and procure a rapidly setting and extremely firm splint, the expense of which is reduced to a minimum.

I have used it now frequently in cases of fracture without swelling, or before swelling has come on, and with most beneficial results. The silicate of potash is, in my experience, inferior to the silicate of soda, and it offers no special advantage in any respect.

In the mixture of silicate of soda and whitening it appears, from experiments which I have made, that a partial decomposition occurs, and a lime silicate is formed, which is of very great strength. But the great hardness of this preparation proves sometimes objectionable, and I have now made use of a mixture of starch and silicate, with the effect of procuring an extremely firm, but slightly yielding, bandage. It seems that the addition of the starch thickens the preparation only, and allows of its being put on more as a paste; and as the mixture when dry is slightly yielding, it does not press so much upon bony prominences, and therefore is less likely to cause injury than the unyielding lime silicate.

Upon the whole, I am inclined to prefer the simple silicate for the majority of cases, as requiring less preparation; but, where special strength and immobility are required, I give the preference to the mixture of silicate and starch or silicate and whitening.

Another plan which I have adopted recently is to moisten the bandage with a saturated solution of calcium chloride, and apply the sodium silicate to this in the same manner as with the dry bandage. The result of this is to form a very hard bandage, which sets quickly. This necessitates, however, a double process, and is therefore not quite so ready of application as the pure silicate alone. In all these applications it is advisable to use the best and densest preparations of the silicate. Considering the readiness with which it can be applied, its cleanliness, and the smallness of its cost, I cannot but think this preparation is destined to supersede the materials at present used for the purpose of making fixed bandages.—*Medical Times and Gazette, March* 7, 1874, *p.* 263.

57.—REMARKS ON THE PRODUCTION OF SLEEP DURING SURGICAL OPERATIONS.

By Joseph T. Clover, Esq.

[There never has been a period when the opinion of surgeons as to the relative value of the different anæsthetics was more unsettled than it is at present. No doubt while there is danger in every kind of anæsthesia, there are conditions of safety, if we only knew them, that would make all the anæsthetics perfectly free from danger.]

When ether or chloroform is inhaled, the throat is stimulated; coughing and movements of deglutition commonly occur; vomiting even is sometimes directly excited, if the vapour be very strong. The act of swallowing is usually performed well enough; but, if the anæsthetic have produced sufficient effect upon the patient to interfere with reflex movements, then the deglutition may be delayed at the moment when the epiglottis covers the larynx. Raising the chin, and pulling it as far as possible away from the sternum, is usually sufficient to obviate this source of obstruction. If any one will try the experiment of attempting to swallow with his chin considerably elevated, he will find that he cannot do so without lowering it. When chloroform or ether vapour of greater pungency is given, there may be a direct spasm of the glottis produced. A similar spasmodic closure often occurs when nerves are pinched or divided in the course of an operation. It commonly occurs when a ligature is applied to a hemorrhoid.

I have never found it necessary to take hold of the tongue with forceps; but I advise that they should be kept ready, for it may be that the greater pungency of the vapour, when applied freely, causes an obstruction which raising the chin will not remove; and in such a case we must not be afraid of hurting the tongue.

Ether.—I was a junior student at University College, and saw Liston perform the first capital operation under ether that was done in this country. Although it continued to be used in certain cases in the operating theatre, there were objections to it which even now are but partially removed. The struggling against it, and the strange things patients said and did under its influence and whilst recovering, made us receive chloroform gladly, as a more convenient and less disagreeable agent. It was given with much freedom, until a death occurred at Newcastle in the practice of Sir John Fife. Of late, chloroform accidents appear to have increased, and thus medical men have very properly been stimulated to seek for a safer anæsthetic. Ether is undoubtedly safer; that is to say, life is not so quickly destroyed by it. But, nevertheless, I have seen a strong dog killed in sixteen seconds by being made to breathe pure ether through a tracheal opening. In the case of death at the West London Hospital after inhaling first chloroform and then ether, the patient had recovered from the chloroform so far as to have a fair pulse; but it ceased to beat very soon after the application of ether by means of a sponge made warm with water. The chloroform had, I suppose, sufficiently narcotised the patient to prevent the usual obstruction to strong vapours by closure of the glottis. If we are prepared to resist the remonstrances and struggling of the patient, ether may be poured upon a hollow sponge or towel folded into a cone, or upon a cone of felt covered with oiled silk, and held over his face. The patient will resist breathing, at first voluntarily, and afterwards more or less spasm of the glottis will be produced. The blood will be imperfectly oxygenated; and insensibility will be produced in a short time, partly from deprivation of oxygen, and partly from the action of ether. Less ether enters the tissues of the body in this way of giving it; and, if the operation last only two or three minutes, the recovery of consciousness will be speedy. Headache is not rarely complained of. Sickness is about as frequent as after a moderate use of chloroform. The patient sometimes remembers the choking sensation, and will not inhale it again if required. The flow of saliva is considerable; and the larger air-tubes are often obstructed by fluid, which makes a loud moist rhonchus; but it does not appear to be of much consequence, as it goes off afterwards.

I think most English surgeons are in favour of giving the ether slowly for about a minute or so at first; and then, when some degree of anæsthesia has come on, to give the vapour strongly. I am decidedly in favour of this plan, and have sometimes succeeded in getting a patient to sleep without any struggling. The most pleasant way is to give nitrous oxide till consciousness is lost, and then administer the ether of moderate strength. I should be afraid of giving *pure* ether by means of a large warmed sponge to a patient already well narcotised, for reasons already mentioned.

I have given ether a good many times by means of a bag, smaller than the one in my chloroform apparatus. The bag was regularly supplied with air, which passed over a large surface of ether, kept at the temperature of about 68 degrees, at the rate of about five-hundred inches per minute. I have for the present put this aside, and make use of what I term a double-current inhaler for ether. The object of this instrument is to effect a mixture of ether and air of the strength required. The internal arrangement is on the plan of an ordinary still, supposing the worm to be covered with calico for holding the ether. The ether is kept at a comparatively uniform temperature, the coldness produced by the evaporation of the ether during inspiration being balanced by the condensation of ether and watery vapour during expiration. The valves are so placed that the inspired air passes over the ether, and the expired air passes through the condenser. The ether in the upper part of the vessel being kept several degrees lower than the patient's breath, it is impossible for the proportion of ether-vapour to exceed a certain point. This proportion is more pungent than is usually required, and is diluted at pleasure by a sliding valve which admits air without passing over the ether. The advantages obtained are—1. The power of graduating the dose so as to avoid very much of the unpleasant pungency of ether as commonly administered; 2. Diminution of the smell of ether in the room; 3. It may be used in hot or cold weather without any water-bath, and the use of glass is avoided; 4. Economy of ether; 5. Saving of time. It can be kept ready for use, and will serve for four or five cases without adding more ether.

Directions for Use.—1. Put ether into the apparatus, after removing the inspiring valve until the vessel is filled to the level of the bottom of the tube. About ten ounces will be required. 2. Refix valve and cap, and rotate the box slowly on its long axis, to distribute the ether over the calico on the outside of the condenser. 3. Open the regulator, remove the three caps, and apply the tube with face-piece. 4. After using, pour out condensed vapour. It will generally be found convenient to suspend the inhaler by means of the strap passed round the neck; but as soon as the patient is asleep, if not before, it can be placed upon the couch. The condensed ether-vapour might be saved and redistilled, if the apparatus were extensively used in hospital practice. In order to prevent the escape of ether-vapour from the apparatus when not in use, I keep it in a tin box, the front and sides of which are double, so as to form a trough for water, into which the lid of the box is immersed. Of course, if the box be heated, the air inside may expand and escape in bubbles; but very little vapour is lost in this way.

Chloroform.—Next to nitrous oxide, I think chloroform is the least unpleasant anæsthetic to breathe. Patients who have previously taken chloroform are often unwilling to try nitrous oxide, because they have no objection at all to the former; but I have rarely found a patient who had once been put fully under the influence of ether, who was willing to inhale it again, unless it had been preceded by nitrous oxide.

Much has been said against chloroform, on account of the sickness which follows its use. I do not find it occur on an average more than once in seven or eight cases. The morphia or other preparations of opium, which are often prescribed after operations, will account for some of the cases of obstinate sickness. I admit that chloroform has more deaths to account for than ether has; but we must not forget that chloroform is much more frequently administered than ether, and that coroner's inquests are not held in countries where the practice of giving ether instead of chloroform is prevalent. Still there are greater dangers from chloroform than from ether, although I do not think them so insidious that every one who inhales it runs even an infinitesimal chance of losing his life.

The proportion of chloroform to the blood which will destroy life varies according to the species of animal, and in some slight degree according to the individual, but there is no animal that may not easily be killed by a full dose; and, therefore, whatever way we give the chloroform, it must be only a reasonable precaution to endeavour to avoid reaching that dose.

I have already explained how it happens that one person whose breathing is *excessive*, whilst his pulmonary circulation is *slow*, may get as much chloroform into the blood of the pulmonary veins, from air which contains a moderate quantity of chloroform, as another person, whose breathing is *feeble* and whose pulmonary circulation is *rapid*, may get from air which contains a much larger proportion of chloroform. Therefore, in whatever way the chloroform is given, the condition of the patient's pulse, as well as his breathing, should be carefully watched all the time he is inhaling; and, when the breathing is deep or the pulse weak, the chloroform should be diminished or removed. In the administration of anæsthetics generally, but especially in giving chloroform, it is advantageous to have the stomach empty or nearly so. I like to give a teaspoonful of brandy, without water, a few minutes beforehand, but not so much as a tablespoonful. If wine be given, or if the patient must have some water with the brandy, then they should be given half-an-hour before inhaling, to allow time for their absorption. Emphysematous subjects, especially, ought to avoid inhaling after a meal, but I do not consider them bad subjects for chloroform. The majority of fatal cases have had good lungs; and, seeing that it is by free respiration that the greatest amount of chloroform enters the blood, we should expect cardiac syncope to happen less frequently in cases where the respiratory function could not be much increased by excitement. It will appear a strange opinion, but I say advisedly, that I think there would be less danger from giving chloroform to the out-patients of a consumption hospital, than to the same number of distinguished athletes. The subjects of advanced fatty degeneration are bad subjects for any excitement or any anæsthetic, but we have no reliable means of diagnosing such a state. I believe I have given it several times when the heart was fatty. Where the disease is believed to exist, greater caution should of course be used, and it would be well to add one-sixth or one-eighth part of ether.

If I did not use any special apparatus, I would fold a piece of lint into a compress, of the size of a walnut, and tie it up; then pour upon each end of it a drachm of chloroform, so as to let it penetrate the lint without wetting all the surface; then let the patient smell at it at the distance of two inches, and gradually cover my hand and the patient's face with a towel; by warming the lint with the palm of the hand, nearly the whole of the chloroform can be evaporated slowly. It is known, by its no longer feeling cold, when the compress has become nearly dry and needs to be supplied with fresh chloroform. Not more than thirty minims at a time should be added after the first charge.

Of course, the proportion of chloroform in the atmosphere breathed is, in most of these ways, modified by circumstances: *e.g.*, by the temperature of he room, and of the chloroform at the commencement; then by the amount of cold produced by evaporation of the chloroform; by the distance the chloroform is held from the mouth; by the temperature of the hand, and the manner of holding the chloroform.

The slowness with which a timid patient inhales causes so much delay, that the administrator is sometimes induced to augment the dose; and then, when consciousness is lost, the patient is very liable to get more of it than is safe. Not a few alarming cases have had no bad symptoms until fresh chloroform was supplied; and then, in from a quarter to half a minute, as soon as there has been time enough for the new atmosphere to tell its effect on the heart, syncope has taken place.

Chloroform is said to prevent the shock of an operation; and it certainly does so to a great extent. We do not find fainting so common as it was before chloroform was used; but I have often felt a weakness and irregularity, and sometimes a decided arrest, of the pulse at a time when extensive incisions or injury to nerves has occurred. I think that a patient should have his pupils insensible to light, or breathe stertorously, before the commencement of a severe operation; but that one or two inspirations of pure air should be taken before the anticipated shock is given.

Methylene.—The so-called methylene bichloride is a compound body, similar in many respects to a mixture of chloroform and ether and alcohol. When put into a test-tube, with the bulb of a thermometer fixed just above the level of the liquid, and boiled, the temperature rises steadily; the more volatile substance escapes until the boiling temperature of chloroform is passed. The same rise of temperature occurs when we boil a mixture of ether and chloroform and alcohol.

It may be noticed also that, as the methylene is boiled, the smell of the residue becomes more and more like that of chloroform; but this odor diminishes again as the fluid is nearly dissipated. I do not suppose it to be simply a mixture of the substances named, but I think there is sufficient chloroform in it to make it unsafe to use it with the same freedom that ether may be used.

I have notes of about one hundred cases in which I gave it. Many of my patients were very efficiently and pleasantly put to sleep by it, and awoke with signs of cheerfulness, not usual after the use of pure chloroform; but in other cases there was very undesirable excitement. In one case, the respiration and pulse ceased, and I had to resort to artificial respiration to restore the patient. After this case, I felt less confidence in giving it than chloroform, and I now very rarely give it from choice.

In administering it, I sometimes used a cone of leather lined with flannel. The pungency of the vapour, when given in this way, makes patients often resist and struggle against it, so I more frequently gave it with my chloroform apparatus, using about sixty minims to each one thousand inches of air. I did not find sickness less frequent than after chloroform. I think it is an useful mixture, but it should certainly be used with caution, not only because it contains chloroform, but because the quantity of chloroform given off is not always in the same proportion to the more volatile ingredient. Methylene ether made by adding ether to the methylene bichloride would be liable to the same objections in a less degree.

Mixtures of Chloroform with Other Things.—I find that four to eight minims of ether and thirty minims of chloroform with one thousand inches of air mixed in my bag, answers very well. It sometimes produces more excitement than chloroform alone, but it certainly tends to make the immediate recovery from chloroform more pleasant, and keeps up a better pulse.

The addition of a fourth part of chloroform with one of ether will make the administration of ether on a towel much easier; but, inasmuch as it must be forced upon the patient more than chloroform alone, and the patients are almost sure to struggle, I have not used it very frequently, and cannot advise it.

I have used a mixture of alcohol, one part; chloroform, two parts; ether, three parts; and like it much, for cases where moderate narcotism has to be kept up a long time—*e.g.*, when caustic chloride of zinc has been applied to a cancerous growth. I have tried, in a few cases, tetrachloride of carbon, ethyliden chlorid, and most of the other anæsthetics that have been recommended; and the conclusion at which I have arrived is, that they are neither safer nor more pleasant than ether and chloroform; and, if not decidedly superior in these respects, it is better to continue the agents we are most familiar with.

Nitrous Oxide is an anæsthetic of a very different character to the preceding. I had the pleasure of showing the use of it at the Oxford meeting. Since that time, I have given it in 6,960 cases. When successfully given, the patient appears to fall asleep without any delirium or excitement; but, if the operation be one leaving much pain behind it, the patient sometimes will have a dream more or less connected with it, and then wake up rapidly and completely. It is by far the best anæsthetic for many short operations, such as the extraction of teeth, opening abscesses or boils. It answers very well in operating for strabismus. Removal of the eyeball was performed lately by Mr. Bowman for a lady, who said she had no consciousness of the operation. It is well suited for examining hysterical cases, wrenching stiff joints, and reducing luxations of recent date. I do not think it suitable for cases where it is necessary to keep the patient quiet more than three or four minutes; but, if the patient be allowed to recover consciousness after one inhalation before another is commenced, the anæsthesia may be kept up tolerably well for half an hour. I have never kept a patient unconscious for more than between six and seven minutes; but, if quietude is not important, I have no doubt that a patient could be prevented from feeling pain for an hour. But, although this is possible, I am sure the recovery afterwards would not be satisfactory. When chloroform or ether has been given for ten or fifteen minutes, a state of anæsthesia continues for some time afterwards; but the sensibility to pain returns within two minutes of ceasing to breathe the nitrous oxide, and therefore, in such operations as tying hemorrhoids and minor amputations, which might, from the short time they take, be thought fit cases for nitrous oxide, it would be unsatisfactory to the patient on account of the after-pain.

In administering nitrous oxide, a plentiful supply of gas is essential to success. There is no fear of patients inhaling too much at first; indeed, they should be told to breathe deeply and slowly. They often disobey the latter part of the advice, and pant as if they could not breathe too rapidly. A supplemental bag was my first contrivance for preventing air from being drawn under the face-piece, and for obviating waste of the gas in cases of this kind. I have now accomplished the same object by keeping the inspiring valve open, so that a part of the expired gas is driven back through it into the bag. I find that I can prolong the sleep of nitrous oxide during operations in the mouth by having the nose-cap and mouth-piece separate, and retaining the nose-cap in its place whilst the operation goes on. The supply of gas must be kept up freely, for, if a little air be drawn in by the mouth, the patient will soon become unsteady, if not actually sensible to pain. Gas is sometimes required where, from the inflamed state of the cheek or lips, the pressure of the face-piece could not be borne. A cap, of the size of the face, formed in front of a piece of glass, and of tin and India-rubber at the sides, answers exceedingly well, but a great deal of gas must be wasted to insure success. It is a good plan to cover the eyes during recovery, as a bright light or any moving object may cause an unpleasant dream.

If nitrous oxide should prove fatal, I think it will happen from the occurrence of syncope, from some other cause, during inhalation. Arrested breathing would soon be remedied by artificial respiration, if the heart were beating. I have, on three occasions, been obliged to resort to this means, and the recovery in all cases was satisfactory in less than three minutes.

Nitrous oxide may be used with advantage to prepare the way for giving ether; but I would not advise it before giving chloroform, because, firstly, chloroform is not very unpleasant if given gradually, and, secondly, it would be more dangerous to give chloroform in the state of excitement from gas than without it. I have used gas preparatory for ether in two ways. 1. By means of a bag of gas, I make the patient unconscious, and then give the ether as usual; but the patients sometimes wake up enough to struggle against the ether. We should be prepared for this, otherwise the patient may remove the inhaler and remain several minutes longer in a state of excitement. 2. By using a small bag which is supplied with gas during the inhalation, and by diverting the current of gas and making the supply of gas pass over ether, as soon as the patient loses consciousness, enough ether can be got into the system this way to keep up anæsthesia for three minutes with the mouth open. It is, therefore, well suited for small operations in the mouth, such as the extraction of difficult teeth. The best time for inhaling gas is when the stomach is nearly empty. If the state of the stomach would cause nightmare in ordinary sleep, it would also in that of nitrous oxide. About once in five hundred cases, I have known the patient to pass urine whilst under gas, the explanation being, that the bladder was so full as to require a voluntary effort to retain the urine.—*British Medical Journal, Feb.* 14, 1874, *p.* 200.

ORGANS OF CIRCULATION.

58.—CASE OF ANEURISM OF THE EXTERNAL ILIAC ARTERY CURED BY PRESSURE WITH LISTER'S ABDOMINAL TOURNIQUET.

By C. G. Wheelhouse, Esq., Surgeon to the Leeds General Infirmary.

[The patient was a publican, but of temperate habits. There was distinct history of syphilis.]

Twelve months ago a popliteal aneurism was found in the calf of the right leg, with pain, which became aggravated by a subsequent strain, and was treated by Mr. Wheelhouse, in consultation with Mr. Newstead, by continuous compression of the femoral artery for eight hours, by means of Porter's femoral compressor, which was well borne, and proved perfectly successful, the tumour in September last being perfectly hard, about the size of a pigeon's egg, and easily felt in the popliteal space. About the middle of July last the patient felt another tumour on the right side of the iliac fossa, constantly pulsating and very painful. It seemed to have formed gradually, there being no history of strain or other injury, but the right leg was colder than before. The author, with Mr. Newstead and Mr. Jessop, found a large pulsating and expansile tumour in the right iliac fossa, reaching from Poupart's ligament upwards to within two inches of the umbilicus, and extending in an outward direction almost to the spine of the ilium, about the size of a small cocoa-nut, hard and firm at the lower part, and softer in the upper portion, with pulsations and dilatations synchronous with the pulse in the left femoral artery. The swelling appeared to be connected wholly with the external iliac artery, but to extend above and overlap the common iliac; and although pressure could not be made on the latter sufficient to stop the beating, it was easily controlled by pressure on the abdominal aorta just above its bifurcation. After a consultation, treatment by pressure was determined upon, and, on Sept. 27th, chloroform was administered by Dr. Barfoot, ether substituted as soon as the muscles were relaxed, and its action kept up continuously for five hours, without any untoward symptoms, twenty-five ounces of ether being used. Lister's large abdominal tourniquet was applied just over the umbilicus, and slowly screwed down until the flow of blood through the aneurism was arrested. Two slips of the instrument occurred during the first half-hour, but it was at once replaced. Pressure was commenced at 1.15 p.m. The foot was cold and a little blue at 2.30 p.m.; at 3 p.m. the tourniquet was unscrewed slightly; pulsation in the tumour returned, and it was at once reapplied. At 4 p.m. blueness had extended beyond the knee, the left leg being very cold; at 5 p.m. the right limb was blue to the groin, and the left to the knee. Pressure was then slightly relaxed; the tumour much harder, but pulsation still perceptible. At 6 p.m. both limbs were black, and body blue as far as tourniquet. The instrument was then removed gradually in fifteen minutes, a quarter turn of the handle being taken every minute. The tumour had ceased to pulsate, and was firm and hard. The limbs were wrapped in wool and tied together, and a hypodermic injection of morphia given. (During the entire operation slight pulsation could be felt in the left femoral artery.) At 7 p.m. there was a slight pulsation in the aneurism, but it was firm and hard. At 10 p.m. pulsation had increased, but the tumour remained firm, and another morphia injection was given. A restless night was passed, and the next day the tumour pulsated with nearly its old force, but the walls of the aneurism felt thicker and harder than before the operation, and the beating was more "lifting" and less "distensile," the author predicting that it would slowly cease altogether. Two grains of opium were ordered to keep the bowels quiet; the limbs were recovering their natural hue and temperature, and on the second morning after the operation the tumour was again much harder, pulsation was almost imperceptible, the patient was easy, and in the evening of the same day the tumour was almost fully reconsolidated, and both limbs normal as to warmth. Three days after the operation the pulsation was found to have stopped, the tumour was hard and firm, and when examined on November 14th (forty-eight days after operation) no pulsation was found, the aneurism had contracted, was hard and firm, about the size of a cricket-ball, the limb had completely recovered, and the patient had returned to work. The author was assisted at the operation by Dr. Barfoot, house-physician, and by Mr. A. R. Dunnage, house-surgeon to the hospital. Sphygmographic tracings taken before and immediately after the operation were exhibited to the Clinical Society.—*Lancet, Dec.* 20, 1873, *p.* 880.

59.—ON THE TREATMENT OF ANEURISM OF THE ARCH OF THE AORTA BY GALVANO-PUNCTURE.

By Dr. Charlton Bastian, Physician to University College Hospital.

[Passing over the first part of this paper, we give Dr. Bastian's remarks concerning the methods of procedure in treatment by galvano-puncture.]

It has been now thoroughly established by the labours of Dr. John Duncan, Dr. Fraser, and others, that a clot can be readily obtained in blood-vessels by the action of the galvanic current. We know, however, comparatively little about the exact nature of this clot. It is described as being of dark colour, and as differing in appearance from ordinary coagulated blood. As Mr. Holmes has pointed out in his most able lectures on the surgical treatment of aneurism (Lancet, 1872, vol. ii., p. 327), the clot, which is first formed by electrolysis, is apt at times not to be persistent. Pulsation may cease in a tumour for a time after galvano-puncture, whilst in a week or ten days, it may again manifest itself—apparently because the clot first formed has subsequently melted away. But, in addition to this clot of somewhat uncertain composition and durability, formable directly by the action of the voltaic current, we have to bear in mind the fact, that even a very minute clot produced in this manner may form the nucleus around which a process of spontaneous coagulation takes place. This process of spontaneous coagulation leads to the formation of a more or less colourless clot, composed, in the main, of interlaced fibres of fibrine containing white blood-corpuscles within their meshes. This colourless clot also grows by the deposition of successive lamellæ, and the whole forms a firm, coherent, and somewhat elastic substance, admirably adapted to take off the pressure of the blood, and to act as a thoroughly efficient buffer. Now it was a clot of this latter nature which we found more than half filling the aneurism, and there are several reasons tending to show that the clot had been recently produced—as an indirect result, in fact, of the galvano-punctures.

It seems obvious that changes took place within the aneurism, as a result of the galvano-punctures. Thus, after the first occasion, a slight *bruit*, not previously audible, was to be heard over the sac itself; there was also apparently an altered incidence of blood-pressure for a time, since the blood began to pass more readily through the valvular opening into the left carotid—the pulsation in this vessel became more distinct. After the second galvano-puncture, there was a slight diminution in the amount of pulsation over the upper end, the *bruit* there still continuing; and there came a gradual diminution in the flow through the valvular opening of the left carotid, as judged by the diminished pulsation. After the third operation, the flow gradually ceased through the left carotid, and a very marked diminution took place in the amount of pulsation over the lower part of the swelling, where it had been previously increasing.

It seems almost certain, therefore, that the clot which we found in the aneurism had been produced as a result of the treatment to which the patient was submitted; this view being still further confirmed by the fact, that a button-like piece of the same kind of colourless material as that which constituted the larger clot, and of just the same diameter as its apex, was actually attached to the wall of the aneurism in a situation corresponding with the puncture made by the zinc-coated needle. I think there is good reason for believing, indeed, that the apex of this clot had been accidentally separated from the parietally attached portion, during the removal of the aneurism. For the laminated strata of the larger clot, were—to use a geological phrase—"conformable" with the surface of its upper portion. The clot had been formed by the successive deposition of laminæ upon its under surface; these laminæ becoming larger and larger, so as to give the whole a rudely conical shape, the base of which was already partly attached around its margin to the walls of the sac.

Methods of Procedure in Treatment by Galvano-Puncture.—The modes of performing galvano-puncture are numerous; and, concerning their respective merits or demerits very opposite opinions have been expressed by different physicians and surgeons. The subject itself is in its infancy; but the time at our disposal now will only allow me to allude to a few of the points concerning which a difference of opinion exists—and this principally with the view of explaining the reasons for the course pursued in the present case, and of suggesting the principles of a safe and tentative mode of attacking sacculated aneurisms of the arch of the aorta or of other great arteries of the body.

The risks specially attendant upon this mode of treatment are thus referred to by Mr. Holmes in his admirable lectures to which I have already referred. He says (*loc. cit.*, p. 665): "The dangers are principally from two causes—the inflammatory action produced in the sac and in the cellular tissue which surrounds it, and the gangrene or ulceration of the skin at the points of entrance of the needles."

Whichever method we adopt, we must, therefore, strive to reduce these dangers to a minimum. The principal points to be considered in this relation have reference to the nature of the battery to be employed, the strength of the current, the mode and duration of its application, and the size and nature of the needle which it is most desirable to use.

As in this method of treatment we wish to make use of the electrolytic powers of the battery current and to avoid the heat-generating effect upon the electrodes, it is better not to use the more powerful combination and larger plates of a Bunsen's or Grove's battery. We may get all of the good and none of the bad effects from the careful use of the battery which we employed in the present case, viz., Foveaux's modification of Smee's battery, made by Weiss and Co.

It is upon the question as to the most advantageous mode of employing the current that the greatest differences of opinion exist. Some writers recommend the insertion of the negative pole only into the sac, whilst the positive pole is applied to the integuments outside. (Others recommend the introduction of two needles into the sac, one of which is to be connected with the positive pole and one with the negative, the current being allowed to pass all the time in the same direction, and in the hands of different operators, being either strong for a short time, weak for a long time, or even strong for a long time. But some of those who recommend the insertion of both poles into the sac, follow Pétrequin in advising that the direction of the current should be changed from time to time, during the same operation. And, lastly, a third set of writers recommend the method of procedure which we adopted in the present case, viz., the introduction of a needle or needles in connection with the positive pole into the sac itself, whilst the negative pole is applied by means of a wet sponge to the adjacent skin.

Now, before making any comments of my own on these different methods of procedure, I will quote the conclusions arrived at by Baumgarten and Wertheimer, as given by Meyer (Electricity in its Relations to Practical Medicine, translation by Hammond, 1869, p. 470). He says:—"The numerous experiments made by them on animals invariably gave the following results. 1. If the needle connected with the negative pole was introduced alone into the blood-vessel, while the other needle was applied to the surrounding parts, no coagulation took place. 2. The introduction of both poles produced a slow, rather weak, and rarely perfect coagulation. 3. The introduction of the positive pole alone, with the application of the negative pole to the neighbouring parts, always brought about a rapid and complete coagulation." It would seem therefore (if we are to rely upon the observations of the experimenters abovementioned), that the third mode of procedure is the best, even from the point of view of the readiness with which coagulation is brought about, and its satisfactory nature; but it was not for these reasons alone that I was led to adopt this method, and discard those which consisted of the introduction of the negative pole into the sac, either alone or in combination with the positive pole. Whilst these latter methods seem to me to be open to grave practical objections, I know no compensating advantages to recommend them. In several cases in which the negative pole only has been inserted into the sac, blood has actually spirted out or come away in smaller quantity on withdrawal of the needle (see Holmes's Lecture in Lancet, 1872, vol. ii., pp. 697, 708), and it has generally happened that bleeding has been more prone to occur through punctures which have been in connection with a negative needle. Then, again, the bulk of gas given off from the negative pole is always much greater than that which appears at the positive pole, and this leads to the formation, in the former situation, of a loose frothy coagulum, as any of you may easily satisfy yourselves by introducing the poles of the battery into a little egg-albumen. Now, independently of the *à priori* undesirability of liberating a larger quantity of gas than is necessary within the circulatory system, this liberation tends to produce effects which the case we have been considering to-day seems to show should be especially avoided. A copious liberation of gas is apt to cause swelling and distension of the aneurism, in confirmation of which I need only refer you to the valuable paper on "The Treatment of Aneurism by Electrolysis," by Dr. Duncan and Dr. Fraser (Edin. Medical Journal, August 1867). Now this of itself is a condition of things which we should generally strive to prevent. On the other hand, if we seek for the reasons which should induce us to employ the negative pole, none are to be found which do not seem to be born of an over-zealous wish to accomplish too much in a short space of time, whilst some of those who recommend it, base their reasonings upon an altogether unsafe foundation. They argue that adequate results cannot be produced with the positive pole alone and moderately weak currents, because, under totally different conditions, a wholly different fluid (such as egg-albumen) is not appreciably affected. But they should recollect that the changes in a fluid so chemically complex and unstable as blood, cannot be safely measured by those which will occur in a fluid like egg-albumen. And, moreover, if we be not dealing with a case where galvano-puncture is resorted to with the view of producing a considerable and immediate coagulation, with the view of warding off an actually impending rupture (as in the case recently operated upon in this hospital by Mr. Beck), I think we should always strive to advance slowly and cautiously. We should operate with the view of initiating the formation of a laminated clot by starting the formation of a small nucleus in connection with the wall of the vessel rather than with the view of producing a large non-laminated clot immediately.

Because, therefore, the introduction of the negative pole into the sac can do nothing more, even according to its advocates, than increase the size and amount of the clot by a material which is admitted to be less durable and tenacious than that formed around the positive pole (Drs. Duncan and Fraser, *loc. cit.*, p. 14); because it is very questionable whether this is to be regarded as an advantage in the class of cases which we are now considering; and, finally, because its use seems to carry with it the positive disadvantages of favouring hemorrhage, of liberating an unnecessary amount of gas within the circulatory system, and of occasionally producing an unsafe distension of the sac itself, I am decidedly of opinion that it is undesirable to introduce both poles into the aneurism.

The method which I should adopt in any future case, therefore, would be that to which I have already had recourse; and you will find that in one of the most satisfactory cases yet on record of decided relief, practically amounting to cure, of an aortic aneurism, the treatment was conducted in this fashion by Dr. McCall Anderson of Glasgow. (Lancet, 1873, vol. i., p. 211.)

We come now to the question as to the nature and size of the needle to be used in galvano-puncture, and as to the strength of the current. The needles previously employed by most operators seem to have been larger than it is at all necessary to use, whilst some of them, such as Weiss's modification of Dr. Duncan's needle, are unquestionably very much too large. Recognising the desirability of reducing to a minimum the chance of hemorrhage through the punctures and of inflammation in the track of the needle, and, at the same time, of diminishing the painfulness of the operation, I was led to use a very small hare-lip needle. Of course, the needle must be large enough, and used in such a manner as to avoid all chance of breakage. The smaller the needle, the greater would be the amount of heat developed in it by the passage of the current, if this were strong and evolved from a large plated Bunsen's or Grove's battery; but, using such a comparatively weak current as we employed from one of Foveaux's batteries, we get rid of the heat-effects, and also, therefore, of any disadvantage which might otherwise attend the use of a small needle. But, whilst it is desirable to keep down the strength of the current as much as possible, in order to diminish the shock and pain which its passage may occasion (Dr. Duncan, *loc. cit.*, page 8), and also with the view of avoiding, as much as possible, the two special risks of inflammation in the sac, or of inflammation or gangrene of the skin at the seat of puncture, we should do all in our power to increase the amount of chemical action excited by the weak current at the positive pole. For you must understand that the experiments of Dr. Fraser and others have unmistakably established the fact that the coagulation induced in the blood by the passage through it of a galvanic current, is wholly attributable to the electrolytic or chemical changes which the current occasions. If, therefore, after the fashion originally introduced by Steinlein, we cover the positive pole with an easily oxidisable metal, such as zinc, we are enabled to summon to our aid what Faraday always distinguished as the "secondary" effects of electrolysis. The salts as well as the fluid of the blood are decomposed by the action of the current, and a part of the oxygen and acids which are liberated at the positive pole will combine with the zinc and corrode the surface of the needle. Some of the salts thus formed will aid in producing further coagulation around the needle. Corrosion occurred to a marked extent in our last operation, when a current from eleven cells of the battery was allowed to act upon a needle which had been plated with zinc, although no corrosion was produced in an ordinary steel needle submitted to the same influences. Of course, a gilt needle would be less still amenable to the action of these "secondary" effects of the current.

With regard to the strength of the current, it was found that eleven cells acting for half an hour, although producing no pain or obvious sensation at the time, did induce some amount of irritation of the skin around the punctures. The current from eight cells, however (the battery being in good condition at the time), produced, on the previous occasion, no irritation, so that all traces of this needle-puncture has disappeared in four or five days. This needle also was insulated within half an inch of its point merely by two layers of spirit varnish; and probably in employing a comparatively weak current such as this, no insulating material is needed. This, indeed, is the opinion of Dr. Althaus. At all events, it seems desirable that the needle should not be pushed too far into the sac, and that the part of the needle actually in the sac and within its coats should not be insulated, because clot should form in contact with the wall of the aneurism, so that it may be left adhering to it on the withdrawal of the needle.

Conducted upon the principles now laid down, the treatment of aneurisms of the arch of the aorta or of other large vessels may, doubtless, in a large number of cases, be safely attempted by galvano-puncture. The treatment should be painless, non-irritating, tentative, and slow; and I believe that it is a method from which, in the future, we may look to obtain the most satisfactory results.—*Brit. Med. Jour., Nov.* 29, 1873, *p.* 623.

60.—CASE OF POPLITEAL ANEURISM SUCCESSFULLY TREATED BY CATGUT ANTISEPTIC LIGATURE.

By John Morgan, Esq., Professor of Descriptive Anatomy in the Royal College of Surgeons, and Surgeon to Mercers' Hospital, Dublin.

[The following case was treated in various ways, by position, by rest, and by different modes of compression, but without success. The case is peculiar, as being that of a woman. As it was going on from bad to worse and the pain was excruciating, an operation was absolutely necessary.]

I ligatured the artery in Scarpa's angle. The patient was put under the influence of ether, and there was no sickness or any other unpleasant symptoms. The incision having been made and the artery exposed, I passed an aneurism needle round the artery and secured it with a catgut ligature. I was about to pass a second ligature when a little vein got torn. I therefore did not pass it, but closed the wound by four carbolic sutures. The woman was removed to bed, and from that time to the present there was not a drop of pus; the wound healed by first intention. I never saw the catgut ligature afterwards. The patient complained of none of that excruciating pain she had previously suffered; but curiously enough she complained of some pains shooting up the upper thigh on the outside and also in the leg. The tumour which before had become prominent became solidified. One spot for some time was a cause of uneasiness, but it gradually consolidated and the tumour became firm, and no pulsation was found in it from the time the ligature was applied. I saw her a fortnight ago and she was then able to walk about the room, and on comparing one limb with the other I found there was a difference of nearly three-quarters of an inch between the two. The tumour had almost entirely subsided, and the woman could walk about without any pain or uneasiness. I might mention that I gave her a medicine which I have found of considerable use, and which I am now giving to another patient—that is, sulpho-carbolate of soda. The case now under my care is that of a man whose leg has been smashed, so as to necessitate amputation. It is a most unfavourable case, the only thing in the man's favour being that he is but 23 years of age. I gave him the sulpho-carbolate of soda, and no untoward symptom has occurred. I have given it also to this woman and, I believe, with beneficial results. I have seen Mr. Tyrrell's case, which, I think, does not go on as favourably as the one just described; the tumour has not disappeared so quickly, but the result in both is equally satisfactory. Both resisted treatment by pressure and were cured by ligature. *Medical Press and Circular, Jan.* 7, 1874, *p.* 3.

61.—CASE OF CIRSOID ANEURISM TREATED BY INJECTION OF PERCHLORIDE OF IRON; DEATH FROM EMBOLISM.

By James F. West, Esq., Senior Surgeon to the Queen's Hospital, Birmingham.

Despite the brilliant success which has attended the employment of coagulating injections for the treatment of nævus in the hands of MM. Broca, Gosselin, Demarquay, Professor Pitha, and others, they are evidently dangerous to medies in certain cases, especially when the tumours are about the face and head; and it is incumbent on those who have had fatal cases to publish them as a warning to others not to use such injections in the future, but to adopt some less hazardous mode of treatment. Mr. Bryant mentions that he lost a patient from embolism a few minutes after injecting a nævus, though he does not say on what part of the body it was situated. Mr. Thomas Smith, in an excellent paper on the same subject, gives several cases in which a fatal result attended the use of perchloride-of-iron injections. Recently Mr. Kesteven has also called attention to an unsuccessful case in his practice; and although he attributes death in that instance to "spasm of the glottis, induced by mental emotion," I cannot help thinking the result was more likely due to embolism. The absence of a post-mortem examination, however, prevents the solution of the question.

Such experience as is afforded by these records and by the following case must make all thoughtful surgeons pause before recommending the injection plan of treatment, and induce them rather to have recourse to either enucleation, the galvano-cautery, the ligature, or what I very much prefer, the wire écraseur. In the Lancet of March 4th, 1871, I have reported several cases in which considerable nævoid growths were treated by me in this manner with success, and further experience has shown me that the écraseur is one of the best, if not *the* best means we possess for the removal of vascular tumours.

Alfred G. T., aged nine months, was admitted into the Queen's Hospital, under my care, on June 2d, 1873, with a nævoid tumour, about the size of a small marble, on the right ala nasi, near the tip. It had existed from birth, but was slowly growing, and beginning to involve the left ala. It was soft and compressible, and over its surface large and distended capillaries were seen in three or four spots, each as large as a pea. Deformity from the prominent situation of the tumour necessitated operative interference, and as it was considered that no mode of operation offered so good an opportunity for the prevention of an ugly cicatrix as the injection of solution of perchloride of iron, it was decided to use it. At 11 a. m., chloroform having been given, three drops were injected by a hypodermic syringe into the lowest part of the tumour. The child struggled and cried, but there was no other unfavourable symptom. Three more drops were then injected into the upper part of the tumour, and three at another highly vascular spot, when, on withdrawing the syringe, the child's face changed suddenly to a dusky hue, the hands and feet became blue, the pulse could not be felt, and the breathing seemed to be arrested; on raising the eyelids, the eyeballs were seen to be fixed and turned upwards and outwards, and the pupils were slightly dilated. The tongue was at once drawn forwards, and artificial respiration kept up for twenty minutes. During some part of this time the child breathed naturally, and then would cease to breathe until Silvester's method was again used. Until 6 o'clock the same evening he continued in a half-unconscious state, with a very feeble pulse, dilated pupils, livid hands and feet, and the angle of the mouth on the right side somewhat drawn down. About every ten or fifteen minutes he would start crying, and then relapse into his former semi-comatose state. During the evening the extremities became warmer and of a more natural colour, the breathing less laboured, and the pulse better; but the eyeballs rolled about, and the child could not be roused to take notice, and could hardly be induced to imbibe milk from his bottle. He continued in this state for three days, when he died, with well-marked symptoms of hemiplegia.

At the post-mortem examination, the membranes of the brain were found slightly congested; the anterior portion of both lobes of the cerebrum, and also the inner surface of the brain as seen in the walls of the lateral ventricles, were reduced to a soft pulp, while the posterior part of the brain-substance was in a healthy state. A clot was seen stretching from the right internal carotid artery along the middle cerebral artery of the same side. The lungs were congested. The heart contained a small quantity of dark fluid blood. The liver, kidneys, and other viscera were normal.

That embolism was produced in this instance by the injection of the perchloride *of iron does not admit* of a doubt, and I therefore unhesitatingly confirm *Mr. Thomas Smith's opinion* that we are justified in rejecting it as a remedy *for nævi on the face.—Lancet, March* 21, 1874, p. 402.

62.—THE WINGED SCREW TOURNIQUET.

By Surgeon Major A. Moffitt, Netley.

In consequence of the reports of Professor Longmore and other writers on Military Surgery, regarding the evils arising from the constriction of the limb in the application of the tourniquet for the arrest of hemorrhage in gun-shot wounds, numerous experiments were conducted by me at Netley, with the view of obtaining an instrument, the use of which would not be attended with the serious and often fatal consequences described as occurring in warfare. The instruments at my disposal were, Lee's tourniquet, Mott's tourniquet, the field tourniquet of the British Service, Petit's tourniquet, Signoroni's clamp, and the artery compress used in the cure of aneurism.

The following were the qualities which it was decided a tourniquet for use in military surgery should possess: 1, portability; 2, simplicity; 3, good compressing power; 4, steadiness, so as not to shift position in lifting and laying the patient; and 5, not to constrict the limb.

Lee's tourniquet answered well for the arm, and was found to be portable, simple, and steady; but for the thigh it had not compressing power enough, and, in addition, it constricted the limb where the patient was at all muscular.

Mott's tourniquet answered fairly for the thigh, though deficient in compressing power; but in the arm, owing to its convex pad and peculiar shape, the slightest movement of the patient caused it to slip on one or the other of the bones, where it could exercise no pressure on the vessel. It is not very portable from its awkward shape.

The field tourniquet of the British service was found to have very little compressing power over the artery—the pad being too small. It, however, constricted the limb to a dangerous extent, and often, when it had no effect upon the arterial circulation, it completely arrested a return of blood through the veins. It has no quality but portability to recommend it.

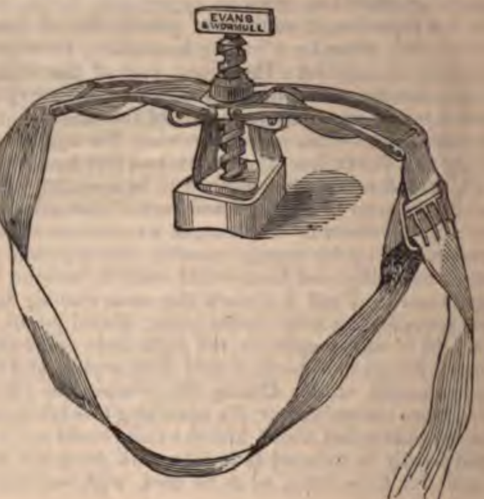


Fig. 1.—The Tourniquet as applied.

Petit's tourniquet was found to have a good compressing power, which could easily and quickly be brought to bear or slackened. It is also very portable; but it constricts the limb, is liable to shift its position, and is rather complicated.

Signoroni's clamp possessed power of compression and did not constrict the *limb*; but it is too clumsy for field purposes, and liable to slip off the vessel.

The artery compress for the cure of aneurism was found to have good compressing power, and did not constrict the limb; but it is deficient in all other requisite qualities. The idea of depressing the pad by the screw is good, and is the principle carried out in the tourniquet which will presently be described.

Thus far none of the tourniquets tried came up to the requirements sought for. A combination of the good qualities of all was then attempted, and for this purpose Petit's tourniquet was selected, as the base on which to work. The idea of wings and the shape of the pad were taken from Lee's. The wings, made of steel bars, were attached to the lower plate of Petit's tourniquet, and the strap carried across rollers at their points. When open, these wings extended out at each side, so as to give a span of 6½ inches, which extends the diameter of any ordinary thigh. A quadrangular wooden pad half an inch thick, two inches long, and one and a half broad, and slightly concave on its under surface, was fastened by a loop on the under surface of the lower plate. The tourniquet thus constructed was tried, and found to be portable, had good compressing power, did not constrict the limb, but was a little complicated, owing to the strap having to pass over so many rollers, and had a tendency to turn over the long axis of the limb. This tendency to turn over, however, was remedied by lowering the points of the wings; but then it was found these latter constricted the limb.

The wings were then attached to the upper plate, instead of to the lower one; the lower plate was removed altogether, and a small square plate substituted and attached to the pad by four screws, having the strap permanently fixed under it. The instrument thus constructed is shown in the accompanying figure (1). On trial, it was found to possess all the qualities sought for—it is portable, simple, has good compressing power, is steady, and does not constrict the limb. When folded up, it forms a package 3¼ inches long, 2 inches broad, and 1¾ deep (Fig. 2). There is a double shortening of the strap at each side, and in this way the requisite amount of pressure is rapidly reached, the pad is pressed down upon the vessel by the action of the screw, as in the artery compress for the cure of aneurism, and at the same time the wings are raised and all constriction of the limb at each side of the pad removed. The double shortening of the strap is of great importance, as it was tried to cut the strap and sew the end of one piece round the cross bar at the point of each wing for the purpose of greater simplicity, and with this arrangement, which gave only a single shortening at each side, the screw reached the head before the requisite amount of pressure could be made.

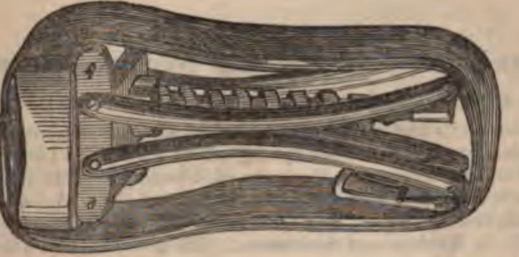


Fig. 2.—The Tourniquet folded up.

As to the concave pad employed, instead of the usual convex one, I need only refer to Lee's arguments on this head.

It is a matter of importance that all old screw tourniquets are convertible into this pattern at a small cost, by the makers, Messrs. Evans and Wormull, Dowgate Hill, who ably carried out my ideas in the manufacture of the instrument.—*British Medical Journal*, *Jan.* 3, 1874, *p.* 16.

63.—ON THE SAFETY OF TORSION IN AMPUTATION.

By Thomas Bryant, Esq., Surgeon to Guy's Hospital.

The following case, brief notes of which I now forward, I think may be read with interest, supporting as it does the practice of torsion, and telling against the use of the carbolised catgut ligature.

On Friday, March 6th, I was called upon to remove a mutilated forearm from a man aged twenty. I amputated below the elbow-joint, and arrested bleeding by twisting all the arteries with the exception of the interosseous, to which I applied a ligature of carbolised catgut. I could not twist this artery on account of its intimate connexion with the interosseous membrane. At the end of six days hemorrhage took place from the stump, which was stopped by raising the limb; but as it recurred towards night the stump was opened. It was then made out that the bleeding had taken place from the interosseous artery. The vessel was again tied by my dresser, and the man has gone on well since.

This is the only case of secondary hemorrhage from a stump that has occurred to me since I began the practice of torsion of arteries in January, 1868; and it is interesting to know that in it the bleeding came from an artery that had been ligatured, and that the ligature was of carbolised catgut.

We have now had at Guy's Hospital upwards of 200 cases of amputation of the thigh, leg, arm and forearm, in which all the arteries have been twisted (110 of these having been of the femoral artery), and no case of secondary hemorrhage.—*Lancet, March* 21, 1874, *p.* 417.

64.—A NEW ARTERY AND TORSION FORCEPS.

By T. Spencer Wells, Esq., Surgeon to the Samaritan Hospital.

Mr. Spencer Wells has introduced a kind of artery and torsion forceps, which very conveniently replace the old spring artery forceps of Liston, and the bull-dogs used for the temporary stoppage of bleeding vessels during operations, while they are the most readily applied of any of the varieties of torsion forceps met with in the shops. They have scissors handles, and were first made for Mr. Wells by Krohne and Sesemann. The grasping and holding extremity is roughened by rather deeply cut transverse teeth, so that the bleeding vessel is forcibly compressed, and its coats squeezed or almost crushed together. This is alone often sufficient to stop the bleeding without any torsion, especially if the instrument be left on the vessel for a minute or more. But if the vessel be large, then two or more rotations may be added. Instead of the spring-catch, the fastening is effected by a Mathieu's catch in the handles. This is quite as easily fixed and opened as the spring, and is much less likely to get out of order. The instrument is made of steel, but is coated with nickel, which prevents any rusting after use. Mr. Spencer Wells, as is well known, trusts to the clamp for securing the pedicle in ovariotomy whenever it can be applied without too much pull on the uterus or broad ligament. Where the pedicle is very short or broad, he either uses the cautery, the ligature, or the *écraseur.* He has never trusted to torsion alone, though occasionally securing one or more vessels in this way. With the bleeding vessels in separated omentum, he trusts to torsion when the vessels are distinct; but when oozing surfaces rather than separate vessels are seen, he uses ligatures of fine pure silk, cutting off the ends short and returning them. He prefers this silk to catgut. With bleeding vessels in the abdominal wall, torsion, or simple compression with the torsion forceps, is sufficient.—*British Medical Journal, Jan.* 10, 1874, *p.* 47.

65.—REPORTS FROM THE METROPOLITAN HOSPITALS ON THE USE OF TORSION IN SURGICAL OPERATIONS.

Torsion of arteries is a proceeding largely but very unequally adopted by modern surgeons. The procedure is commonly adopted at some of the metropolitan hospitals, but is rarely witnessed at others; in explanation of which fact it will be noticed (upon perusal of the following reports) that the opinions of the value of torsion entertained by different surgeons are widely divergent. Gentlemen who have most largely employed it are generally found to be those who most commend it; whilst dread of secondary hemorrhage, or other mishap, seems to deter many from even entering upon its use. Such theoretical fears, however, appear to be groundless, as the anticipated mishaps do not occur in actual practice. Mr. Callender, of St. Bartholomew's Hospital, remarked, in a lecture published in the British Medical Journal, on January 20th, 1872, "there is no record, where the operation has been properly practised, of any sloughing of the twisted end, or of any abscess along the track of the vessel; and whilst the presence of a foreign body in the wound is avoided, the patient escapes the anxiety which the prospect of the removal of ligatures entails. And to add one other, and this a strong argument in favour of torsion, it is free from all risk of that secondary bleeding which is sometimes associated with the separation of a ligature." The opinion of Mr. Birkett of Guy's Hospital is equally strong in favour of torsion; he always uses it in his operations, and has never met with recurrent or secondary hemorrhage after its employment, nor with suppuration extending up the sheath of the twisted vessel. In fact, if the bleeding be once thoroughly arrested by torsion, it never recurs. Torsion requires considerable manipulative skill for its successful employment; a circumstance which young operators are apt to forget. Much time also is consumed where all the small arteries are twisted; consequently the catgut carbolised ligature is sometimes used for small vessels, and torsion is restricted to the main arteries—the very reverse, as Mr. Forster observes, of the precepts formerly inculcated. Want of space will not permit all the replies which have been received to appear now. Our thanks are due to the surgeons who have favoured us with the communication of their experience and opinions on the subject.

Sir James Paget writes: I have not any large experience of torsion of arteries. My impression is that we ought to learn which cases are best suited by each of the chief means of stopping hemorrhage—ligature, carbolised ligature, acupressure, torsion; and in each case to use the most appropriate means.

Mr. John Wood, of King's College Hospital, is of opinion that, although torsion is undoubtedly of value in arresting bleeding from the smaller arteries, especially in plastic operations on the surface of the body where extensive primary union is essential, yet its application to the larger arteries does not present so many advantages as that of the ligature. The grounds of his opinion are as follows.

1. Torsion is not so certainly effective against primary and intermediary hemorrhage as a well applied ligature. A surgeon obliged to leave his patient without immediate surgical supervision after an operation, cannot be so confident about his safety from bleeding as when ligatures have been employed.

2. The ligature is more speedy in its application in cases where many arteries are bleeding at once, and it is important to prevent much effusion of blood without other means of restraining it. However dexterously torsion may be applied, it occupies more time than the ligature.

3. In instances where the artery is cut off short in a hollow part difficult of access, as between the bones of the leg in amputation below the knee, and when the vessel and the tissues in its immediate contiguity are diseased, thickened, and consolidated, the process of isolation necessary for torsion is tedious, and sometimes involves much loss of blood, and a good deal of violence to the diseased parts; while, on the other hand, the ligature, including perhaps a small portion of the parts adherent to the vessel, and thus supporting and strengthening it, is usually a quick and effective proceeding.

4. Arteries of unusual delicacy of texture, or softened and rendered friable by disease (constituting the most trying tests of any hæmostatic proceeding) are apt under torsion, especially unlimited torsion, to have so much injury inflicted upon their sheath-attachments and *vasa vasorum* for a considerable distance, as to become subject to disorganisation, suppurative inflammation, and burrowing of matter along their sheaths. In limited torsion, the pressure of the forceps applied transversely above the twisted part, necessarily inflicts a certain amount of damage upon the arterial coats at that point, rendering them more likely to slough or yield to the forces of the circulation, than when a ligature is applied, and such pressure is not needed. This is especially important in the not uncommon cases where an efficient internal clot is not formed.

5. Where a branch issues from a large artery close above the point of section, torsion involves the probability of such a branch either preventing sufficient and effective twisting, or becoming itself detached from the parent trunk, and thus forming a source of hemorrhage from the aperture of its exit, and perhaps also from its torn-off distal end. Under the same circumstances, a ligature is usually easily and effectively applied.

6. In instances where torsion is hurriedly or roughly performed upon a large artery, the vein in its immediate neighbourhood, and often closely connected with it, is apt to be pulled about, dragged out of its sheath, or torn, in separating those connections. So the vein is rendered more prone to assume unhealthy inflammation, or pouches and interspaces are left, which may become the seat of putrefactive or suppurative processes in dangerous contiguity to the open end of the vein.

7. Neither limited nor unlimited torsion removes the possibility of necrosis of the twisted end of the artery, which would, in case of its occurrence, interfere with and prevent primary adhesion at that point, just as much as a ligature or any other foreign body, and leaves a less direct way for the escape of the slough, than a ligature does when left hanging out of the wound.

8. A metallic or hempen ligature placed upon a large artery, and left hanging out at the nearest and most dependent part of the wound, acts beneficially in conducting the discharges to the surface from the dangerous vicinity of the large associated and contiguous vein, and this advantage more than compensates for the small interference with primary adhesion to which it may give rise. In very many cases also, these larger vessels lie in close proximity to a cut bone, which, from the shape in which its section is usually left, preventing close contact of the surrounding soft parts, is the least likely place in the whole wound for primary union to take place, and is the most usual seat of deep suppuration during the process of healing after amputation. In such cases, the ligatures may be made use of as valuable conductors of purulent discharges from a dangerous and often inaccessible neighbourhood. Mr. Wood usually utilises ligatures in this manner by passing carbolised drainage-tubes alongside or over them. When hempen ligatures are used by him, they are usually first well waxed and steeped in carbolised oil. In plastic operations, especially those involving the urinary organs, acupressure and silver wire sutures are employed.

9. Lastly, Mr. Wood is of opinion that, after amputation and excision, entire adhesive union is not so usual a result as to render it a matter of essential importance to avoid the presence of a few ligatures, which may interfere with it in their linear course towards the surface, but that, on the contrary, they have the more important advantage of acting as direct drains, providing against the not unfrequent deep retention of discharges which prolong the convalescence, and often seriously imperil the safety of the patient.

Mr. SAVORY (St. Bartholomew's Hospital) thinks torsion, which is an old practice recently revived, is one of the best means of arresting hemorrhage in certain cases; but, as a rule, for the arrest of hemorrhage from important vessels, he prefers ligature. He considers that we are not yet justified in assuming that torsion affords the same degree of security as ligature, and he believes that the objections to ligature when properly applied have been exaggerated.

Mr. COOPER FOSTER (Guy's Hospital) writes: "The subject of torsion, so far as I am concerned, is pretty well used up, and I have nothing further to add to the subject. It is much more applicable to large than to small arteries, and easier to apply; this is quite contrary to what I was taught in my student days, when the twisting of large arteries, or of one larger than the radial, was never thought of. Mr. Morgan, however, taught us to twist a vessel of that size if we liked, though he never did it. The smaller vessels are troublesome sometimes to twist, and then the catgut ligature comes in very well, as it saves time. I never use any other plan than torsion for all my operations when hemorrhage has to be arrested, except as stated above.

Mr. Thomas Smith (St. Bartholomew's Hospital) writes: I have used torsion in surgical operations enough to know that in skilled hands it is an efficient and safe method of arresting hemorrhage from divided arteries, and that, in cases where a speedy union of the operation wound is desirable, torsion is superior to silk ligature, inasmuch as no foreign body is left in the wound. I rarely use torsion, on account of the time that is often consumed in securing the smaller vessels. I much prefer, and therefore employ, carbolised catgut, cutting off both ends of the ligature. This can be employed, without difficulty or loss of time, by an unskilled person, to all bleeding vessels; and it has the chief advantage that torsion possesses, namely, that no noxious foreign body is left in the operation wound.—*British Medical Journal, Dec.* 20, 1873, *p.* 723.

66.—EPISTAXIS SUCCESSFULLY TREATED IN TWO BAD CASES BY A SIMPLE PROCEDURE.

By Dr. William Waugh Leeper, Loughgall, Ireland.

A few weeks ago I was in attendance upon a man in typhoid fever. The case pursued the usual course. On the fifteenth day there occurred profuse perspiration with abatement of all the grave symptoms; but on the night of the sixteenth day of the disease, some bleeding occurred from the nose. It was a mere trickle at first, but after the patient slept a second time the hemorrhage increased to such an extent that I was sent for. On my arrival, after a drive of four miles, I found the man cold and almost pulseless, with a constant effort at vomiting. The blood was slowly oozing from each nostril, and when he coughed or made an effort to speak, he spat up the same dark blood. The idea of plugging the nostrils occurred to me, but the operation seemed fraught with great danger, in the case of one in almost a dying state from exhaustion; so I thought of trying a remedy of easy application. I pushed up into each nostril two medicated bougies or suppositories of perchloride of iron. These I happened to have with me; they had been made (of double the usual strength) with two grains in each. I had them made by Duncan and Flockart, of Edinburgh, for another purpose altogether, *i.e.*, to push up into the uterus. I pushed two *up together*, and placed a bit of dry lint on the anterior nares. The effect was a present cessation of the bleeding, and I prescribed five grains of ergot, reduced to powder, every four hours. The result was successful for two days; when again in the middle of the night, the bleeding returned. The same remedy was again used, and with the same happy result, and after that there was no return, and the man made a good and perfect recovery.

Case 2. About ten days after this case I was called to visit an old man (aged seventy-two), suffering from epistaxis, which every remedy had failed to check. This hemorrhage also occurred in the middle of the night. When I reached the man's residence the bleeding had been going on for several hours. The old man was pale, face covered with cold sweat, and he had *frequently* vomited up blood. A stream of blood flowed from each nostril, and on looking into his fauces a similar stream seemed to flow down to the stomach, which accounted for the vomiting of such a quantity of blood.

I prescribed the ergot, this time the liquid extract of B. Ph. in scruple doses every four hours, and applied, precisely in the same way, the perchloride of iron bougies or *suppositories*. The result was the same as in the preceding case —*viz., almost the immediate* cessation of the hemorrhage, and in this case there *was no return of it.*

I have recorded these two cases simply because of the simplicity of the remedy used. These pencils soon melt, and the iron gradually comes in contact with the bleeding surface, and instantly its styptic effect is produced. The remedy is easy of application—much more so than plugging, and was in these cases more effectual than injecting the nares with the same remedy in a fluid form—a mode which I have occasionally tried. I attach great importance to the internal use of ergot in all hemorrhages, and hence I prescribed it in these cases. In hæmoptysis, and in certain hemorrhages from the uterus, I have found the sub-cutaneous injection of ext. erg., dissolved in glycerine, the most effectual and the promptest remedy.—*Dublin Journal of Medical Science, Nov.* 1873, *p.* 364.

67.—AN IMPROVED PLUG FOR EPISTAXIS.

By Dr. Henry Cooper Rose.

Having lately had a severe case of epistaxis, which was obliged to be restrained by the old-fashioned plug of lint drawn into the posterior nares by means of a string passed through the nostrils, my attention has been directed to the subject. It occurred to me that, if an elastic India-rubber bag could be introduced into the nose, and then either inflated with air or filled with iced water, all hemorrhage must inevitably be stopped.

I proceeded, therefore, to make an instrument in the following manner. I cut a No. 4 gum-elastic catheter into two equal lengths. At the end of one portion, I made small holes by transfixing it with a needle heated to redness in a spirit-lamp. These holes extended, at intervals of a quarter of an inch, for the length of about two and a half inches. Over this punctured portion I tied firmly in two places, first at the tip, and then again about three and a half inches nearer the handle, a soft India-rubber bag, so as to include between the two tied portions all the perforated part of the catheter. To the open end of this catheter I fixed a short piece of India-rubber tubing furnished with a small stopcock.

Mode of Using.—All the air having been let out of the bag by opening the stopcock and gently compressing the India-rubber, somewhat after the manner of fastening an umbrella, the instrument is passed into the bleeding nostril. When it is coaxed so far that the end projects into the posterior nares, or as far as it can be, the nose of a small ball-syringe is applied to the mouth of the stopcock, and the bag is gently inflated while the air is being propelled into it with the right hand. The short piece of India-rubber tubing attached to the stopcock is compressed with the thumb and finger of the left hand at the right moment to prevent the air from passing out again; and then with the right hand the stopcock is turned. By this means, the inflated bag is made to fill all the inequalities of the nasal cavity, and a painless and perfect plug results. Should it be deemed advisible, iced water may be used instead of air. It may be found in practice necessary to have a thicker tissue for the bag, or even to use two, one over the other, to prevent bursting with the pressure employed, which, however, need not be great. Should it be found necessary to make the instrument stiffer while it is being passed through the nostril, the ordinary wire stilette may be introduced through the open stopcock. Each instrument should be accompanied by several supernumerary bags; and, before using the instrument, it should be dipped into warm water, to increase the elasticity of the India-rubber.

I believe that this contrivance possesses sundry advantages over the ingenious instrument devised by Mr. Godrich; *e.g.*, it is exceedingly inexpensive, quickly made and repaired, very easily introduced because of its small calibre, and the universal pressure over the whole nostril is much less annoying than when exerted over two points only.

I have taken a specimen of the instrument to Messrs. Coxeter, of Grafton Street, East, who will have some prepared for inspection and trial. It is well adapted to hospital practice, because the introduction of it for an hour will probably stop hemorrhage; and the removal is effected very easily by simply turning the tap and letting out the air.—Brit. Med. *Journal, Jan.* 10, 1874, *p.* 49.

68.—THE EMPLOYMENT OF THE CAOUTCHOUC ELASTIC LIGATURE.

By Dr. Francis J. B. Quinlan, Physician to St. Vincent's Hospital, Dublin.

After some introductory remarks upon the different modes of removing vascular growths by the galvano-cautery, the écraseur, and by ordinary ligature, Dr. Quinlan said:—

The case under our consideration, and lately under your observation, is that of Bridget G., a healthy, well-nourished countrywoman, of plethoric temperament, aged twenty-seven years. Upon examining her, we found, at the upper part of the vulval opening, a large vascular tumour, of the size of a very large orange. This tumour was of several years' standing, and bore an exact resemblance to a small cauliflower. It was connected with the site of the clitoris by a pedicle resembling the stalk of that vegetable, which was topped with the arborescence of the tumour. From either side of the pedicle descended two hypertrophied labia minora, like enormous cockscombs. The case was evidently one of syphilitic hypertrophy—a diagnosis which was thoroughly confirmed by crops of condylomata around the anus and on the folds of the nates. The cauliflower tumour, which was divided into two portions by a fissure extending to the very root of the pedicle, was evidently the hypertrophied prepuce and frenum of the clitoris, and the peculiar arborescent appearance was attributable to the trabeculæ of the erectile tissue in this part of the labia restraining the hypertrophy of the rest of the tissue, and, by binding it down in bundles, producing the arborescence. The condylomata and the local irritation being reduced by rest and appropriate treatment, the question arose as to the best and safest method of removing this disfiguring hypertrophied mass; and it struck me that the newly introduced elastic ligature would admirably act in the case of this most vascular growth, so liable to cause dangerous hemorrhage. Accordingly, on the 20th of December I ligatured the whole mass with a solid India-rubber cord two millimetres in thickness. I used this solid cord because the result of some previous experiments had satisfied me of the inadequacy of drainage-tube or any other variety of hollow caoutchouc tubing. The solid cord which I used is to be obtained at most India-rubber shops, the only difficulty being to procure it of sufficient thinness; when thicker than I have mentioned, it is unmanageable, especially for small growths. When drawn out to four times its rest length, this cord is not more than half a millimetre in thickness, and the tension upon half a metre of it is 1230 grammes. It is thus admirably adapted for this gradual ligaturing and cutting process.

Having etherised the patient, I tightly ligatured the whole mass, tying the knot by the ingenious plan, devised by Sir Henry Thompson, of the addition of a series of ordinary ligature knots of sewing silk to prevent the India-rubber knot slipping. Immediately the temperature of the tumour fell from 98·3° F. to 96·5°. After a few hours it rose again slightly, and the tumour became very tense. The temperature then fell again rapidly, and the sphacelating process steadily continued until the growth became destroyed.

The pathological process thus occurring is plain, and it is in this that the great merit of the elastic ligature consists. When it is applied there is a brief interruption of all circulation, but quickly the arterial flow recovers itself, the venous system being still impeded. Great distension of all the capillaries naturally ensues, superinducing a condition of things resembling that which is the cause of varicose ulceration of the legs. The danger of the process (I speak solely from theoretical considerations) appears to be the presence of a mass of gangrenous organic matter immediately opposite a granulating and highly absorbent surface. For this reason in our recent case we kept outside the India-rubber ligature a loose skein of cotton yarn, about the same thickness as the ligature, and well soaked in carbolic oil. This was renewed three times a day, and a piece of lint kept well wetted with a permanganate solution prevented any offensive odour. On the third day it became evident that the ligature was no longer acting, and on examination it was found to be quite loose. You may remember the simple way in which this difficulty was combated. A slender rod of ivory, one millimetre in thickness, was passed through the ligature circle and twisted round a few times, in the manner in which a ready tourniquet is made from a handkerchief tied round a limb with a stick twisted through it. These ivory rods can be had of various thicknesses in the shanks of ivory crochet-needles. Ivory is, I think, the best material, on account of its strength, lightness, smoothness, and rigidity; and by this simple means ligatural pressure can be kept up to the very last to any degree of tightness required. On the fifth day the mass dropped off, leaving a surface which healed up kindly; and, with some constitutional treatment for the syphilitic taint, the patient made a good recovery.

I would particularly impress upon you that at the time of application, and while it is cutting through the skin, the ligature is painful. For this reason just before commencing, we injected hypodermically a quarter of a grain of acetate of morphia in the neighbourhood of the operation, a procedure which much assisted the anæsthesia, and reduced subsequent local pain, by keeping the patient throughout the whole day of the operation in a drowsy, quiescent state.—*Lancet, March* 7, 1874, *p.* 331.

69.—ERECTILE TUMOURS TREATED BY CROTON OIL.

According to Le Progrès Méd., 28th November, several physicians, among others M. Lafargue, have employed croton-oil in the treatment of erectile tumours, and we have already referred to a case by Dr. de Smet reported in the Presse Méd. Belge. In response to the inquiries of several correspondents we add the following particulars of Dr. de Smet's interesting case:—A patient, aged five, presented below the right lower eyelid an erectile stain of little extent surrounded by a zone the size of from 15 to 58 millimètres, in which were numerous small superficial vessels. The stain and the zone united are rather larger than a piece of 50 centimes. The centre is slightly salient. After some fruitless attempts (vaccination and repeated instillation of perchloride of iron) Dr. de Smet had recourse to croton-oil, and he thus describes the method used: Some fifteen needles were fixed in a piece of cork and the points were free for about 2 millimètres. These points were thus disposed so as to represent as well as possible the shape of the spot and the direction of its principal vessels. This being done the cork was dipped in croton-oil and then accurately applied over the spot and quickly pressed upon it. The inoculation, painful for the moment, only left after it a slight sensation of heat and pricking. A little cotton-wool was used for dressing. Next day the parts were swollen, and there was a slight vesication, but no pain. On the second day crust surrounded with little vesicles and a certain number of the vessels had disappeared, others were stopped up by clots. Another inoculation of croton-oil. In four days there was no trace of the nævus, nor any scar left.—*Medical Press and Circular, Dec.* 10, 1873. *p.* 530.

70.—A CASE OF SUDDEN DEATH QUICKLY FOLLOWING THE INJECTION OF PERCHLORIDE OF IRON INTO A NÆVUS.

By W. B. KESTEVEN, Esq., London.

Fatalities are often more instructive than successes in surgery. They point to sources of danger to be avoided, and compel us to cast about for conditions of safety. For these reasons I have thought that it might be advisable to add the following to the list of casualties that have been recorded in connexion with the treatment of nævi by the injection of perchloride of iron.

On April 3rd, 1873, I injected with perchloride of iron a nævus on the head of an infant aged nine months. The nævus was of a circular form, was situated on the top of the head, over the upper border and middle line of the frontal bone, and was about three-quarters of an inch in diameter. No ill effects followed, and the operation was apparently successful. In about three months afterwards, however, a re-appearance of the growth began to show itself, steadily increasing in extent, so that six months after the first injection it was determined to repeat the operation. By this time the fontanelle was closed, and the child in average health, save that it was excitable, and subject to child-crowing. On Oct. 4th five minims of perchloride were taken up in a graduated syringe with a screw piston, and, my son assisting me, three minims were injected ; the rest escaped from the wound. The child cried a good deal during the few seconds of the operation. In a short interval of time, it may have been a minute, it again began to cry, then suddenly turned pale and was slightly convulsed, at the same time that it began a series of eight or nine shrill laryngismal cries, attended with distinct struggles to recover its breath, which suddenly ceased in death. The whole time that elapsed from the first insertion of the needle to the child's death could not have exceeded five minutes.

In the Lancet for August 17th, 1867, Mr. Thomas Smith of St. Bartholomew's, has collected several fatal instances following shortly upon the injection of nævi with perchloride of iron. In these cases the nævi were situated upon some part of the face or near the veins in the neck. As the result of these fatal consequences, Mr. Smith concludes :—"Sufficient is known of the effect of the possible admixture of perchloride of iron with the general circulation, from injecting nævi on the face, to justify us in rejecting it as a remedy for nævi in these parts, unless, by pressure or by the employment of some instrument, the circulation in the growth is controlled, at least for some time."

In the above-mentioned case, the child, as already stated, had been the subject of laryngismus, in a paroxysm of which, doubtless, it died. I had no opportunities of ascertaining by post-mortem examination whether coagulation of the blood in any veins had occurred, but since no accident followed the former injection, and as the nævus was far away from the veins of the face and neck, I am of opinion that death in this instance was not the result of embolism but took place from spasm of the glottis, induced by mental emotion. A fatal result would, I believe, have followed had any other mode of operation been employed.—*Lancet, Feb.* 7, 1874, *p.* 195.

ALIMENTARY CANAL.

71.—ON HARE-LIP.

By Sir William Fergusson, Bart., F.R.S., Surgeon to King's College Hospital.

[Prior to Sir William Fergusson's investigation of this subject but little was known as to the anatomy of cleft palate. Few men could state positively, or could show by specimens, what was the nature of the substance of the intermediate hard material in the midst of the two gaps in the upper alveolar ridge, in a case of double hare-lip.]

The development of the teeth in these cases has, in my opinion, attracted less attention than in the normal condition of the upper jaw ; but it is less my intention to refer to such anatomical and physiological matters than to some points in surgery, which I think specially worthy of notice as part of the result of my long experience in such cases.

First, as to the date or age when operations for hare-lip are most eligible, there is no doubt in my mind that an early period is now mostly preferred The custom of delaying until after the completion of first dentition has now almost passed into abeyance, and the once prevalent idea that infants were specially liable to fatal convulsions after such operations may be said to have faded away. Experience has shown that they may be performed with safety at any period from a few hours after birth. Even at that dreaded period, first teething, harm is no more likely to happen than at any other time, provided always that the child be in good health. My own favourite date is from three weeks to three months after birth. Healthy-looking children at birth often pine and languish within the first few weeks, especially hare-lip cases, and particularly if the palate be implicated. I think it best to wait, in such instances, to see that health flourishes. A few weeks will give the requisite proof, and then for many good reasons, I am of opinion that an operation should not be further delayed. I have a strong impression, from my experience, that the older a patient is, the more is the effect of the operation perceptible on the constitution, bodily and mentally; and I am equally convinced that, if it be performed at an age when there can be no subsequent recollection of the circumstances, so much the better for all parties. The parents and others interested in the infant are almost invariably in an unhappy state of mind until the operation is satisfactorily over. Maintaining, as I do, that the younger the patient is after the first three weeks, the safer and better is the operation, I consider it disloyalty to surgery to delay such a decidedly advantageous proceeding on doctrines which will not bear the smallest scrutiny.

In the case of double hare-lip, with double cleft in the alveolar ridge, there may be great projection, or little or none. In the latter case, particularly if the columna and lateral portions of the lip be of good size, there may be no need for meddling with the intermaxillary mass. If, however, the projection be considerable, or what may be called great, and if the columna and side portions of lip be scanty, there ought then, in my opinion, to be no hesitation about taking away the projection at its junction with the vomer. The attempt to push this part back by gradual pressure is troublesome, or well-nigh impossible, in most instances, even if, as has been proposed, its narrow neck be broken. In either instance, it has never been told from experience in what condition, in what direction, the teeth come in the part thus displaced. In either or both instances, I have no doubt in my own mind that the teeth, if they came at all, would so slope backwards as to be of no value either for show or for use. There is, however, indubitable proof that, without thus meddling with the part, there are only two incisors of respectable size, after all, and these are of such indifferent quality, that they had better have been dispensed with at the earliest date. I, therefore, never hesitate to remove the intermaxillary mass when it seems the least in the way of a satisfactory operation. The advantages of doing so seem to me greatly to preponderate, and, if there be cleft hard palate at the same time, there is far greater chance, in after years, of the gap becoming narrower, whilst, in adult life, there will be greater facility for the assistance of the dentist. But, I imagine, there is less hesitation or difficulty in the surgeon's mind in the case of capacious double gap, than when there is only a single one with considerable projection of the intermaxillary margin. It is to such cases that the chief object of these observations is directed. If it be difficult to apply compression on the intermaxillary portion in double cleft, it is still more so when only one side projects; for its base is broader and firmer. The instances where there is no special projection are common, and require no comment, as there is then, as regards this matter, no obstacle to a saisftactory and successful operation; but, when there is a projection, if considerable, it is a more serious obstacle to these results than those inexperienced may imagine. I believe that this condition is a frequent cause of failure in the ordinary operation, particularly if it be done without the truss-compressor on each cheek to push the lateral portions of the lip towards the mesial line. In such a case, the surgeon is naturally anxious to leave the alveolar ridge untouched, and, in accordance with a common practice, when it is desirable to secure union by first intention, when the stitches or needles are withdrawn, strips of plaster are carried from cheek to cheek to hold the union firm. Scarcely a greater mistake can be made, for, the line of union in the lip being generally, under such circumstances, exactly over, or opposite to, the sharp angle of the projection of bone, the young cicatrix is pressed against it, and gradually thins away, until it is fairly split open, when the operation proves a failure. This, I have a strong impression, is an explanation of the failure of many cases that do not seem, in any special way, complicated. I do not mean that straps always conduce to this effect, and that, therefore, they should never be used. On the contrary, I have very frequently seen them of much service. But, if the single projection alluded to be conspicuous—in which case, there will always be a somewhat sharp angular margin—it is, in my opinion, best to get rid of it at the time of the operation. In my own practice, I was at one time in the habit of cutting the projection away with sharp small bone-forceps, dividing gum and bone at the same time, and aiming chiefly at getting rid of the projection. This usually involved all the intermaxillary bone on that side, and implied, perhaps, little heed of what damage might be inflicted on the sound side, although latterly, I always passed the blades into the mesial line between the intermaxillary bones, so as to secure this side from material injury. In the course of my experience, I fancy that I refined on this practice. I found that it was well to detach the portion as high up towards the nostril as could conviently be reached, and here I discovered that, in all young subjects, there was only cartilage to be divided. This could easily be done with the knife or scissors, and so, for many years, I haye used only one or other of these instruments. Usually, I have passed the scalpel through the mucous membrane, under the frænum, up between the bones, and divided the cartilage, periosteum, and gum to sever the part; and thus the use of cutting bone-forceps has been dispensed with, for, to say the least, such an instrument is coarse-like and clumsy in an operation for hare-lip on an infant only a few weeks old.

Whilst I can offer little objection to this proceeding, I fancy that I have recently fallen on one equally efficacious and void of certain objections which, I think, might be urged against it. Instead of this sweeping wholesale abstraction, I content myself with making an incision, vertical, sloping, or horizontal, with a scalpel through the mucous membrane and periosteum, over the projecting piece of bone; with a few touches of the knife, or a little squeeze with finger and thumb, I so separate these tissues as to permit the entrance of a gouge of a quarter or three-eighths of an inch in breadth, with which I scoop out the body of the milk incisor-tooth in as far as it is formed, taking no heed of the cyst or of that of the permanent one, and even cut out such wall of bone as may be there; usually, at four or eight weeks, only small plates of bone. In this way, the hard projection is removed, and the tissues that remain offer no obstruction to the union of the junction of the lip in front, whilst the operation, as it appears to me, is less destructive, therefore more conservative in character. There is thus left only the mucous membrane, with possibly some periosteum, which form a soft cushion behind the wound in the lip, and so the remaining intermaxillary bone is not divested of covering so thoroughly as when cutting instruments are passed in the mesial line to take all away on the offending side.

I have now adopted this plan in several cases, and have been much pleased with the effect and result. In one instance, I used a silk stitch to hold the edges of the wound in the mucous membrane together, but I doubt if it be needful, and I have not seen any spurt of blood from the deep part of the wound, such as that which I have often found, in other instances, has required the application of a pointed heated cautery. The wound has healed without attracting special notice. In the case where the stitch was used, the thread came away spontaneously, and, in the end, the gum appeared as if there had been no projection, and, therefore, no such operation.—*Brit. Med. Journal, March* 28, 1874, *p.* 403.

72.—ON CLEFT PALATE.

By Sir WILLIAM FERGUSSON, Bart., F.R.S., Surgeon to King's College Hospital.

It is now more than thirty years since I proposed the application of the practice of myotomy in the operation for cleft palate. Excepting in a few instances, objection has never been taken to the theory. There are instances where division of the levator palati muscles need not be resorted to, and union may with confidence be anticipated, but these are the exceptions; and doubtless it is owing to this preliminary step, that I can boast of a success well nigh equivalent to the usual result in hare-lip operations. Half-a-dozen instances of non-union are all that I can number, in between two hundred and three hundred operations. The result contrasts favourably with that of Roux, who, as is recorded, was well content with success in two out of every three cases on which he operated—amounting to about one hundred and twenty operations in all. My theory, I maintain, is as sound as that in the operation for strabismus; yet I congratulate those who have succeeded without its application, and I freely admit that there are instances where the operation may and does succeed without this preliminary to the ordinary process.

I claim to be the first in this country to have drawn special attention to Dr. Mason Warren's method of dealing with fissure in the hard palate. The proposal of that surgeon to peel the soft tissue off the bone on each side of the fissure in the hard palate has been extensively acted upon with varied modifications, but with very indifferent success. For myself, I may say that I have tried perhaps every plan that has been suggested, but I have been sorely mortified with the results. Some cases, after one or more operations, have turned out admirably; but in a large number the result has been such as to have made me despair of closing these openings by operative means. There are instances where such hopes should never be entertained, owing to the wideness of the gap and configuration of the parts; but there are probably as many as one half of the cases amenable to operation which have such slight deficiencies in the hard palate, that I have always acted on the idea that closure in the mesial line would or might be effected here as in the soft palate. Experience led me to give up the plan of Mason Warren of trying to close the gap in both soft and hard parts at the same time, even in instances where the defect in the bone was but slight. For many years latterly I have ceased to meddle with the front part of the gap where the bones have been implicated, reserving the attempt for a subsequent operation; but to facilitate the approximation of the margins of the soft palate, I have often followed the plan practised by Roux, of making transverse incisions, so as to divide the soft parts from the posterior margin of the osseous palate. Whilst I have had reason to feel satisfied with this step, I have in no way found my effort to close the hole in the hard vault more successful. From time to time, on subsequent operations for the express purpose of closing such openings, I have been highly satisfied with Mason Warren's plan of separation—that of paring the soft tissue from the hard, by working from the margins outwards to the alveoli, sometimes, in addition, making the lateral incisions proposed by Mr. Field of Brighton, or by making these lateral incisions first, and separating the soft tissues towards the mesial line. In some intances, I have had good results from a sort of gliding process, facilitated by separating the front end of one of the flaps from its connections; but, alogether, the success of these operations has not been equal to my anticipations or desires. Since using chloroform, I have been solicited by patients, again and again, to try further operations, but the results have led me to give little encouragement to such proceedings. Latterly, however, a dawn has opened on my hazy views in this respect; and my chief object in putting these observations on cleft palate together is to draw attention to my latest experience on the subject.

My impression is, that the frequent failure in this latter kind of operation is induced by the contraction of granulations, whereby the lateral portions are so drawn towards their original position, that the union in the central line is either prevented or broken, and thus the gap remains when the side-flaps have resumed their original positions. An idea came into my mind years ago which, however, I thought so wild, that I could not dare to bring it into practice. Repeated failures, however, by usual methods, brought it more forcibly into my thoughts, and at last I resolved to put it into execution. My project was that, instead of making the separation between the soft tissue and bony palate, for a quarter of an inch or so, I should divide the palate, soft tissue, and bone, about a quarter of an inch from the margin of the gap on each side, cutting the soft tissue in the roof of the mouth with a scalpel, and the bone, with mucous membrane above in nostrils, with a chisel, by means of which I could push the margins towards the mesial line; so that, having been previously made raw by removing the mucous membrane, they might be brought into apposition and held so by stitches.

This project was carried into execution, in King's College Hospital, on November 22nd, 1873, in the following manner.

J. H., aged 18, had the soft palate closed two years ago; a small aperture remained in the hard, which had been twice operated on unsuccessfully by the ordinary proceeding. The patient was placed under the influence of chloroform, and the mouth held open by Wood's gag. The edges were made bare by dissecting off the mucous membrane; then, by means of a small scalpel, an incision was made on each side. The back part of these wounds penetrated the soft palate, and in front they were close to the bone. Then the point of a chisel was forcibly, but carefully, pushed upwards through the bone into the nostril through each wound, and, by slight lateral movements of the blade, each lateral portion could be readily made to meet the other in the mesial line, whereby the raw margins made at first could be placed in apposition. The parts were then held together by a single stitch introduced in the usual way, passing, on each side, through the soft tissue, that it might remain steadily in one place. At the conclusion, the conditions seemed much as after the operations formerly effected. Subsequently, granulations filled the lateral gaps as on previous occasions; but, when the stitch was removed, union in the centre seemed firm, and was not disturbed by any dragging power, such as I supposed had, in earlier operations, drawn the flaps upwards and outwards towards the bone. The result was perfect, and I have heard nothing to the contrary since the patient left the hospital. On the same day, I repeated similar proceedings on a patient who had had a successful operation on the soft palate, but on whom I had been unable, after four different attempts, to close a fissure in the hard. Here the opening was larger. Similar steps were followed, and, to all appearance, during the first eight days, with similar results. Two stitches were employed, but, on removal, the edges, where one had been, seemed to fall asunder. Two days afterwards, a new stitch was put in, by means of an ordinary aneurism-needle, and the margins made raw by a gentle scrape. The hole was thus closed; and, on removal of the stitch, eight or ten days later, union was perfect. Since this patient left the house, a small separation has taken place, which has left an opening a little larger than the point of a probe. I have recently had made, by Matthews Brothers, a modification of a most ingenious needle suggested long ago by my friend Mr Brooke, of Westminster Hospital, for introducing stitches into the soft palate in operations on cleft in that part. The point of his needle was sharp; and I have often used it with much satisfaction. The point is blunt, like that of a common aneurism-needle.

Soon afterwards, a third case came under notice in private practice, wherein, after closing the soft palate, I had failed to succeed with the hard, by the ordinary proceeding. Here the opening was midway in size between the cases above related. The steps were much the same as in the first, excepting that I passed the threads through the lateral apertures without fixing it in the soft tissue. After the knots were tied, they never slipped from their position. These were left some days longer *in situ* than in the previous cases, and on removal, union in the mesial line was perfect. This union, I have recently heard, remains perfect.

A fourth case has recently come under my care at King's College Hospital. The gap here was larger than in the three preceding cases. It was an inch long, by more than a quarter of an inch wide. The lateral incisions were of proportionate length, and when the parts were brought together, a gap into each nostril was visible. Here three stitches were used, and, as in the case last related, they were passed through the side incisions, without piercing the soft tissue.

A fifth case is now under notice. Here the ordinary operation was performed on the soft palate in June, 1873; and the front part, which involved a small portion of the hard palate, was left untouched, in the conviction that it would be better to leave it alone until a future date. The aperture left was at the end of the healing process, of this size. Here the scalpel and the chisel were used as above described, and the parts were satisfactorily placed for union in the mesial line.

From the experience already obtained, as also from careful consideration of the subject, I feel justified in calling attention to this addition to what is already more or less familiarly known to those who have studied this complicated subject. At first thought, several strong seeming objections arise. It might be doubted if the osseous palate could be cut and moved in the way described. If an experiment were made on a roof of bone in the natural condition, it would be impossible; for, if the chisel were pushed through the bone a little on each side of the mesial line, it would be impossible to move the intervening portion towards the middle line, because the space is already filled up. This objection does not hold, however, in the malformation; for the space is not filled up, and the vacancy permits the osseous margins to be approximated. Then, division of the bone on two sides, and breaking the front part of each, seems so rude, rough, and destructive, that the idea arises that caries or necrosis might ensue. But in reality the process is, in my opinion, less hazardous than when the flaps of soft parts are dissected or forcibly pushed or drawn off the bone. Besides the fact that these parts do not always unite, one flap or both will occasionally slough, and so things are rendered worse than ever.

In favour of the proceeding about which I now write, I can state from experience that the loss of blood is much less; and on that account there is less trouble in mopping out the pharynx, and consequently less hazard in the use of chloroform; that the tissues on the lower surface of the bone are not so much disturbed or divided as by the older process; that the periosteum and their tissues below are less disturbed than otherwise; and that, from all these circumstances, there seems less risk of sloughing of the semi-detached parts.

As to the healing of the lateral wounds, I am of opinion that the gap will invariably be closed, and that in the bond of union there will be a firm cicatrix of soft tissue, and possibly bone; that the vault of the mouth will be as firm as in a normal condition, and probably firmer than where a fortunate result has followed the process of Warren, Field, or Langenbeck.

These observations apply solely to instances where the surgeon, in a first operation, has not attempted to close the gap, or has failed in a design to secure union throughout. They will apply, however, with equal, even greater, effect to instances where heretofore, in my own experience, I have left the front part of the cleft untouched. I remember scores of instances of the kind, wherein I now feel confident the whole gap could have been closed by the process above described, with, as I imagine, as much success as attended the operation on the soft palate. In cases of defective hard palate, after the usual preliminaries, and having bared the edges of the gap in the front, in hard as well as in soft, I would now make the additional wounds with scalpel and chisel, as above recommended, thereby hoping to avoid a second operation by doing all at once, under the beneficial influence of chloroform.

Since the above was written, I have had an opportunity of putting these latter views into execution. A youth of eleven years of age, on whom I had operated soon after birth for wide hare-lip, was brought under my notice, with a fissure extending from the lip to the uvula. A year ago, I had advised delay of a proposed operation on the gap in the soft palate, which was very wide. Now, I thought I should venture to test the proposal in an unusually severe case. Under chloroform, the usual incisions for dividing the levator palati on each side were first made; then the edges of the gap were pared as far forwards as to within half an inch of the front; next the scalpel and chisel were used in the way described; and finally, six stitches were introduced—two through the hard part, without piercing the tissue; and four through the soft parts, in the usual way.

Unfortunately, the first two stitches slid together, so that practically they acted only as a single one. The effect was, however, highly satisfactory. There was *no* tension in the front of the gap in the soft palate. The approximation of the bones took off all drag, such as is common under ordinary circumstances; and the result has been equal to my anticipations. The whole of the gap in the soft palate has united, and even a considerable part in the hard. With another operation in front—a repetition of this new process—I am convinced that the gap may be closed up to the alveolar ridge. I have no doubt whatever that, in instances where there is only a slight fissure in the hard palate, the surgeon, by use of the chisel, will thus be enabled to close it, and that in the soft with a certainty hitherto very questionable.

In my anatomical knowledge of such malformations, there is one condition which I fancy might prevent these views being carried into effect. In the generality of severe cleft in the hard roof, the lower and back part of the vomer is incomplete; but in some cases the vomer is entire, although perhaps swayed to one side; and it remains attached by its lower margin to one side of the hard palate throughout. Here would be a difficulty which might puzzle; for, although the chisel might permit the approximation of the edges, it would be difficult to introduce stitches; but even here there would be a redeeming quality in the new process, for pushing the parts to the mid-line might lay them so together that, though union should not occur, they would lie so close that a fissure might elude ordinary observation. In one of the cases above recorded, I have found that entire union in the mid-line has really not taken place. The margins are, however, in such close approximation, that the want of union can be ascertained only by slipping the point of a probe through the suspicious-looking part.

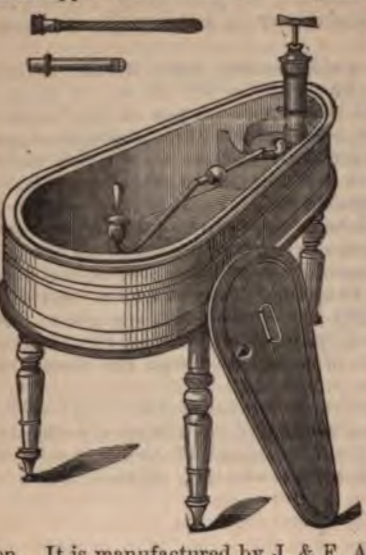
With these remarks, I leave the subject for the present, under conviction that I have touched new ground in a most complicated field of anatomy and surgery, and in sincere hope that I have added to, if not completed, the power of surgery in such cases of malformation.—*British Medical Journal, April* 4, 1874, *p.* 437.

73.—UMBILICAL HERNIA IN INFANTS.

By John Gorham, Esq.

An experience of forty years of active practice enables me to speak very decidedly as to a plan of treatment which has been adopted, and in every case with uniform success, so that I am not able to point to a single instance in which it has failed. I am induced therefore to request an insertion of the plan adopted in the pages of the Medical Times and Gazette. It is as follows:—Having placed the child on its back in the mother's lap, the hernia is pushed in with the forefinger. The skin of the abdomen on either side is now pinched and folded up with the finger and thumb, and brought over so as to make both folds meet. The hernia now lies underneath the folds, which are represented by two parallel lines in contact with one another. Holding the folds in close apposition, by applying the thumb and forefinger of both hands above and below, the mother or an assistant now applies six strips of diachylon plaster—four transverse, two oblique. The transverse are three inches long by half an inch wide; the oblique are four inches long by half an inch wide. The small belly-band which has been used from time immemorial is now applied, and serves to keep the plasters in their place. These plasters will remain on, although the infant is washed well every night and morning, for four days, when they can be removed, and fresh ones substituted. It may be as well to notice, however, that the whole of the strips should not be removed at once, but one by one; and as fast as one strip is removed another fresh one should replace it. In this way the chances of protrusion during the crying or coughing of the child are much diminished.—*Medical Times and Gazette, Dec.* 6, 1873, *p.* 648.

74.—IMPROVED ENEMA AND DOUCHE WITH BIDET.



This apparatus is made of strong tin, mounted on a stand, and enamelled white inside; the outside being enamelled in imitation of wood, marble, or china, as desired. The back part forms the bidet, upon which the person using it is seated; the back forms a compartment for holding warm or cold lavements or injections, and in which the pump is fixed. There is also a silvered jar for medicated lotions or enemata. Both pump and jar can be instantly removed for cleaning, by lifting out the small band round the pump by which it is held firmly in its place. The tubes from the pump are jointed in such a manner that they can be raised or lowered at pleasure; and each of the injection tubes can be reversed from the end to the middle joint, or *vice versâ*, so as to serve every form of application, for male or female use. It has an elegant appearance placed in any bed- or bath-room, and is most simple in construction. It is manufactured by J. & F. Allen, 64 and 65, Marylebone Lane, London, W.—*Lancet*, *Feb.* 14, 1874, *p.* 235.

ORGANS OF URINE AND GENERATION.

75.—ON THE FUTURE OF OPERATIVE SURGERY FOR STONE IN THE BLADDER.

By Sir HENRY THOMPSON, Surgeon Extraordinary to the King of the Belgians, and Professor of Surgery at University College Hospital.

I commence then, boldly, by saying that I have come to the conclusion that stone in the bladder, like many other maladies, is an exterminable one. I believe that this grave malady, which has tested surgical skill during two thousand years and produced a ponderous literature, which has been dreaded beyond all things by mankind, and has been the source of untold suffering to thousands in every age, is nevertheless a malady which may be exterminated; so far, that is, as it is a painful and dangerous one.

It is my business to-night to deal with stone in the adult—the largest and most important part of our subject; and in speaking of what lithotrity can do, although it is not absolutely powerless for children, being only useful for very small stones, I beg you to bear in mind it is solely a question now of stone in the adult, and that necessarily means stone in the adult of advanced age.

To pursue my subject, I place before you four trays from my cabinet, holding about two hundred calculi removed by lithotrity: the mean age of these two hundred cases is upwards of sixty years; there are very few below forty, many between seventy and eighty. To be exact, there is a tray of sixty-three rather small stones, two trays containing ninety-nine stones of middle size, and a fourth tray containing thirty-five stones of rather larger size; in all, one hundred and ninety-seven. I could have brought you many more, but these will suffice for my illustration, containing as they do several remarkable individual specimens.

Let me now consider the single question—What is the object we propose to attain in lithotrity? I answer, in brief, to reduce the stone to such small fragments that they may pass easily by the urethra, and to do this by exerting as little mechanical action as possible.

We are to aim at applying force to the stone in such a manner as not to injure the delicate canal which must be traversed, and the sensitive organ, the bladder, in which it lies. And the force should be so applied that the minimum of irritation should accrue from the fragments that are made. In fact, the sources of danger in lithotrity are but two in number: injury to the soft parts by the instruments employed; and injury to those same parts from the sharp edges and angular forms of the fragments which are produced by the process. When we have reduced the mechanical action to the minimum of capability to inflict mischief, and have learned to make fragments in such a manner that they shall produce the least amount of irritation, why then we shall have arrived at perfection in lithotrity. Hence it is that I have always endeavoured to work with the fewest and most simple instruments possible, and to reject preliminary injections always, and subsequent washings-out as far as possible.

How far have we solved the problem in question? I answer, perfectly, in relation to calculi of a certain size. With a calculus of certain weight and dimensions, whether of uric acid, phosphates, or oxalate of lime, say not exceeding the volume of an ordinary nut, a perfect result may be ensured. I call your attention to that tray containing sixty-three stones, and in which, let me remind you, the patients were of a mean age of over sixty years. There was not a single death among those cases. The size I name was not exceeded. And I assert that I have never yet had a death following the operation of lithotrity in which the stone was within the limits of that size. Nor have you any right to expect anything but success with such stones, requiring, say, two sittings, or perhaps three at the most, if you only take care to act with the utmost gentleness.

So far, then, is the problem solved, and triumphantly. But it is another thing when the stone much exceeds these dimensions, and where, not two, but five sittings are necessary for its removal; and, *a fortiori*, danger still increases when the requisite number of sittings rises to eight or ten.

In the next two trays are about a hundred stones of the middle size. At this size, which is that, say, of an almond in its shell, the result is still most excellent, far superior to that of lithotomy, but not a sure success as before. Hence a certain small proportion of deaths was met with—about one in twelve or thirteen cases.

In the last tray of larger stones, the mortality was more considerable, perhaps one in eight or ten cases. With regard to some of the latter, it is quite possible that some of them would have been better cut. No man is always wise, and errors of judgment must be admitted; he is the best who makes the fewest, and marks well his errors, when they are made, for future guidance. After the event, also, with the fresh light of the past on the case, it is easier to know what should have been done. Then there must always be a number of cases in which the choice of operation hangs on the slightest circumstance. There is a wide border-ground, so to speak, which is common to both operations. No man, whatever be his experience, can lay down a hard and fast line, and say, "On this side lie the circumstances which determine lithotrity, and on the other side are the circumstances which make lithotomy imperative." The border-ground is much broader than I often like, and there are a good proportion of cases in which it is impossible to predict which operation of the two will be most successful: stones which are just large enough to be crushed, which are certainly not outside of the limits of size for lithotrity, and in patients of a certain class for *whom a half-dozen sittings* will not tell heavily. On the other hand, the *crushing of the same stone* in the sensitive patient with a worn-out nervous system may make too large a demand on his endurance. I do not *know* that. I only say it may; for, after all, the hale and hearty country squire, "who has never been in bed a day in his life," and who never ailed anything until he had a stone, often bears confinement and attacks of irritation less easily than any other patient whatever. Indeed, it is most difficult sometimes to decide; and sometimes the patient who is approached from sheer necessity, with fear and trembling for the consequences of interference, turns out the best of all, and astonishes you with his capacity for getting well. Thus you see the only certainty of attaining success lies in getting the stone to operate on when it is small. And success being absolute then, as I think I have been able to prove beyond all manner of doubt, it follows as a matter of necessity—

That the diagnosis of the presence of stone in the bladder and of its size is a matter of the highest importance.

I affirm that it is not less important to be capable of finding a stone when small and determining its size, than it is to perform the operation properly afterwards. I might almost go further: I think I shall find you agreeing with me before I conclude, that the diagnosis I speak of is the more important matter of the two; and that I may venture to say that I would rather, for the sake of calculous patients at large, and for the future of lithotrity, have keen diagnosticians than expert handlers of the lithotrite, if I could not have both. Because, as we shall see, all progress depends on the *early* diagnosis; for when the stone is really small, no man worthy of the name of surgeon, with a fair experience of instruments in the bladder, will fail to crush it safely. You see it is lithotrity that has brought this question of diagnosis home to us. When there was but one mode of removing the stone—when it was necessary to cut from the perineum to the bladder for every stone, no matter how small or how large, it mattered very little whether we made an exact estimate of its size, if we only were quite certain a stone of some sort was there. What, again, did it signify whether it were mulberry, uric-acid, or phosphatic in its character? It was nothing to the lithotomist whether one would crush easily and the other with difficulty or not at all.

Nor is there any real difficulty about making the diagnosis on which I lay so much stress. Nothing is more easy, as I shall soon have to show, if only you follow the right method. Granting me this, and the unrivalled success of lithotrity for small stones, already proved, it logically follows—

That the operation of lithotomy must in future be rejected for all stones which are of moderate size.

Now this is a most important fact, and it is one, I believe, which has not yet received its full consideration from the profession. For it follows, further, that all those attempts which have been made during the last fifty years, and may still be making, to perfect lithotomy for small stones are useless and obsolete. We cannot require an operation by cutting for small stones. And I am quite safe in saying that the results of that tray, with sixty-three cases of elderly adults without a death, never have been, and cannot be, equalled by any cutting operation whatever.

But it may be said that there are still some exceptional cases of small stones to which lithotrity is not applicable; such, for example, as cases of narrow stricture, preventing the introduction of a lithotrite into the bladder. Perhaps there may be such, but I have not yet lived long enough to find one. During the last few years I have crushed several cases of small stones in presence of narrow stricture, and in this manner: nothing is easier than to dilate temporarily any stricture if you only permit a gum catheter to remain lying in the urethra a sufficient number of days, and this is what I have done in the wards of University College Hospital with the best result. I tie in a very small catheter, and having arrived, after a few days, say at about No. 9, the patient is submitted to chloroform, the instrument withdrawn, a small lithotrite introduced two or three times, the stone crushed, *débris* removed, and the catheter is replaced. In three days or so the process is repeated, and so on until all is finished. There is no difficulty in thus dealing with a small stone, however resilient be the stricture. Such cases, happily, are very rare, but when they do occur such an operation is usually preferable to lithotomy, for, I repeat, a small stone.

Well, then, these matters being held as proven, lithotrity being answerable of the successful issue of all cases of stone of moderate size, it follows that there are two desiderata, and two only, to bring us to the perfect practical solution of the great question.

1. We want the best operation for the removal of large stones from the bladder.

2. We want the best method for discovering the existence of small stones in the bladder.

Now, the first I do not intend to discuss at length here; that would not be within our limits. I shall assume that for most cases the lateral operation is probably the best. It is no new question, and need not come up again here. But the second is, I declare, a new question. It may seem trite to say so, but practically, I repeat, it has engaged no one's attention at all adequately—that is, compared with its surpassing importance. I find people again and again with small stones in their bladders which have been overlooked. Not because any one is to blame for not finding them; first, because the early signs of stone, the evidence of the presence of small stone, have not been sufficiently considered and taught; and, secondly, because the paramount importance of the discovery did not exist until the power and value of lithotrity in these cases had been demonstrated. Why, I have myself witnessed the sounding of a patient, and heard the result stated in such terms as these:—"I am glad to tell you that you have nothing considerable in your bladder; there may be a small stone perhaps; just possibly, but you have nothing considerable there, so all is well." The patient is congratulated because he has not a stone as big as a hen's egg! But suppose there should be one the size of a bean? Why then the discovery of that stone would actually be a fact of greater importance to that man than the discovery of a big one; because, in the case of the large stone, there is but one thing that you can do, and the issue is doubtful; but find that man's stone when it is small, and you are *certain* to save him. This is a matter I cannot exaggerate the weight of. And it is by following the clue that this gives us, and no other, that we shall arrive, as we certainly shall some day, at the end I have in view—viz., the extermination of stone in the adult.

This brings us to the practical mode of looking for the stone early, and discovering its presence when small. How is this to be done?

First, respecting the mode of sounding. It is absolutely essential to employ a light sound, which can be easily turned in the bladder and urethra. Nothing but a quick and delicate movement will elicit an audible note, or produce a sense of contact, with so small a body as a pea lying in the interior of the bladder. Therefore it is better to have an instrument which will roll easily between the finger and thumb, and not require the wrist or the arm to create the movement. Hence the handle should be cylindrical, like the handle, but smaller, which I originally designed for my lithotrite, and which is now much used here, and is almost universally employed abroad. The beak should be very short, so as to be turned with the utmost facility. To find a small stone the bladder should be empty or nearly so. I prefer a patient to make water a few minutes before sounding, and certainly never to be injected or prepared in any way, which only tends to defeat our object. Let him lie down, with his pelvis a little raised, and then let the instrument gently glide down the urethra; it is five to one, however small the stone may be, that it is just grazed as the beak passes through the neck of the cavity into the bladder. This is perceived easily if the sound is only guided lightly by the faintest touch of the finger and thumb. Held and guided by the wrist and arm so slight a graze may be unnoticed. If not felt, let the sound make two or three quick semi-rotations right and left; if still nothing found, depress the handle slightly to turn the beak below, close to the neck of the bladder, and make two or three similar movements there. That is *where the stone will be found*, if there is one, in the nearly empty bladder; *and in the same manner* a small fragment will be found at the close of a case.

Now look at the sounds which were formerly used—heavy, large, with considerably curved and long ends, like those of an ordinary catheter or nearly so. No doubt you might find a large stone with such a tool, but a small one never, except by accident. I have repeatedly demonstrated, not only here, but in Paris, the presence of a small calculus, or fragment smaller than a pea, by producing an audible note from it by means of the small sound with cylindrical handle, passing a lithotrite to the spot immediately afterwards, and withdrawing the bit uncrushed as a proof. Pray permit me to say that I don't adduce this as any instance of fancied dexterity on my part—far from it; for you see it would defeat my very purpose to do so; it is merely an illustration of what is possible equally for you as for me, if only you pursue the rational system of seeking the small stone with an appropriate instrument. If it be not so I will sit down and give up my case in favour of lithotrity, because it is altogether a worthless operation if it is not capable of removing the whole stone. If lithotrity means only crushing a stone to pieces, and does not mean also making sure to remove the last, then by all means let us do lithotomy, and nothing else but lithotomy. Lithotrity is quite equal to its object in nineteen cases out of twenty. I do not mean to say that a case will not now and then happen in which the last fragment will not yield to our search, and may give some trouble. In all operations, however perfect, some *contretemps* may present itself. But I contend that it is very rare to miss the last piece. Seek it in an empty bladder with such a lithotrite as this; no danger whatever to the bladder exists. It is useless to seek it in four or more ounces of water, which is simply "looking for a needle in a bottle of hay;" currents and counter-currents are produced in all that water by the opening of the blades and the action of the bladder, which keeps your little bit in a state of constant movement.

Secondly, the early signs and history of vesical calculus are to be carefully noted. This brings us to what appears to me a very striking and suggestive question,—How is it that the existence of a calculus in the bladder, a product almost always of slow growth, and giving abundant signs of its presence, can ever attain anything beyond a certain size without being discovered? That it does so is too true; but that it should ever grow to be of large size is to me astonishing. I assert that more than half the stones I operate upon are found in cases in which no suspicion as to the real malady has arisen until the sound has been employed. Now, with the utmost deference to others, and only after the acquirement of a profound conviction on my part, I venture to say that it is my belief that the early signs of calculus are not generally sufficiently recognised. In the whole course of my experience I have not met with more than two or three cases in which the obvious early signs of calculus were absent. To me they appear quite unmistakable. They may be present, or nearly so, in cases in which no calculus exists; but when these signs are present, then always ought the sound to be used. So far have we been from recognising who are the real calculus patients that we find it stated in our classical works on the subject that stone is most prevalent in children. Not at all. Stone is uncommon in children compared with its frequency among elderly adults. I am quite aware that, in most large hospital records, half the entire number of cases are found below the age of puberty; and it is that which leads to such surprising statements about the small mortality of the lateral operation as I have before alluded to. But then among the poor, stone is comparatively frequent in children. Among the well-to-do it is very rare to find a juvenile case at all. The latter class, however, furnish it abundantly at the other end of life, and here it is that the bulk of stone cases is to be found.

And what is the ordinary or typical history of a stone case? I speak, as anyone, of course, may understand, of uric-acid and oxalate of lime. The phosphates are mostly of local origin in a bladder incapable of emptying itself, and belong to another category. You will find a healthy-looking man with good family history as to longevity, but mostly tainted with gout, one or two cases of it existing, antecedent or collateral, or in its absence some record of gravel or stone in an ancestor. At middle life he finds uric-acid in his urine, as a brickdust deposit, more or less persisting. Soon after, a small bit of gravel passes, with or without a marked attack of renal pain; if the latter, he is at the time much relieved by medicine; but often no special treatment or regimen is adopted at this critical point in his life to check the tendency now fully developed; so, after an interval, another and another pass, and then no more for a few months; and although some little suspicious symptoms appear, they are thought very lightly of, especially as he has not during the last nine or twelve months passed any gravel as he used to do. Whereupon he congratulates himself and is congratulated—not prudently; and the suspicious symptoms are credited so often "to that little weakness of the bladder which all people have as they get onwards in years." Delusive axiom. But what are these suspicious symptoms? Not very marked, but ample to render almost certain to the experienced observer that that interval of freedom from passing gravel only marks an advancing stage of the malady, and shows that the gravel is now too large to pass the urethra; and that it is in the bladder and is growing by accretion, consuming surplus uric-acid for that purpose day by day. For on inquiry you will find that the frequency of micturition is greater by day, during movement, than by night, during rest—condition altogether contrary to that "weakness of advanced years" (prostatic hypertrophy) when the frequency is almost always greater by night than by day. You will find a slight pain—a mere passing sting—is mostly present at the close of the act of micturition and in the end of the penis, while in the "weakness" the pain, if any, is from distended bladder, before micturition, and is relieved by the act. Next, it will probably be ascertained that some day lately, after an unusual walk, or it may be after an hour or two in the saddle, a little blood was observed in the next urine; soon forgotten, or, if named, was followed by the recommendation not to do it again, but which unhappily aroused no suspicion of the true cause; so, not being done again, the occurrence did not reappear, which again comforts everybody. Well, after listening to such a history, I am always morally certain that one or two small stones exist, and of course the sound is introduced at once, and almost invariably a small stone or more are discovered. No anxiety need arise, and the patient may now with reason be congratulated, since a small stone is certainly the safest solution of his symptoms; for, as I have already said, the malady for the most part occurs in people of otherwise good health and strong constitutions. I have no hesitation, then, in saying that from fifty-five to seventy-five years of age is the favourite term for calculous diseases, at all events in this country.

I now think I have fairly proved that the operation of crushing the stone is safe and successful for all small stones, and I think I have also proved, or have gone far to do so, that a stone may be always found when it is small. It follows then, if you admit these things, that lithotrity must be the future operation for calculus in the adult.—*Lancet, Dec.* 20, 1873, *p.* 867.

76.—ON THE DETECTION AND REMOVAL OF VESICAL CALCULI.

By William Donald Napier.

[Three points with regard to vesical calculus are obvious. 1. The difficulty of early diagnosis. 2. The danger to life involved by the operation of lithotrity as now practised. 3. The impediments to convalescence consequent on its successful performance. The instruments described by Mr. Napier are we think of great value, especially as by their simplicity of mechanism and manipulation, they will do much to ensure efficiency in the hands of practitioners who have not had any very special experience.]

To render a doubtful result to investigation conducted with ordinary care more exceptional than hitherto—and I imagine that it is in the first stages *of the disease that the* percentage of uncertain issues is by far the greatest —*it seemed necessary to* devise an instrument of peculiar sensitiveness, and capability of application to minute objects. Whether the one before you will fulfil these conditions in all instances, remains to be proved. It has not failed me yet in the experiments to which it has been subjected. I believe it to be unique in its mode of action, depending, as it does, upon ocular instead of oral proof; and I cannot help hoping, that, by reason of its exquisite susceptibility of impression, immediate manifestation of the existence of a foreign body may follow with absolute certainty its introduction into the bladder.

The Calculus Detector, for that is the name I have given it until a better can be found, is precisely similar in form to the ordinary sound. It is composed of steel up to the commencement of the curve, where it is reduced in diameter, and a coating of pure lead is cast round the smaller part, of sufficient thickness to render the surface perfectly smooth and even along the entire length. The leaden extremity is then polished as highly as possible with a leather and rendered keenly sensitive to the slightest contact with any rough or hard substance, of which it would be now found to bear visible trace. On some of these instruments I tried to increase the susceptibility by dipping the leaden end in a weak solution of nitrate of silver, or what is better, chloride of platinum, which produces a black pigment on the surface; but further experiments have almost convinced me that this preparation is unnecessary, and that the pure lead is perfectly efficient. The instruments that I showed before were coated with pewter, on which the use of lead is a great improvement, by reason of its readier impressibility.

Before the introduction of the detector, it should be carefully rubbed with a leather, and submitted to the test of a magnifying glass, so that no slight scratch or indentation previously incurred should, on its withdrawal, mislead the operator, who will find it satisfactory again to apply the same means for discovering the marks which contact with a hard substance may have produced during its employment in the bladder.

With regard to the other instrument, which I call the Calculus Extractor, I may premise that I incline to confine its use to the removal of small stones—I trust that the day of large ones is at an end—and fragments of stone; and this because I do not myself believe that lithotrity should ever be resorted to when a stone has from any cause, such as absence from available medical assistance, or the dread of the sufferer to submit to the necessary test, been allowed to attain to large dimensions. Then the patient's chance for recovery must lie in lithotomy.

To be as concise as I can, I shall simply describe it as an instrument intended to withdraw from the bladder either a stone intact, or crushed into fragments, so enveloped in a delicate elastic wrapper—I use the term advisedly, for as you will see, it literally folds itself round the substance in question—that injury to the passage will be simply impossible. It consists of a soft India-rubber tube, terminating at one extremity in a funnel-shaped orifice, greatly resembling the ordinary convolvulus flower, and it is capable of containing a calculus or portion of calculus of considerable dimensions. When it is inserted into the bladder, its form and position render it the receptacle of such foreign substances as would from the position of the patient and the flow of urine determine towards the natural outlet. This receptacle, enclosing such bodies in the folds of its delicate membrane, and permitting itself to be gently withdrawn, retains them in its elastic envelope, and protects the urethra from direct contact with them.

After bringing it to the state of completeness in which you now see it, I was very much and very long perplexed as to the best means of introducing it into the bladder; and it was only after many trials and as many failures that I was able to see my way to the accomplishment of this, without which my instrument would be useless.

My first idea had been to insert the convolvulus-shaped cup folded in the form and manner of a bud, and retained in its position by some adhesive substance that should permit it to expand after its introduction. But there were several objections to this. At last it struck me that, inserted in a fine silver cannula, it would be entirely at the direction of the manipulator; and I was successful in reducing my idea to practice; but there I was met by a difficulty that I feared at one time would be insurmountable. How was it possible to avoid the trouble to which the insertion of a cannula open at the end could not fail to give rise? An artificial point appeared to be the only solution; and it seemed to me that cocoa-butter was possessed of a peculiar fitness for the purpose; for this reason; for a hard dense substance, its dissolubility under moderate heat is, I believe, quite without parallel. Subjection to a temperature of 96 degs. dissolves such a portion of it as my purpose requires, with curious rapidity—in as nearly as possible one minute. There is besides the additional advantage that it serves as a lubricator in its passage to the bladder. But it is not in connection with my instrument alone that this application of it will, I hope, be found to be a profitable discovery, but in every operation where it is expedient temporarily to close the mouth of a catheter or cannula. I mean that I consider the cocoa-butter point *per se* worthy of attention.

I would now briefly suggest to you a further adaptation of the principle and form of the Extractor that may be found useful. As a self-retaining catheter, I cannot help thinking it would prove as simple and effective an appliance as could be used; and I know that the increased demand for these instruments of late has called forth various improvements and emendations in the means both of introduction and of retention.—*British Medical Journal, Dec.* 20, 1873, *p.* 720.

77.—MR. GUTTERIDGE'S OPERATION OF STONE IN THE BLADDER.

Mr. Gutteridge's scheme consists of two parts, viz.—to use his own words, —"of a due combination of incisions of parts ascertained by extended experience to be capable of being safely severed; and of implements with which the successive stages may be most surely and with least danger accomplished." The patient was a boy sixteen years of age, a native of the Black Country, and was submitted to operation without being anæstheticised, as Mr. Gutteridge has a strong objection to chloroform or ether in lithotomy. Kneeling before the patient when in the lithotomy position, and with his eye thus in a line with the perineum, the operator first passed the staff, which he handed to his assistant; he then made very firm pressure with the fingers of his left hand upon the anus, thus drawing the skin of the left side of the perineum, with the lower extremity of the gut, well over towards the right tuber ischii. He next rapidly plunged the scalpel into the groove of the staff at a point a little to the left of the raphe, and on a level with the anterior extremity of the tuber ischii, and then, without running the scalpel along the groove, he made it cut its way through the soft tissues. Next he passed the beak of the cystotome into the roughened groove, and, with the cutting edge directed a little upwards from the horizontal direction, he passed it onwards through the prostate and then turned the edge downwards and brought it out at the lower extremity of the skin-wound. This done, he passed his left index-figure into the bladder, felt the stone, then withdrew the staff, and passed in along the finger a grooved conductor, much like a narrow blunt gorget, and along this, after withdrawing the finger, he guided the forceps, only one blade of which was *in* the groove of the conductor. The stone was seized and at once withdrawn, and the operation thus completed.

It will be understood from this account that the method differs from that adopted by other surgeons in that—(1) no anæsthetic is administered; (2) that in dividing the structures in the ischio-rectal fossa the scalpel is thrust by one plunge into the groove of the staff at the point of the anterior extremity of the wound, and that owing to the traction made on the anus, the incision through the skin, when the parts are relaxed, is seen to extend over the tuber ischii; (3) that the incision whereby the bladder is opened is semilunar in shape, and describes a curve constituting an arc (as Mr. Gutteridge states it) of 100 degrees of a circle; (4) that the forceps are guided into the bladder along a grooved director instead of along the left index finger. Mr. Gutteridge spares, if possible, the membranous portion of the urethra, and lays open only the prostatic part. His aim is to make his knife enter the canal at the apex of the prostate gland.

For these modifications in the procedure special instruments are provided. These were recently shown to members of the British Medical Association, at the forty-first annual meeting, in August last, a description of which Mr. Gutteridge has himself given as follows:—"A table constituted of a fixed frame, on which is a sliding top, whereby the patient may be firmly fixed and kept symmetrically in all respects, and yet through which, by means of a simple screw action, the position of *tight-up trussing* may be speedily relaxed, and the pressure on the respiratory and venous systems lightened, so that in a protracted operation rest may be given and renewed effort made, by interval, at the operator's pleasure—a resource in the aged and the fat of the very first importance as respects the sustaining the patient's power of endurance without fatal congestion. The manual instruments consist, first, of a staff, with handle capable of being used by the operator and the assistant at the same instant of time, and with a furrow for the knife to traverse, coiling slightly, and ruffed so that the holder of the knife may have a positive assurance of the knife and staff being in real contact throughout the course of the knife, until it is entered into the bladder. Secondly, an implement consisting of a handle shaped for the finger and thumb, with scalpel mounted at one end; and at the other a beaked knife—properly a cystotome—fixed at a right angle to the scalpel on the axis of the haft, so that one instrument may effect, by a move of the finger and thumb, the two distinct incisions, the external one and that of the prostate. Thirdly, a conductor for the forceps conformable in respect to size to the operator's left forefinger. Fourthly, forceps with supplementary handles by which the stone may be compressed with hair-light pressure while increased force is brought to bear on the main arms of the forceps; admitting of the separation of the process of compression and traction; as well as relaxation of the hold of the stone, for the purpose of adjustment of the forceps to the stone, or gentleness of pinch, in case of the stone being of friable material."

The operation for stone performed with these instruments and in this way Mr. Gutteridge would desire to have known as "lithotomy made speedy and safe." Certainly in his own hands, when performed as we saw it, it is rightly so styled. It was done in a remarkably short time—less than half a minute,—and every step in the operation was executed with rapidity and precision. The boy has gone on perfectly well since the operation, and within forty-eight hours afterwards the urine was all voided by the natural passage. This, as is well known, is an unusually short time between the operation and the safety of the patient, which is secured by the flow of urine along the urethra. Yesterday, the eleventh day after the operation, the patient returned to his home in Staffordshire, perfectly cured. We sincerely hope we shall have other opportunities of seeing Mr. Gutteridge cut for stone.—*Medical Times and Gazette, Nov.* 15, 1873, *p.* 548.

78.—CURRENT VIEWS ON THE TREATMENT OF STRICTURE OF THE URETHRA.

By William S. Savory, Esq., F.R.S., Surgeon and Lecturer on Surgery at St. Bartholomew's Hospital.

[The most current view of the nature of stricture is simply a mechanical one.]

That stricture of the urethra is, to all intents and purposes, a constriction of a portion of the canal, due to thickening and contraction of a deposit in its walls, just as any tube or pipe may be blocked up, to a greater or less extent, by foreign matter; and the worst of this is, that it at once suggests to him the notion of treatment by purely mechanical means; that the obstruction must be overcome by force, either gradually and more cautiously by the repeated application of less force, or rapidly and violently by the sudden application of much force. Stricture of the urethra is too often regarded as a simple physical condition, to be treated only by physical means.

The point I am anxious to impress is this: this plan—the usual plan—of treating stricture is founded on a misconception. Excluding from consideration now those cases of so-called stricture in which there exists no structural changes, even in the simplest cases of organic stricture, the condition is hardly ever, practically never, a purely physical one. With the structural change, there is associated an active morbid state, either some inflammation or congestion of the parts, and almost always more or less of spasm. The evidence of this seems plain to those who will read it.

How otherwise can one account for the varying size of the stream, even when permanently diminished? Very few instances of stricture occur in which, upon investigation, it does not appear that the stream of urine varies much in size; that, at certain times, and under favourable circumstances, the urine is passed much more easily and quickly than at others. This fluctuation in the size, and other characters of the stream, which is so very common in cases of stricture, cannot be due to any permanent alteration of the canal, but must be referred to some temporary cause which is capable of modifying its size and shape. Then the greater facility with which instruments may be introduced at some times, and under certain circumstances, than at others, irrespective of skill; and the speedy improvement which follows measures that can hardly, in so short a time, affect a permanent stricture—such as rest, warmth, aperients, and opium—can only be explained in the same way. And again, would an organic stricture, in a passive state, account for tenderness and disposition to bleed of the portion of the urethra which is the seat of the obstruction?

So, too, is evidence of the same kind offered by the effect of removing the pressure of the urine from behind, as by tapping the bladder. This is often at once followed by partial relaxation of the contracted portion of the canal. Beyond the immediate relief which is afforded by the operation, this appears to be the great advantage which is derived from puncturing the bladder or urethra behind in retention of urine from stricture. The urine is no longer impelled against the obstruction by straining, and the urethra is placed at rest. So also the evidence of the effect of enlarged prostate. Brodie remarks, "Where a simple chronic enlargement of the prostate gland supervenes on stricture of the urethra, the latter usually becomes less liable to spasm, and is more easily dilated, and altogether more tractable than it was before." Because now the pressure of the urine, which was a constant source of irritation, is intercepted by the prostate.

How otherwise can one explain the fact of complete retention of urine suddenly supervening upon stricture, with the fact, often verified after death, that the degree of organic contraction will not account for the degree of obstruction which exists?

Thus, it may be understood how the controversy about impermeable stricture has arisen. Much discussion has been expended on the question, whether such a condition as a really impermeable stricture ever occurs. That the canal of the urethra is ever so permanently closed and sealed up by organic stricture as to be altogether impervious to urine, may be very well denied; but no position can be less tenable than that taken by those who argue that, where urine can pass out, an instrument may be passed in. In some cases, indeed, neither urine nor instrument can pass; but, even if the former can somewhat freely escape, it by no means follows that the latter can be passed through. This is a very different matter; for, setting aside other considerations, everything may depend upon the presence or absence of spasm or muscular contraction. There may be a passage for the urine, but it may be closed against an instrument.

Therefore, in the treatment of stricture, the employment of instruments should *never be an early*, only a late resource. Of course, I am not speaking *now of retention of urine* in consequence of stricture—that is another sub ject—but of the direct treatment of stricture of the urethra. In these cases, I submit that we should turn our attention more to the cause and less to the effect. The new matter, the lymph, which has been deposited or formed, and has subsequently contracted, causing stricture, is the result of a morbid process, which has either passed away, or which is still active, or lingers.

If such action be still present, what can be conceived more mischievous than irritation by an instrument of any kind? What can be more opposed to all that are recognised as sound principles of surgery? What would be done in an analogous case elsewhere? Would it not be deemed of the first importance to remove all causes of irritation—to obtain as far as possible absolute rest—to meet at once and directly the cause of the mischief, and assuredly not by rash meddling with the effect to aggravate the cause? Surely this is sound in reason, and I will venture to say supported by the results of the practice founded on it.

Foremost among the means at our command is rest—absolute rest in the recumbent posture. I do not pretend to say precisely how long this should be persevered in, or how strictly it should be enforced in every case; but I venture to assert that in the treatment of every case of stricture it should have a fair trial before any other means are resorted to. I do not think mere avoidance of business or confinement to the house sufficient; it should be continuous rest in the horizontal posture. Much good must be attributed to the effect of position. In short, the patient should be kept in bed, for this not only secures rest in the right position, but uniform warmth, which is the great point also. No doubt, many patients with stricture will object to this, just as surgeons neglect to enforce it, because its full value, and all the cost and trouble it saves, are not recognised, but I believe that, in proportion as experience is gained of the practice, so will it be esteemed—that those who carry it out most strictly and perseveringly will be best pleased with the result.

Beyond the all-important means of rest and recumbency, assisted by warmth in the form of baths or otherwise, there are remedies of great power in allaying irritability. Opium, which in these cases is so often valuable, as a rule perhaps is best administered, as laudanum, by the rectum; but this, or some other form of it, whether solid or liquid, may be given, when required, by the mouth, or morphia may be injected under the skin. Belladonna, too, or its alkaloid atropine, is sometimes useful, especially when locally applied; but, as a rule, I think they bear no comparison in efficacy to opium.

Active aperients also are often strikingly serviceable. The urine, which up to the period of their action had been obstinately retained, will sometimes pass freely during a brisk action of the bowels. The escape of urine, which is often simultaneous with the action of the bowels, and usually then most free, is a fact which should be always borne in mind. And it may be remarked that the success which attends the use of such remedies as those which have been indicated, is an additional proof, and a strong one, of the nature of the case. But, except in extreme and urgent cases, the use of drugs and of all active remedies may well be dispensed with, and reliance placed on simple measures of hygiene, such as wholesome food, warmth, and, above all, rest for a while even in bed.

But suppose a case in which all morbid action in the way of cause has passed by, and in which nothing remains but the effect, the thickening and contraction, what then? Now, in the first place, I say—and here, no doubt, I shall be at issue with many—that such a case in actual practice is, to state the least, extremely rare; that a troublesome stricture, due simply to a mechanical cause, apart from all morbid action of any kind, is to be regarded as a clinical curiosity; that this condition of things, which so often exists in idea, is hardly to be found in fact. But assuming it to be, even then I would repeat, still keep instruments in the background until other measures have been fairly tried. No one, I think, who has probably tested the value of these measures will be disposed to treat them with indifference in any case. Is it so anomalous an event for this new matter, this lymph, this outcome of inflammation, or of some kindred morbid process, to yield to time, when the parts thus for a while hampered in their functions are placed under conditions favourable to recovery; when causes provocative of still further mischief are carefully excluded? At all events, I would repeat the statement that these simple measures seldom if ever fail to effect improvement of the stricture. Of course one could not pretend to measure in a given case how much, if any, active mischief may still be lingering there. In many cases it may be very little, in some possibly even none. But this I can affirm: I cannot call to mind a case in which a stricture that gave trouble was not relieved by rest, recumbency, and the other means which have been mentioned. If this be so, should it not form an essential and primary part of the treatment of stricture? Should not the most be got out of these measures before we resort to instruments?

If it be urged that this would be waste of time, which may be saved by employing instruments at once, it may be replied that chiefly out of such mischievous practice have arisen such convictions as those to which I referred at the commencement of this paper. But then it will be argued that after all you come at last to instruments to accomplish the cure. Not always—very often not. I do not pretend that, under the sole influence of these measures, which are simply hygienic, all traces of stricture usually disappear; but the patient is very often, and sometimes speedily too, restored to such comfort, he is so relieved from every trouble, the stream of urine flows so naturally, that he is well satisfied with the result, and declines any further interference.

But by these means you have not thoroughly cured his stricture; you have given him only temporary relief, and his troubles may at any time return. That will in many cases depend on himself, and the care he takes of his health. No doubt in most cases of severe stricture these simple means are only palliative. Frequently, indeed, they will succeed so well that the patient himself is no longer conscious of anything amiss, yet the passage of an instrument will still detect an abnormal condition. But supposing you fall back on instruments, what then? How much more than this can you do? Dare you, in the majority of cases of severe stricture, promise by their aid a complete and permanent cure? Can you do more than restore a patient to a condition of comparative comfort, yet still with liability under unfavourable circumstances to relapse? The sum of my argument, then, amounts to this: while I am not suggesting that instruments are never to be employed in the treatment of structure, I venture to affirm that they have been and still are, grossly abused; that an untold amount of mischief is perpetrated by their abuse; that the routine employment of instruments in the treatment of stricture is in the highest degree unscientific and improper; that, in a word, in the treatment of stricture, instuments of any kind should never be employed any way except as a last resource.—*British Medical Journal*, *Dec.* 20, 1873, *p.* 713.

79.—ON EXTRAVASATION OF URINE.

By William S. Savory, Esq., F.R.S., Surgeon to St. Bartholomew's Hospital.

Extravasation of urine occurs in two forms. The one, severe and rapidly destructive—the acute, as it may be called; the other, slower, far more gradually and indirectly mischievous—the chronic.

The first, where the escape of urine and infiltration of tissue are sudden and abundant, is usually the result of a fall or blow upon the perineum, whereby the urethra is lacerated. The immediate effect of the injury is to produce ecchymosis—swelling and discolouration of the perineum, and more or less of the scrotum, with perhaps the escape of blood from the orifice of the urethra. But the patient soon finds that he cannot pass water, and the attempt to do so is followed by a very rapid increase of the swelling of the perineum and scrotum, until the former becomes extremely tense and the latter enormously distended. Then, if no relief be given, the infiltration and its consequences extend forwards towards the penis and upward over Poupart's ligament on the abdomen; its course being determined, as everyone knows, by the relation and connection of the superficial and deep fasciæ. Now such mischief is marked by very striking local and constitutional effects. Locally, by rapid destruction of the infiltrated cellular tissue—decomposition, suppuration, and sloughing; generally, by severe constitutional disturbance or fever, which rapidly assumes a typhoid form.

Now what is to be done in such a case as this?

[All the mischief being due to the presence of decomposing urine, which is confined by the skin, our first step should be to make free incisions. The gush of stinking urine is succeeded by an abundant drain.]

There is no difference of opinion about the necessity and efficiency of free incisions, and nothing can be more gratifying than to observe their effects. A patient, beforehand, may appear to be almost moribund—in a state of extreme depression, with a pinched and anxious face, a feeble running pulse, a dry and brown tongue. A glance at the scrotum tells the tale. You give him brandy, gently raise and separate still further his legs, and lift him to the edge of the bed; in a few seconds the incisions are made, and in a few hours after he is a different man. The relief has been complete; he rallies rapidly, and usually does well.

[An important question is, whether a catheter is to be retained in the bladder or not. This the author decides in the negative, because it may cause much irritation, and even if it does not do so, it does not fulfil the object intended, as the urine still continues to flow by the side of the catheter and through the wounds, although in lessened amount.]

It is urged, the escape of urine by the rent in the urethra prevents its closure. Now, considering the kind of injury the urethra has sustained—not a clean cut, but bruising and tearing,—I doubt very much whether the passage of urine over it is for some time more unfavourable to repair than the constant presence of an instrument there. But be this as it may, the objection to the retention of a catheter rests on other, clearer, and far stronger grounds. It is a fact that when an instrument is retained in the bladder all the water does not continue to escape through it, but some will at length inevitably find its way along the outside of it—between it and the urethra, and this urine, mingled with pus from the suppuration of the canal provoked by the catheter, escaping by the wound in the urethra, must, at least, be mischievous enough to countervail any advantage that could accrue from the instrument.

As a matter of fact, I have often seen a catheter employed in these cases, but never, I must say, without, in my opinion, mischief. Given a case of rapid extravasation of urine, such as I have sketched at the commencement, when the surgeon has made incisions into the cellular tissue requisite for the escape of the fluid he has done enough in the way of manual interference. No farther measures are at the time called for beyond poultices or fomentation with antiseptics locally, and nourishment, with stimulants if needful, and perhaps opium. But what will happen? The patient, in all probability, will rapidly rally; urine will escape freely by the wound or wounds in the perineum; there will be an abundant escape of urine and pus, which will quickly become less fetid; and by and by the separation of sloughs of cellular tissue, extensive as the case had remained long without relief. Then the wound contracts and heals, as a rule, completely, though occasionally a fistulous track may for a while remain. As the wounds close in, the urine returns to its natural channel, and at length the patient passes all his water in the natural way. But often, very often, with considerable obstruction, and the introduction of an instrument at this period always discovers a contraction—a traumatic stricture at that part of the urethra which was injured. Now the time has arrived for the dilatation of this stricture, and, if not before, as the stricture is dilated, any fistulous aperture will usually close.

But of course the question will be pressed here,—Could not this contraction of the urethra have been prevented by the retention of an instrument in the first place, and could not much time have been saved thereby? Yes my part, I say, No; and as the whole matter in question turns upon the answer just given, it may be worth while to consider it more fully.

Though the urethra may be much bruised and very badly hurt by the injury, yet, for the most part, it exhibits a remarkable power of repair. Injuries which have destroyed the urethra in its whole circumference, or damaged it to an extent which may be measured even by inches, are yet repaired in such a way that the continuity of the canal is restored, although the calibre is of course contracted; so that any notion that the constant presence of any instrument is needed upon which the restored portion of the canal may be moulded can hardly be the result of clinical observation. But then again, Could not the contraction—the stricture—have been thereby prevented? No. Because this contraction—assuredly the worst part of it—is a subsequent process. To retain any instrument continuously for several weeks is of course, as a rule, out of the question. Even supposing it to be retained from the first for two or three weeks—and surely this is a liberal assumption,—still there will persist the most inveterate tendency to contraction long after this, and the hardest part of the battle has still to be fought. In fact, I have not found, to say the least, more difficulty in introducing an instrument and dilating the urethra in those cases where the canal has been left to itself until all disturbance has subsided, than in others in which frequent attempts had been made to maintain its patency from the first. In short, do what you will in these ugly but interesting cases, you must almost inevitably have, by and by, the most obstinate and inveterate form of stricture to contend with: and I cannot but come to the conclusion that you will save your patient much irritation and distress by not attempting to oppose the contraction of the urethra until all active disturbance, local as well as general, consequent on the injury has subsided. I repeat again, that the contraction, which is your chief difficulty, occurs late in the case. The difficulty of introducing an instrument early depends, for the most part, on the swelling and the interruption of the continuity of the canal. The contraction which constitutes stricture is a process, remember, of the material of repair—of the cicatrix, in short; and is there the shadow of justification for the idea that a cicatrix in the urethra follows a law of its own, altogether different from those that prevail elsewhere? And, depend on it, the urethra during its repair cannot be disturbed with impunity. Therefore I submit that the practice, in these cases, of introducing and retaining at the outset an instrument, requires something more to justify it than the vain hope that we can by this means prevent the formation of stricture.

No strict rule can of course be laid down as to when the dilatation of the resulting stricture should be commenced. The best test for this is the degree of difficulty there is in emptying the bladder. While the urine can escape without distress there is no urgent need for interference; and when doubt arises, the examination of the urethra by an instrument will greatly assist you in your decision. If an instrument can be passed into the bladder, so much the better; and when you will repeat this proceeding, or whether you will adopt some other, will be determined by the effects of the first trial.

But now, supposing all immediate mischief due to the extravasation to have subsided, and, as the wounds in the perineum contract, the patient to experience great and increasing difficulty in passing urine; and supposing, under these circumstances, that you fail to introduce any instrument,—what is to be done? Surgeons will answer this question very differently, but you may make up your mind to this: that whatever plan of treatment you adopt, your practice will be beset with difficulties; and all your judgment, care, tact, and skill will be required to overcome them. You may do many things and much mischief; if you avoid this, and succeed in enabling your patient to pass water in a natural way without discomfort, you will have assuredly done very well.

You must, if you can, establish the route from the penis to the bladder; and for this purpose I know no better plan on the whole, than the following:—*The patient is placed* in the same position as for lateral lithotomy, *and an instrument of* moderate size is passed through the urethra as far as practicable, and there firmly held. Then an incision is made in the median line of the perineum down upon its end, and the resisting structures in front of it are cautiously divided. Then through this opening an instrument, such as a long probe, is passed onward to the bladder. Now this is undoubtedly the great, at times the almost insuperable, difficulty of the operation. To discover the track of the urethra, now converted into a fine tortuous fistula, even from the perineum, with a probe is no slight feat; temper, patience, delicacy of touch—large demands will be made on them all. I have seen this operation done many times; often it has seemed as if the operator would, after all, fail. I have known him to fail; but, in my experience, with very few exceptions, the bladder is at length reached. Then the remaining steps of the operation are comparatively easy; either a catheter passed through the penis is, directed by the probe, carried on at once to the bladder, or a larger instrument is previously passed from the perineum guided by or over the probe; its passage assisted, if need be, by the further division through the wound of any resisting structure. The result of this operation is, that an instrument is secured in the urethra throughout its length, retained there so long as it can be borne, and afterwards an instrument is passed at intervals to maintain the size of the canal.

But it is argued—Why not do at first what you have been compelled to do at length? Why not, when you give escape to the extravasated urine, assuming, as is often the case, that an instrument cannot be passed without—why not at once seek for the further end of the urethra from the perineum, and then pass and retain an instrument. Why? Because, in the first place, it does not follow that such a severe step will ever be required. You cannot tell at first to what precise extent the urethra has been injured, or to what degree it will be restored. Nay, even if you examine the injury through an incision in the perineum, you cannot calculate the extent to which it will recover; you cannot—and this is the point—tell whether at length you may not be able to pass an instrument by the natural course, and so happily—as in almost every case you may—avoid the risks of the operation altogether. But waiving this objection, I contend that you undertake the operation afterwards under far more favourable conditions than at first. Your difficulties are lessened because, although the parts are still in an unnatural state, you are not further embarrassed by the great distention due to recent effusion. But, above all, your patient is in a far better state for so trying a measure. Anyone who is the subject of rapid extravasation of urine is of necessity very ill—must be ill for some time to come—is, beyond all question, in a very unfit state for any operative proceeding, especially one which in all probability will prove tedious and exhausting. I say, therefore, that by adopting this proceeding at the outset you are doing, under almost the worst possible condition of your patient, that which might never have been required. Furthermore, the difficulty, the anxiety, and even danger of retaining an instrument, through his subsequent illness, may be so great as to compel you to remove it before the parts have closed in and consolidated, and so in a short time the urethra is no better off than if it had been left alone.

For these chief reasons, then I prefer to wait. I do not think that thereby I lose time; on the contrary, I am convinced that in the end I gain it, for you see when the immediate effects of the injury have subsided, the parts are in a condition to recover much more directly from any operation.

The other form of extravasation of urine, where the escape is far more limited and the infiltration very gradual, produces very different effects. Here there is no acute suppuration and sloughing; but the cellular tissue, starting from the perineum at the point of leakage, slowly, at times almost imperceptibly, becomes thickened and brawny, and the skin, too, dense, rough, tuberculated, and reddened. The infiltrated, thickened, and brawny tissue extends, involving in its course, first, the whole scrotum, which may become enlarged to several times its normal size; and then the penis, which from the swollen state of its integument and cellular tissue becomes not only much enlarged, but often curiously twisted and otherwise misshapen, the prepuce especially sometimes being so twisted and tucked in at the orfice that it is very difficult, or even impracticable without slitting it up, to find the meatus of the urethra. When matters have advanced to this stage the appearance is very characteristic, and the nature of the case is revealed at a glance; but at first, when the infiltration has just begun, or when it is very trifling—for it is easy to understand how there may be all degrees of it,—the signs are obscure, and a careful examination is needed to establish the fact. More especially if there be one or more fistulæ in the perineum, it often requires some tact to distinguish between the surrounding condensation of tissue which is the inevitable result of their existence, and that which is due to the occasional escape of some urine beyond their limits.

Now, this form of extravasation, far less urgent than the other, does not call for such prompt and decisive measures. It is almost always consequent on long-standing and neglected stricture, and is, therefore, unlikely to occur, to any considerable extent except in those who are placed beyond the reach of ordinary care and attention. And thus in these cases one is often struck by the signal improvement which rapidly follows simple rest in a hospital bed, and the removal of all those injurious influences to which such patients are accustomed to be exposed. In many of these cases such simple measures of hygiene will suffice, not merely to stay the spread of mischief, but even to enable the tissues involved to recover their healthy state. But in others, again, a farther step must be taken—the dilatation of the stricture. Whether this is immediately necessary or when it should be attempted must be chiefly determined by the condition of the stricture itself, by the mode in which the urine is passed and the size of the stream, and by the result of examination. But of those cases in which the stricture needs to be, without delay, dilated, very few remain which call for farther measures. Unless there are signs of the accumulation of fluid in the cellular tissue, incisions cannot afford any material relief. The brawny tissue may be here and there divided; but one is too often disappointed at the small advantage of any kind which is gained, while they sometimes bleed actively, and these patients are so frequently broken down in health that they have no strength to spare, and their wounds are very reluctant to take on healthy action and heal.—*Lancet, Jan.* 17, 1874. *p.* 79.

80.—A CASE OF RETENTION OF URINE FROM IMPASSABLE STRICTURE.

RELIEVED BY ICE IN THE RECTUM, AND THE STRICTURE SUCCESSFULLY TREATED BY CAUSTIC POTASH.

By W. F. Teevan, Esq., B.A., Surgeon to the West London and St. Peter's Hospitals, London.

For all practical purposes, strictures of the urethra may be divided into two kinds, the Passable and the Impassable; and the former are as easy to treat as the latter are difficult. An attempt to relieve retention of urine from an impassable stricture is not always successful, and an operation has sometimes to be resorted to. Any remedy, therefore, for this complaint which is harmless in its operation and speedy in its effects commands our attention. I believe M. Cazenave, of Bordeaux, was the first to introduce into practice, some quarter of a century ago, the plan of treating retention of urine by plugging the rectum with ice; and if the method is only half as successful as he claims it to be, it will prove a valuable auxiliary to the surgeon. When called to a case of retention, whatever may be its cause, I always endeavour at once to pass an instrument; and I only resort to other means when foiled with the catheter. In the case I am about to relate I had in vain endeavoured to introduce different instruments, and, knowing how highly M. Cazenave had extolled the merits of ice in similar instances, I determined to give his recommendation a fair trial; *with what result will be seen.*

On October 18th, J. B., a labourer, aged forty-six, came under my care at

St. Peter's Hospital, as out-patient, for an impassable stricture of long continuance, situated five and three quarter inches from the meatus externus. Immediately in front of the stricture, in the median line, there was a false passage into which a catheter could be passed up to the hilt without drawing a drop of blood, thus demonstrating that it had acquired the respectability of age. On October 18th, and again on the 20th, I tried in vain to introduce different instruments; and on the 22nd the patient came to the hospital with an attack of retention of urine. Mr. Walter Coulson, who happened to be in the institution, endeavoured, without success, to pass a catheter, and I was sent for to take charge of my case. On my arrival I at once employed the catheter, but without any result; and therefore resorted to M. Cazenave's plan, and plugged the rectum with small pieces of ice. Exactly twenty minutes after the last fragment had been inserted into the bowel the patient began to pass water guttatim, and in the course of half an hour contrived to empty his bladder. He was kept quiet and warm in bed, and his bowels were gently moved every day. I endeavoured every other day, for about five minutes, to pass an instrument, but without success; and on Nov. 11th I had his perineum freely leached, and continued my attempts at catheterism, with no better result till Nov. 24th, when I fancied I had succeeded in introducing a very slender whalebone bougie into the bladder. In order to demonstrate the accuracy of my supposition, and to utilise its realisation, if correct, I attempted to slide one of my smallest grooved silver catheters over the bougie into the bladder; but in doing this I used an amount of force which nothing but the intractable and serious nature of the complaint could justify, and the result was that the bougie was cut in two about its centre, six inches of the whalebone being left behind in the stricture. As the patient experienced no pain from its presence, I determined to leave it in the urethra, in the hope that it would set up a profuse discharge, and so facilitate the treatment. In this expectation, I was, however, disappointed; and on Dec. 1st I removed the broken portion of the bougie after seven days' imprisonment, the patient being all the while entirely ignorant of its whereabouts, and complaining of nothing beyond an occasional smarting when passing water. I now directed the patient to pass down a large bougie as far as it would go, and to keep it pressing against the face of the stricture for half an hour every day. Twice a week I tested the urethra with instruments to see if any progress were made, but the results were always of a negative character. The man's stricture possessed but little sensibility, and hence the unremitting assaults made on it were productive of neither pain nor annoyance.

On Dec. 19th I came to the conclusion that a fair and unusually patient trial had been afforded for the success of all ordinary means, and nothing apparently remained but the knife to cut the Gordian knot. As, however, the patient's perineum was intact, I was loth to cut into it, and the presence of a false passage did not render the most difficult operation in surgery more easy of performance. On reflection, I considered that this was a fit and proper case on which to try the "bougie armée," and I communicated my intention to Mr. Henry Smith, the redoubtable champion of this potent machine, who was kind enough to intimate to me his gladness to be present when I used it. Accordingly, on Dec. 22nd, in the presence of Mr. Smith and two other gentlemen, I passed down to the stricture a wax bougie slightly curved, and armed with a small piece of caustic potash. For a couple of minutes I kept the bougie firmly pressed against the stricture, slightly rotating it whilst doing so; and on withdrawing it I found the potash had all deliquesced against the stricture. Being aware of the exact relations of the false passage, I was particular in keeping the bougie's point pressed slightly upwards, in order to avoid entering the abnormal route in the lower wall. The application of the caustic was not attended with any pain or bleeding, and on December 25th I reapplied it, the patient complaining that the orifice was very sore, as its lips had tasted the caustic on its entry to the urethra. The following day the man stated that his stream of water had much improved, but there was a gonorrhœal discharge, with, however, diminished frequency of micturition. On Dec. 27th, I again employed the "bougie armée" as before; and on the 31st, knowing the instrument had penetrated about a quarter of an inch deeper than on the first occasion, I determined to endeavour to pass a very small olivary bougie. Accordingly, before using the bougie, I gave it the well-known *coudée* turn of M. Auguste Mercier, and I had the satisfaction of seeing it enter the bladder. I then successfully introduced two larger bougies, and finally left a No. 6 olivary catheter in the bladder for the night, the patient removing it at 9 A.M. on New Year's Day. The effect of leaving in the instrument was that I was enabled on the day mentioned to pass Nos. 7, 8, 9, one after the other. On Jan. 3rd I introduced No. 12, in the presence of Dr. Reyher, of Dörpat, and others; and on Jan. 5th I passed a No. 13 olivary elastic catheter, when Dr. Budgett was present and drew off the patient's urine. The following day the patient went out on leave, returned intoxicated, and was discharged from the institution. The patient reappeared on January 31st, when I passed No. 15 olivary catheter with ease.

It will thus be seen that the satisfactory result obtained in this case was entirely due to the employment of the "bougie armée;" for rather more than a two months' trial had been given to permit the effects of rest, combined with leeching and diligent attempts at catheterism, to produce the desired result, without success. It would appear, therefore, that the application of caustic is justifiable and useful in those cases of stricture which are proved to be impassable; and it is to be used not to tunnel through the stricture, but to open up its mouth, the impassability of the stricture being due to the eccentricity of the entrance to the contracted portion of the canal. The retention which took place in the early treatment of the case was successfully relieved by the introduction of ice into the rectum—a method which I believe is practically unknown in this country. One great advantage of the plan is that it in no way interferes with an attempt at catheterism. In the instance I have related I found the introduction of pieces of ice into the rectum gave rise to some local annoyance, which could be entirely prevented on another occasion by the employment of a speculum open at both ends.—*Lancet*, *Feb.* 7, 1874, *p.* 194.

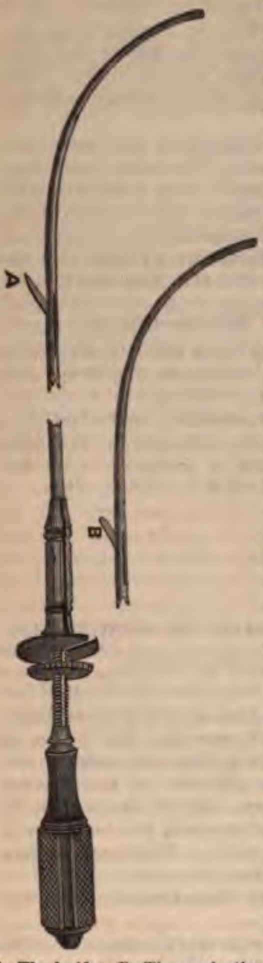
81.—AN URETHROTOME FOR INCISING NARROW STRICTURES.

By BERKELEY HILL, Esq., Surgeon to University College Hospital.

Hitherto, among the variety of instruments that have been constructed for internal urethrotomy, none, so far as I know, can be used for strictures through which a No. 6, or, at most, a No. 4, catheter cannot be passed. The treatment of such cases, whatever method of dilation be selected, does not usually present much difficulty, but with smaller passages, the difficulty of introduction and the amount of suffering to be contended with have induced me to seek for some means by which a narrow stricture, when an instrument has been passed through it, may be at once enlarged to the normal capacity. This may be done by *internal incision* or by *divulsion;* which of the two proceedings should be adopted in any given case, depends upon certain points that are now pretty well agreed upon.

The instruments which I have here are both manufactured, and, to a great extent, devised by Mr. Coxeter, who has exerted much skill in constructing instruments to carry out my suggestions—one for cutting, and the other for breaking through the stricture. Before introducing the instrument for enlarging the stricture, the distance of the contraction from the meatus urinarius should be ascertained by passing an olive-headed bougie with a slender graduated stem. This being done, both instruments are passed in the same manner. A guide bougie of fine whalebone, about one-fourth the calibre of a No. 1 catheter, is passed through the stricture to the bladder; should the operator be in doubt if he have reached the bladder, a fine silver catheter, having its end tunneled with an eye, is threaded, and, thus guided, the catheter is slipped along the bougie and through the stricture till the bladder is reached. Urine then runs through the catheter, and makes it certain that the instruments are in the right track, when it may be withdrawn, leaving the bougie *in situ*. However, in most cases, this precautionary exploration may be omitted.

The next step is to pass the enlarging instrument along the guide in the same way. The incisor consists of a slender grooved sound of steel, with the ordinary curve, and provided with a tunneled end. In the groove runs this little rod having a blunt end fixed to it by a joint. When run along the groove of the sound, the jointed end and rod are quite concealed until they reach a point 6½ inches down the shaft. Here the groove becomes shallow and ceases; thus, the jointed end is thrown out, and projects about one-tenth of an inch beyond the surface of the sound. (See B in the figure annexed.) It thus forms a little catch on the sound, which will travel along the urethra until it reaches the stricture, against which it will be caught. After the grooved sound has been passed along the guide, through the stricture, the little rod may be run down, and its end made to project into the urethra behind the stricture. By drawing forwards the sound thus armed, the posterior limit of the stricture is ascertained; the anterior limit is already known. The little rod is then withdrawn and replaced by another which carries, instead of a blunt point, a knife attached by a joint in the same way; this knife, concealed in the groove of the sound, travels down until it has reached the place where the groove grows shallow. (See A in the figure annexed.) This termination of the groove being already carried beyond the posterior limit of the stricture, the knife is thus made to project to a distance regulated by a screw at the handle against the floor of the urethra; and, if the sound be drawn forwards, the knife cuts its way until the whole stricture is divided. When the anterior limit of the stricture is reached, the knife is again brought within its groove and the sound is withdrawn. The knife projects half an inch as a maximum, but, by adjusting the screw, can be set to any less distance. The sound, with the knife lying in its groove, is of less calibre than a No. 2 catheter, being between that and No. 1 of Weiss's catheter scale. After the incisor is withdrawn, to make assurance more certain that the stricture is thoroughly divided, the split sound of the wedge-dilator may be passed, and the wedge which corresponds to the natural capacity of the urethra No. 14 or 16 may be run through the divided stricture.



A. The knife. B. The projection to ascertain the posterior limit of the stricture.

This wedge-dilator, which I had the pleasure of showing to the profession at the Plymouth meeting, acts on the same principle as that so well known and so successful in the hands of Mr. Barnard Holt. It differs in being of simpler construction, being a wedge at the end of a rod, which, being pushed between the blades of a split sound, separates them to the required extent, and thus splits the stricture it passes through.

My dilator can be made as small as No. 1½ catheter, is easily passed along the canal, and when through the stricture, the wedge requires very little force to separate the blades; for the surfaces of friction are together little more than half an inch in extent, instead of being the whole distance traversed by the instrument both within and without the separating tube, as in Mr. Holt's dilator.

At the beginning of the paper, I said that, when forcible enlargement is selected, the rules that influence the adoption of a cutting or splitting instrument are tolerably well known. I may, perhaps, be allowed to recapitulate the most important.

1. All contractions in front of the bulb—that is, in the penile part—should be cut, not split, because cutting causes less cicatricial tissue to be formed than does splitting; this is important where the layer of erectile tissue is thin and easily replaced by fibrous tissue, by which erection at this point may be wholly lost.

2. Whenever the contraction is very elastic, it stretches rather than breaks before the wedge, and is best divided by a sharp edge.

3. Whenever the induration matter extends more than half an inch along the passage, or is very thick, it is usually best to make an incision through it.

In other cases, as divulsion is simpler, and needs less precaution, it is on those accounts generally preferable to incision.—*British Medical Journal, Nov.* 29, 1873, *p.* 627.

82.—ON THE VALUE OF THE "BOUGIE CONDUCTRICE" IN THE TREATMENT OF STRICTURES OF THE URETHRA.

By Rushton Parker, Esq., Surgeon to the Stanley Hospital, Liverpool.

A few weeks ago Mr. Savory made some remarks upon the general clinical aspect of urethral stricture, and his allusion to the superaddition of spasm is a point as undeniable as it is liable to be overlooked.

With regard to the extensive employment of the recumbent posture, as a substitute for operative measures, I have no experience, and see little prospect of ever having it; for I am sure that in the majority of instances, especially those occurring in private practice, this cannot be even attempted. There are many men suffering from stricture, even of narrow dimensions, who find the malady inconvenient enough to wish it away, and who will gladly submit to anything that shall not confine them to the house for more than a week or so, but who would go on fighting with the stricture unaided by surgery, to the detriment of bladder and kidneys perhaps, rather than suffer prolonged recumbency. They have not the leisure for it, to say nothing of strong disinclination.

It becomes, then, advisable to quickly dilate the urethra if possible, and the results of practice are eminently satisfactory, provided an occasional catheter be subsequently passed.

The instruments employed for divulsion have been brought to a high state of development both in simplicity and mechanical efficiency, and the dilator of Mr. Berkeley Hill is, in my opinion, the simplest and the best of these.

As to urethrotomes, those commonly used in England are not fine enough for immediate passage through a stricture less than the size of a No. 4 catheter —the urethra must be previously prepared by soaking in a fine catheter or bougie for a day,—so Mr. Hill has had constructed an ingenious urethrotome, which can, it seems, be employed in the tightest cases.

I wish to draw attention to the urethrotome of M. Maisonneuve, which has been in use, I am told, about eleven years, which is much simpler than that of Mr. Hill, which I find remarkably efficient and easy to work, adapted for the narrowest strictures, and which I think far the best urethrotome I have ever seen or heard of. It costs 16 francs.

The sound is of steel, hollow, split along the upper surface, and the size of a No. 2 catheter. The knife, of which I have two sizes, attached to a thin flexible arm of steel, which bends as it slides along the concavity of the sound, cuts in advancing and receding, but only along its slopes, not at the free apex which is knobbed and very blunt, blunt enough to leave uninjured the healthy part of the urethra. The lash-like appendage is a filiform bougie, fitted with a female screw at one end for attachment to a male screw at the tip of the sound.

The bougie is first passed, its size being that of an English No. ½ or less; then the sound is screwed on to it, and pushed *after* it, the bougie coiling up in the bladder. The knife is then run down the sound and withdrawn, the sound and bougie being pulled out next. The operation is easy, rapid, and successful, and produces neither more pain nor more bleeding than does "divulsion."

The "bougie conductrice" is used in Paris with a variety of urethrotomes and dilators; and could be adapted to Mr. Hill's dilator easily, if wished for; in any case a small metal cap covering the screw at the tip of the sound when the bougie is not used.

The advantage of this fine appendage is that when once the filiform bougie is passed, the surgeon has only to screw to it the sound of a dilator or urethrotome, which will then be safely piloted into the bladder, and the stricture may be immediately divulsed or incised.

I have only employed Maisonneuve's urethrotome in five cases hitherto (not having long possessed the instrument); thrice with, and twice without, the bougie. All except one *required* urethrotomy, the stricture being in the penile portion, but in this one the stricture was behind the penile portion, and the split sound of Mr. Hill's dilator could not be passed, while a "bougie filiform" went easily, and so the operation was concluded with Maisonneuve's urethrotome, as I have not had the dilator fitted for bougies.

The details of my cases are not given, nothing occurring but what is ordinary. None of the patients were in bed more than a week, and some might well have been less; while one, a ship's officer, was up in two days, and out daily after five days, feeling quite well and urinating in comfort. The strictures were very small in size and of some years' duration, and varied from one to three in number in each patient. Following Mr. Hill's practice, I never tie in a catheter after uncomplicated divulsion or internal incision, nor have I had reason to regret the omission.

In cases, then, requiring rapid dilatation, I should say, incise *all, if you like,* with Maisonneuve's urethrotome; for *some* divulsion *may* be used with equal but not greater advantage.—*Lancet, Jan.* 31, 1874, *p.* 162.

83.—THE BOUGIE CONDUCTRICE.

By W. F. Teevan, Esq., London.

I am glad that Mr. Rushton Parker likes the urethrotome, but if it be used in the manner described by him at p. 162 of your journal of January 31st, it will repeat some of its Parisian accidents. It is now twenty years since the instrument was brought into practice by M. Maisonneuve, and it has since then been much improved by M. Voillemier, Prof. Gauley, and others. There are three points which call for remark. In the first place Mr. Parker takes it for granted that when the "bougie conductrice" has passed down the urethra it has entered the bladder, and it is in this assumption that the danger lies. The bougie may have gone into the bladder, rectum, or false sac, and it is for the surgeon to verify the situation of the bougie before he passes the knife into the urethra. Now this can only be done by using the "sonde à vis," which is unknown in this country, or by inserting in the sound a stylet which shall fill the slit, so that when it is withdrawn urine will flow if the bougie has gone into the bladder. Secondly, Mr. Parker states that the blade cannot injure the healthy part of the urethra; but the contrary has been proved in Paris, hence M. Voillemier's modification to guard the blade. Thirdly, French surgeons are now of opinion that the blade ought to slide along the convexity of the sound, and not the concavity as described by Mr. Parker. I would recommend to him Prof. Gauley's tunneled urethrotome, which is vastly superior to the one he has illustrated.

As for "divulsion," "dilatation instantanée," "immediate treatment," or any other synonym by which the process of tearing open a man's urethra may be concealed, it has fully justified the almost universal condemnation passed upon it by Parisian surgeons. It has acquired the unenviable distinction of being the most fatal operation known for stricture, and I am aware of thirty deaths following its use by different surgeons. The operation has been abandoned or condemned by Sir William Fergusson, Mr. Thos. Bryant, Mr. Walter Coulson, Mr. O. Pemberton, Mr. H. Smith, Prof. Stokes, and others.—*Lancet, Feb.* 14, 1874, *p.* 250.

84.—A DESCRIPTION OF THE "SONDE A VIS PORTANT UN CONDUCTEUR."

By W. F. Teevan, Esq., B.A., Surgeon to the West London Hospital, &c.

This useful instrument, unknown, I believe, in this country, but used by Mr. Stokes of Dublin, and by American surgeons, was introduced into practice many years ago by M. le Docteur Ch. Phillips of Paris; and when I mention that that gentleman was sent for by M. Nélaton when he was unable to pass an instrument, and that he succeeded where the great master of surgery had failed, I think it will be deemed unnecessary for me to say much for an instrument coming from such authority. The *sonde à vis portant un conducteur* is, as its name expresses, composed of two parts—a flexible bougie and an elastic catheter. The bougie tapers away at one end to a fine line, whilst its other extremity enlarges to about the size of No. 2, and is fitted with a screw, which works into a socket on the vesical end of the catheter. The ends of the bougie and catheter are screwed together like the bamboo rods used by the chimney-sweeps. The bougie is first passed into the bladder, and the catheter is screwed on to it, and then pushed slowly into the bladder, causing the whole of the bougie to enter that viscus, where its great flexibility allows it to coil up and remain quietly without the patient being aware of its presence till the catheter has effected its object of drawing off the urine. The entire instrument is then withdrawn, or the catheter can be unscrewed to leave the bougie *in situ*, to serve as a guide for the next occasion, or to dilate the stricture.

The instrument is most useful. Firstly, for those cases of enlarged prostate and tumours of the bladder where the urethra is so tortuous that no ordinary catheter can be passed. The slender bougie can be wriggled in first, and so guide on the catheter. Secondly, for cases of stricture where the difficulty arises not so much from the tightness of the stricture as the deformity of the canal (Delpech). Thirdly, for the performance of internal urethrotomy in the deeper portions of the urethra. When the *sonde à vis* has demonstrated that the bougie is in the bladder, it can be unfastened from the latter, which is then screwed on to the urethrotome to conduct it into the organ.—*British Medical Journal, Feb.* 7, 1874, *p.* 171.

85.—ON THE TREATMENT OF FISTULA IN ANO BY THE ELASTIC LIGATURE.

By William Stokes, Esq., Professor of Surgery, Royal College of Surgeons, Ireland.

As the substitution of the elastic ligature for the knife in many surgical operations, a method recommended originally by Signor Silvestri, of Vicenza, and recently adopted and strongly advocated by Professor Dittel, of Vienna, is now largely engaging the attention of the surgical profession both at home and abroad, a few particulars of a case of fistula in ano, recently operated on by me in the Richmond Surgical Hospital by this method, will probably be read with interest.

William M., æt. 48, a strong, muscular man, of irritable temper and intemperate habits, was admitted into my wards in the Richmond Hospital on December 10th, 1873, suffering from fistula in ano. Six years previously he suffered from an abscess at the left side of the anus, which was opened while he was in the Whitworth Hospital, under treatment for an attack of delirium tremens. No fistula resulted from this opening. Two years subsequently he suffered again from an abscess in this situation, and in September, 1873, another abscess formed, which burst, and a blind external fistula resulted.

On examination shortly after his admission into hospital, I perceived the external opening of the fistula, situated at about an inch and a quarter to the left side of the anus. On introducing a director in the fistula, I found that it passed upwards and inwards to an unusual height, and also that it was incomplete. In consequence, therefore, of the great length of the fistula and the distance of the external orifice from the anus, I considered it a suitable case for treatment by the elastic ligature, as by the ordinary operation, owing to the amount of tissues which would have had to be divided by the knife, the hemorrhage would probably have been very excessive and difficult to control. This, considering the patient's habits and constitution, would have been a matter of serious consequence.

I first completed the fistula by Blandin's bistoury, and then I passed a long probe, armed with a double elastic ligature, through the fistula. Owing to its great height, I had considerable difficulty in detaching the ligature from the probe in the rectum; but this I eventually succeeded in doing, and drawing out the loop through the anus I removed the probe, thus leaving the two free ends of the ligature through the orifice of the fistula. I then put the double ligature tightly on the stretch and fastened it securely. This latter procedure was accompanied by a considerable amount of pain, which continued for about an hour subsequent to the operation. A morphine suppository then administered, however, effectually relieved the pain.

On the evening of the fourth day after the ligature was applied it came away, the patient not having lost during the entire proceeding a single drop of blood. The parts divided by the ligature united with remarkable rapidity, and recently the patient returned home in excellent health and spirits.

The impression produced in my mind by the result obtained in this case has been most favourable, the case being one which, owing to the peculiar habits and constitution of the patient and the great height of the fistula, most surgeons would have certainly hesitated or declined operating on by the ordinary method of division of the sphincter.

Although I entertain grave doubts as to the alleged immunity from the occurrence of pyæmia, which Professor Dittel claims for this method, I am confident it will be in many instances preferred by operating surgeons to cutting instruments, "when," as Professor Dittel observes, "he has to undertake operations in cavities and canals which are out of sight, or which are so narrow that it is extremely difficult, if not impossible, to use cutting instruments within them, or when he feels uncertain whether he may be able to tie the bleeding vessels (as in anal fistulæ situated high up). In other cases he will desire to avoid hemorrhage and the formation of large wounds in children or in old persons."—*Medical Press and Circular, Jan.* 7, 1874, *p.* 3.

DISEASES OF THE SKIN, ETC.

86.—ON SUSPECTED RINGWORM (SCURFY HEAD).

By Dr. TILBURY FOX, Physician to the Department for Skin Diseases in University College Hospital.

The practitioner is very often puzzled to make a diagnosis in cases of suspected ringworm. Cases, especially in schools, are brought to him which exhibit here and there—or it may be only in one small spot on the scalp—"scurfy" looking places, without apparently any diseased hairs, and he is asked—Is it ringworm? Without the microscope it is difficult to decide the question; and

I would venture to say that under such circumstances the observer can only blame himself if he falls into error by neglecting the use of the microscope, which will readily reveal in all cases whether or no ringworm is present by the appearance presented by the scales which can be scraped away from the suspected patch. The scales will always be found to have little bits of diseased hairs entangled in them where ringworm is present, and which diseased hairs are not perhaps visible to the naked eye. The accurate diagnosis of these cases is very important where schools are concerned, and a mistake in not recognising the nature of these "scurfy spots" may lead to the silent but wholesale propagation of the disease amongst the healthy. The following case affords an illustration of what I mean:

Case.—I had been prescribing for one or two children in a certain ladies' school, at different periods during two years, for ringworm of the body and head. When the mistress thought that all ringworm had vanished from amongst her pupils, she, having taken every possible means to detect at the earliest moment the faintest trace of mischief in her pupils' heads, in order to prevent the spread of the disease in her school, sent me her little daughter, aged six years, that I might look at a tiny suspicious-looking spot on the crown of the head. This spot turned out to be ringworm, and I destroyed the disease at once by iodine paint. The next day the niece (aged thirteen) of the schoolmistress was sent to me for examination, and I learnt that two years ago the scalp of this child was noticed to be "slightly scurfy" in one or two patches here and there over the scalp. The hair thinned out slightly, but the place was not bared of hair, nor was it red. The disease "did not look like ringworm; if it had," the aunt remarked to me, "of course advice would have been sought." The child had been treated with a "little ointment" now and then; and a medical man saw her, but did not think it ringworm. The appearance of the disease in the mistress's young child induced that lady to send the niece to me, lest the "scurfy" disase from which she had been suffering might in reality be ringworm. When I examined the head of the niece, there were one or two irregular-shaped spots, the size of a shilling or so, covered over with fine micaceous scales, not devoid of hairs. The hair looked a little thin, but not more so than is commonly seen in slight cases of seborrhœa, nor did the hair come out too easily or break off; and on a superficial glance there was no appearance of short broken-off hairs, as in ordinary ringworm. On using a magnifying glass, however, and searching over the diseased areas, certain dark-looking portions of hair-shafts came into view, and these did not run in a natural direction, but were out of the line of the normal hairs, and they were, moreover, in some cases twisted and about three or four lines in length. They were concealed in great part by the healthy hairs. There were, perhaps, five or six in each patch of disease. They turned out to be brittle, and portions came away easily when pulled at. Under the microscope the hairs exhibited the ordinary appearance of hairs invaded by the fungus of tinea tonsurans and fungus of luxuriant growth.

Remarks.—The above case illustrates a not uncommon occurrence—viz., the non-detection of the nature of slightly-developed ringworm of the scalp (tinea tonsurans). Cases of ringworm may present the same characters as those exhibited by the example under notice from the outset and during their whole course; but these characters may be assumed when the disease has become chronic and is supposed to be well, for ringworm leaves behind in many cases a surface that gives off for a while furfuraceous desquamations. The diseased patches may be small—the size of a split-pea—or the area of the disease may be larger. In either case there is apparently a little scurfiness, and the hair is somewhat thinned, and that is all, save an occasional suppurating hair-follicle in the centre of the scurfy spot. But if the scales be scraped away, here and there a bit of opaque-looking hair may be seen attached to or projecting from them, and these bits of hair will be found to be crammed full of spores. Further, in all these cases, here and there a dark stub or two, or one or more broken-off hairs, will be detected over the scurfy surface, and afford a certain indication that the disease is parasitic. Very often, as before observed, the condition referred to occurs in a case of ringworm apparently well, and the solitary or few diseased hairs constitute so many spore manufactories to spread the disease if no parasiticide remedies are used.

The treatment of these cases consists in very carefully getting away every particle of scaliness, and fully epilating the scurfy area, and applying any simple parasiticide until the hair grows healthily again; epilation being repeated to get rid of all short, dull, and opaque-looking hairs.

Practical conclusion.—Whenever a child is brought to the practitioner for his advice on account of the presence of what seem to be scurfy-looking places on the head, if these are small, and the general surface of the scalp is healthy, they are to be inspected for ringworm. A careful search should be made for broken-off hairs, and these or the scales, and any attached hairs, should be submitted to microscopic examination for fungous elements in them. In cases of chronic ringworm, all merely scurfy patches should be carefully examined, for a solitary piece of dead hair lodged in the follicle may explain the mischief, as it is generally loaded with fungous elements, which are readily sown broadcast to relight up the old mischief if parasiticide treatment is abandoned. Such ill-developed cases of ringworm, as before observed, may be the source of infection to many a child in public institutions and schools.

Note.—Dr. Duckworth has recently called attention to the effect of chloroform in rendering diseased hairs in ringworm opaque; but it will be evident that this effect will not be marked where only two or three short hairs are present, whilst the test will be of no value where there are only diseased pieces of hair filling up the follicles and not projecting above the level of the latter.—*Lancet, Nov.* 22, 1873, *p.* 733.

87.—ON A NEW METHOD OF DETERMINING THE PRESENCE OF AND RECOVERY FROM TRUE RING-WORM.

By Dr. Dyce Duckworth.

A few drops of chloroform are to be poured upon the head of the patient, who must be placed in a good light between the operator and the window. On evaporation of the chloroform, the hairs affected by ring-worm are seen to become of a yellowish-white colour, opaque, and like fine filaments of a vegetable lichen. This change is observable, not only upon the hairs, but also on the skin in the immediate neighbourhood. Small whitish masses are seen upon the scalp, and especially at the point of emergence of the hairs. The healthy hairs are quite uninfluenced.

[Dr. Robert Liveing makes the following remark upon this.]

Having tried Dr. Duckworth's method, I can bear testimony to the fact that the effect produced on patches of ring-worm by chloroform is striking, and that healthy skin and hair are not affected by it. It appears to me, however, that a similar appearance is produced, though in a less degree, on scaly patches of skin unaffected by parasitic growth; in other words, that the appearance does not depend upon the presence of the parasite, but on an altered condition of the cuticle and hair which may or may not result from it.—*Lond. Med. Record, Dec.* 3, 1873, *p.* 766.

88.—THE TREATMENT OF NON-PARASITIC SYCOSIS.

By Dr. Tilbury Fox, University College Hospital, London.

No disease, I take it, is more unsatisfactory to treat than the common inflammation of the hair-follicles of the beard and whiskers, to which the term sycosis non-parasitica is applied. On the continent, especially in Germany, the practitioner is advised to adopt epilation, and to apply some simple astringent ointment; and there is a great disposition nowadays to regard epilation as *the* remedy for the disease under notice. The reasons for epilating are variously stated. Some affirm that the inflammation in sycosis is caused by a premature development of a new hair in the follicle, and that it is necessary to its cure to rid the follicle of the old hair. Others think that suppuration extends to the root of the hair, and that epilation relieves the tension of the parts and permits the exit of the pus. The first explanation will not bear examination. The second is true in part. In non-parasitic sycosis inflammation travels downwards, and may reach the bottom of the follicle, the root of the hair being bathed in pus, whilst the hair is loosened from its surrounding connexions, and lies, as it were, a dead piece of tissue in the follicle. In such cases epilation does but get rid of the loosened hair, and its extraction allows the escape of pus that would otherwise be pent up. But in many cases the inflammation does not proceed to the extent of causing suppuration in the deep part of the follicle, the hairs are not loosened in the follicles, and their extraction gives great pain, and can do no good. Epilation is, therefore, a fit procedure only at a certain stage of sycosis—if the skin is much inflamed, the follicles freely suppurating, and the hairs are thereby loosening or loosened in them.

The treatment which I have found most successful may be summed up as follows:—In the early stage, when the follicles are very hyperæmic, saline aperients in persons of full habit; or aperient tonics, such as sulphate of magnesia with sulphate of iron, in those who are debilitated; together with hot fomentations, and simple, soothing applications which exclude the air, locally. When there is free suppuration, the same internal remedies, together with the removal, by epilation, of the loosened hairs from freely suppurating follicles, and the application of mild astringents, such as zinc lotions and ointment; and, lastly, in the sub-acute or chronic stage, where there is only a suppurating follicle here and there, but mostly a number of indurated tubercles—i.e., follicles thickened by hyperplastic growth of the connective tissue,—a course of Donovan's solution, together with, locally, hot fomentation and the application of a weak nitrate-of-mercury ointment (a drachm and a half to an ounce) night and morning. Of course, for persons of scrofulous constitutions, cod-liver oil and iron are to be given in combination with alterative remedies. I fully admit that the exhibition of Donovan's solution is in great part an empirical proceeding; but I prefer it to any other remedy, and have reason to speak with confidence as to its efficacy in sycosis when employed in the way and at the particular stage above indicated. Lastly, I may add that it is an easy matter to do harm in sycosis by the injudicious use of local irritants, which intensify the hyperæmia and the hyperplastic thickening; and I believe this to be the radical fault in the treatment of sycosis.—*Lancet*, *Dec.* 27, 1873, *p.* 902.

89.—HYPODERMIC INJECTION OF CARBOLIC ACID IN ERYSIPELAS.

Dr. AUFRECHT, of Magdeburg, having last year lost four patients of advanced age who were attacked by erysipelas of the extremities after injury, determined to try the effect of carbolic acid, and in a short paper in the *Centralblatt*, for February 21, he communicates the results which he obtained in two cases. If (he observes) it be true that erysipelas in such cases as these arises from the penetration of organisms into the subcutaneous tissue, and their multiplication there, and if carbolic acid possess the power of destroying such organisms, or of impeding their injurious influence, this substance should be able to prevent the spreading of the erysipelas, and to a certain extent diminish its danger. In order to ascertain whether carbolic acid may be hypodermically employed without any ill consequence, he experimented upon himself with a 1 per cent. solution, of which he threw in six decigrammes at a time—*i.e.*, the amount contained in an ordinary Pravaz's syringe. Neither local nor general ill-effect resulted. Since then he has employed the injection in two cases—the one a woman aged fifty-six, with erysipelas of the forearm and the hand, arising from a slight abrasion; and the other a man, aged eighty-two, with erysipelas of the thigh following slight ulceration of a cicatrix. In the first case five injections were employed during three successive days, and in the second four injections within two days. The injections were thrown into the sound subcutaneous tissue just beyond the margin of the erysipelas as it advanced towards the trunk. Its progress was at once arrested in the direction where the injections were made, the injection being repeated in consequence of some insular erysipelas appearing beyond the first injection-points. More remarkable still than this limitation of the erysipelas was the decided influence of the injections in diminishing the febrile action and the frequency of the pulse, and in inducing a general improvement in the patients' condition. Convalescence was quite satisfactory in both patients.—*Medical Times and Gazette, March* 28, 1874, *p.* 355.

90.—ON THE LOCAL TREATMENT OF CARBUNCLE.

By Dr. Peter Eade, Physician to the Norfolk and Norwich Hospital.

[Carbuncle occurring on the face is a most serious affection, and as Dr. Eade has lately had a case in which he succeeded in cutting short its progress, he records in the following paper the treatment adopted.]

In a short communication published in the Lancet for Dec. 11th, 1869, I recorded the particulars of a case of extensive carbuncle of the back of the neck which I had treated by the free application of carbolic acid to the diseased part, and especially by its free insertion into the holes and sinuses which had formed, as is usually the case, over the central portion very early in the course of the disease. An experience since that time of two or three cases of large carbuncle and of many cases of carbuncular boils, treated by the same method, has proved to me that the action of this remedy is so definite and so constantly beneficial that I have no hesitation in recommending it to the favourable notice of the profession.

My recent case was this:—A gentleman of seventy years of age, but still strong and hale, had suffered for some weeks from asthmatic bronchitis, from which he had in great measure recovered, when he was suddenly attacked (now some five weeks ago) with pain and hard nodular swelling of the lower lip near to its right extremity. Almost immediately a double festering pimple formed over the centre of this swelling, and the painful induration rapidly extended past the corner of the mouth to the adjacent cheek, and, to a less degree, along the substance of the lip, causing also considerable œdematous swelling of the whole of this part. On the second day severe constitutional symptoms manifested themselves, and feelings of extreme illness were complained of. During this day and the next the tumefaction and hardness continued to increase, the festers over the centre of the original spot opened and were moist with dirty pus, the whole of the lower lip and right corner of the mouth and the adjacent cheek became tense and swollen, and it was quite evident that a carbuncle was forming on the right side of the face. Into the centre of the two holes which had formed I now pressed with a probe some threads of lint soaked in a strong solution of carbolic acid in oil (one part to four), and I also laid a piece of lint wet with the same over their apertures, so as to supplement the small quantity which the shallow sinuses would contain. A little smarting was complained of, but the application was repeated after a few hours, and again the following day. Almost at the end of twenty-four hours it could be perceived that a check had taken place in the morbid process, but by the next day it was plainly evident that the inflammation and induration were really beginning to subside. The carbolised lint was still carefully and scrupulously thrust to the very bottom of the small holes, and from this time no further spread of the disease took place, but, on the contrary, there was a rapid subsidence of the œdema, and in two or three days more little remained but some diffused swelling of the lower lip, some tender induration at and around the seat of the original pimple, and the ragged discharging opening which had formed at the site of the primary festers. The disease was therefore stayed, and in a few days more the patient was convalescent. He is now quite recovered.

I should scarcely have ventured to instance this case as proving the aborting power of carbolic acid over carbuncular or furuncular inflammation—seeing that the disease never attained to very formidable dimensions, and that possibly it might not, under any circumstances, have increased to a very serious size or extent,—had I not witnessed precisely the same effects in other cases as were here produced by the application of the acid. My conviction, nevertheless, is that the carbuncle was not only rapidly extending, but that it would quickly have become both large and important. And I cannot help contrasting its course with that of one which I had the opportunity of seeing, in its later stages, some three years ago, where the disease attacked the same parts of the face, where only the usual treatment by poultices, &c., was adopted, and where (instead of a duration of about one week for the severe symptoms, and of about ten days more for recrudescence) the suffering patient passed through a morbid process which took many weeks to complete its course, and which not only caused great and prolonged suffering, with great temporary disfigurement, but even at one time appeared to threaten life itself.

As I have stated, I have now used carbolic acid in this way in several cases of carbuncle; and in all of them its application has been followed by a uniform and immediate check to its increase, and a speedy amelioration of the local conditions. When it has been applied early, it has plainly gone far to abort the disease; and when it has been commenced later, wherever it could be brought into contact with the inflamed and hardened tissue, there at least no further spreading has taken place, while swelling and tension have diminished, and dirty suppurating slough has quickly given place to florid healthy granulation. And, from my observation of its action, I entertain no doubt that, if it could be brought sufficiently early into contact with the spreading disease, it would be quite competent to prevent its extension beyond the degree to which it had already advanced. Unfortunately, the acid appears to have little or no influence when applied over the unbroken skin; but directly it can be brought into contact with the diseased mass, either by being inserted into the sieve-like holes, or by being applied to it after being laid open by incision, its beneficial action becomes at once manifest.

Much credit is, I think, due to the late Mr. Startin for having suggested that both boils and carbuncles might be due to the growth and spread in and beneath the skin of a parasitic development; and the efficacy of the treatment he was in the habit of adopting, the remarkable effect of carbolic acid in checking its increase, and the almost certainty with which boils in their early stage may be aborted by applying freely to the core of the festering pimple the acid nitrate of mercury, go far to prove the truth of this opinion.

It is extremely unfortunate that the carbolic acid cannot always be brought into sufficient contact with carbuncles in their early stage—partly because of the insufficiency of the sieve-like openings, and partly because the parasite-studded (?) growth may have already extended beyond the central portion over which these are situated. But even with a very imperfect application I have observed a distinct retardation of the circumferential spread when the acid has been carefully introduced into these holes, conveying to my mind the impression that some destructive influence had been exerted upon the central root or stem of the diseased mass; and in all cases which I have hitherto observed, no extension of the disease has taken place from any part to which the acid has been fairly applied. Hitherto I have only applied it through the natural openings, or after the mass has been exposed by incision or caustic; but I think it extremely probable that a beneficial effect would be produced by the subcutaneous injection of a watery solution of the acid around its edges (after the manner in which it has lately been employed in the case of spreading erysipelas and some other diseases), or possibly the spread of a large carbuncle might be stayed by drawing through it some small loop-holed drainage tubes, and allowing the carbolised fluid freely to percolate through these.

The strength of the solution of carbolic acid which I have employed has been about one part of the acid to four or five of the solvent (oil or glycerine), and its efficacy, I would repeat, has appeared to be limited almost absolutely to those parts *with which it* could be brought into actual contact; and although *it appears occasionally to* have produced injurious effects when used in large quantity, yet I have kept a large sloughing and granulating surface for days together constantly covered with the carbolised oil, without any harm arising, although the urine soon presented the peculiar blackish colour which has been several times observed during its employment.

I will only further add that, in looking over the various methods of treatment which have from time to time found favour with the profession, there seems to have been a constant under-current of feeling in favour of such remedies as have a more or less caustic or antiseptic (? parasiticide) influences Terebinthinates, resinous applications, caustic potash, and several other caustic, have been held by various competent authorities in high esteem, and their partial usefulness would seem to fortify the theory of the vital, the living nature of the disease—a theory which I think is greatly supported by the decided influence upon it of carbolic acid, a substance so well known to possess a special power of preventing the increase and development of low forms of life and cell-growth.—*Lancet, March* 28, 1874, *p.* 439.

SYPHILITIC AFFECTIONS.

91.—WHEN AND HOW TO USE MERCURY IN SYPHILIS.

By Jonathan Hutchinson, Esq., Senior Surgeon to the North London Hospital, to the Royal London Ophthalmic Hospital, and to the Hospital for Skin Diseases, Blackfriars.

As regards the precise method of administration of mercury, I confess that it does not seem to me a matter of much importance, provided it be done cautiously enough. If by the mouth, the doses must be small—at any rate at first, until the degree of the patient's tolerance has been estimated. I have seen the hypodermic injection plan pursued in Paris and Berlin, but nothing which came under my observation induced me to think favourably of it. The inunction method is safe and very efficient, but it is dirty and inconvenient, and probably nothing can be said in its favour which may not be alleged with greater force of the vapour bath. This latter method, as introduced by Mr. Langston Parker and perfected by Mr. Henry Lee, is an admirable plan. Its only drawbacks are the expense and trouble which attend it. At Aix-la-Chapelle, which may, I suppose, in some sense be regarded as a better-class Lock Hospital for the north of Europe, amongst medical men of immense experience in the management of constitutional syphilis, the inunction plan still enjoys undisputed pre-eminence. From my friends, Dr. Diemer, Dr. Wetzlar, Dr. Brandis, and Dr. Ziemssen of that place, I heard but one report: all of them believed in mercury as the one cure for syphilis, and inunction as the best method of using it.

In order that I may not fail in clearness I will, at some risk of being accused of dogmatism, put my conclusions in a categorical shape. They are as follows:—

That mercury is probably a true vital antidote against the syphilitic virus, and that it is capable of bringing about a real cure.

That, in practice, a good many cases are really cured by mercury; the cure being proved by the restoration to good health; and, in some cases, by renewed susceptibility to contagion.

That the probability of cure depends upon the stage of development attained by the disease when the remedy is resorted to, and upon the perseverance with which it is used.

That, in order to secure the antidotal efficacy of mercury against syphilis, it is desirable to introduce a considerable quantity into the system, and to protract its use over a very long time.

That ptyalism and other evidences of the physiological action of mercury, so far from being beneficial, are, if possible, to be carefully avoided, since they prevent the sufficiently prolonged use of the remedy.

That in cases in which the patient shows an idiosyncrasy peculiarly susceptible to mercury, the indication is to reduce the dose rather than to omit the drug.

That it is impossible to begin the administration of mercury too soon, and that it should be resorted to without loss of time in all cases in which a chancre shows a tendency to indurate.

That many cases of indurated chancre, treated early by mercury, never show any of the characteristic symptoms of the secondary stage.

That in other cases of mercurial cure of the chancre, in which yet secondary symptoms do occur, they are usually milder than if allowed to develop without specific treatment.

That, when mercury does not wholly abrogate the secondary stage, it exhibits a remarkable power in delaying it.

That delayed outbreaks of secondary syphilis are to be regarded rather as proof that the administration had not been sufficiently persevering than that the remedy was not efficient.

That it is probable that the risk of tertiary symptoms is in ratio with the severity and prolonged duration of the secondary stage.

That there are some grounds for believing that the tertiary symptoms of syphilis are both less frequent and less severe in those who have been efficiently treated by mercury than in others.

That mercury, cautiously given, does not, in a great majority of instances, do any injury to the general health, and that its local inconveniences may usually be prevented.

That the doctrine of the real antidotal character of mercury, in respect to syphilis, ought to lead to much more prolonged administration of it, with the hope of destroying utterly all lingering germs of the malady.

That most collected statistics as to the duration of treatment and freedom from relapse are misleading and worse than useless, because usually the treatment was far too short to be effectual.

That it has not yet been proved that there are any special forms of syphilitic disease in which mercury ought to be avoided, although, as a general rule, it is acknowledged that it must be used with more caution in all forms which are attended by ulceration than in others.

That iodide of potassium possesses little or no efficacy against either the primary or secondary form of syphilis.

That the efficacy of mercury is often most signally proved in cases which have utterly resisted the action of iodide of potassium.

That it does not much matter whether mercury is given by the mouth, by inunction, or by the vapour bath, provided that, whichever method be selected, care be taken to avoid salivation, purging, &c.

That the doses usually resorted to for internal administration are for the most part too large, and thus often necessitate a premature discontinuance of the remedy.

That if one method of administration does not proceed satisfactorily, another should be tried; and that in no case of difficulty should the vapour bath be forgotten.—*Lancet, Jan.* 31, 1874, *p.* 159.

92.—MERCURY IN SYPHILIS.

By the EDITOR OF THE MEDICAL TIMES AND GAZETTE.

The paper recently read before the Hunterian Society by Mr. Hutchinson, on the giving of mercury in syphilis, is one which merits the most careful attention of the profession, and this, too, all the more that it contains a kind of retractation of former opinions more opposed to mercury than those now entertained by the author. Unfortunately, the constant dinning of the evils of mercury into the ears of the public has not been without effect, and a kind of dread of its evils has arisen in men's minds, which experience does not justify. Not every venereal sore is syphilitic; not every syphilitic sore is venereal. The former proposition gives us the clue to the bias against mercury, for it was used in olden times not only against all kinds of venereal sores, but even against gonorrhœa. The latter commends itself mainly to medical men who run risks of infection to which the ordinary public are not liable, and to whom, therefore, personally the proper cure of syphilis is of the first importance. Again, in dealing with syphilis, men too much lose sight of the fact that there are slight cases of syphilis, just as there are severe cases—that certain cases tend to get well, just as certain cases tend to go from bad to worse; but, as pointed out by Mr. Hutchinson, even the worst cases may be benefited by mercury if judiciously given. In the giving of mercury, blue-pill has had too much place. As a preparation it is bulky, and it tends when long given to upset the stomach. Syphilis is really (as shown by Mr. Hutchinson) a kind of fever, lasting, however, much longer than do ordinary fevers, sometimes even a lifetime. If, therefore, we are to benefit our patients, we must be content to give mercury in such a way as will least interfere with nutrition—to give it, therefore, in small doses, and over a lengthened period of time. In this way perhaps the liquor hydrargyri perchloridi, in drachm doses, is one of the best modes of exhibition, for this contains only a very small quantity of mercury, and, given in a vegetable infusion, is often of the utmost benefit. But in many cases there can be no doubt whatever that inunction, or the vapour-bath, suits better than any other mode of giving the remedy; and a paraphrase of the old rule —viz., the stomach for food, and the skin for mercury—might be held to be the best mode of solving the difficulty.

As to the good effects of mercury, if skilfully given, there is nowadays such a consensus of opinion that no wise man would attempt to dispute it any more than he would dispute the value of iodide of potassium in the later stages of the malady. But the word "skilfully" implies much: it means sometimes giving large doses of one form of the remedy, sometimes small doses of another. That mercury is truly curative in many cases of syphilis, we firmly believe; nor, on the whole, are we inclined to quarrel with the following categorical conclusions by Mr. Hutchinson, namely:—

"That mercury is probably a true vital antidote against the syphilitic virus, and that it is capable of bringing about a real cure. That, in practice, a good many cases are really cured by mercury, the cure being proved by the restoration to good health, and in some cases by renewed susceptibility to contagion. That the probability of cure depends upon the stage of development attained by the disease when the remedy is resorted to, and the perseverance with which it is used. That, in order to secure the antidotal efficacy of mercury against syphilis, it is desirable to introduce a considerable quantity into the system, and to protract its use over a very long time. That ptyalism and other evidences of the physiological action of mercury, so far from being beneficial, are, if possible, to be carefully avoided, since they prevent the sufficiently prolonged use of the remedy. That in cases in which the patient shows an idiosyncrasy peculiarly susceptible to mercury, the indication is to reduce the dose rather than omit the drug. That it is impossible to begin the administration of mercury too soon, and that it should be resorted to without loss of time in all cases in which a chancre shows a tendency to indurate. That many cases of indurated chancre treated early by mercury never show any of the characteristic symptoms of the secondary stage. That in other cases of mercurial cure of the chancre in which yet secondary symptoms do occur, they are usually milder than if allowed to develope without specific treatment. That when mercury does not wholly abrogate the secondary stage, it possesses a remarkable power in delaying it. That delayed outbreaks of secondary syphilis are to be regarded rather as proof that the administration had not been sufficiently persevering than that the remedy was not efficient. That it is probable that the risk of tertiary symptoms is in ratio with the severity and prolonged duration of the secondary stage. That there are some grounds for believing that the tertiary symptoms of syphilis are both less frequent and less severe in those who have been efficiently treated by mercury than in others. That mercury cautiously given does not, in a great majority of instances, do any injury to the general health, and that its local inconveniences may usually be prevented. That the doctrine of the real antidotal character of mercury in respect ɔhilis ought to lead to much more prolonged administration of it, with ope of destroying utterly all lingering germs of the malady. That collected statistics as to duration of treatment and freedom from re- are misleading, and worse than useless, because usually the treatment ar too short to be effectual. That it has not yet been proved that there ny special forms of syphilitic disease in which mercury ought to be ed, although, as a general rule, it is acknowledged that it must be used more caution in all forms which are attended by ulceration than in s. That iodide of potassium possesses little or no efficacy against the primary or secondary forms of syphilis. That the efficacy of mer- is often most signally proved in cases which have utterly resisted the a of iodide of potassium. That it does not much matter whether mer- is given by the mouth, by inunction, or by the vapour-bath, provided whichever method be selected, care be taken to avoid salivation, purg- &c. That the doses usually resorted to for internal administration are ie most part too large, and thus often necessitate premature disconti- ce of the remedy. That, if one method of administration does not suc- satisfactorily, another should be tried; and that in no case of difficul- ould the vapour-bath be forgotten."—*Medical Times and Gazette, Jan.* 74, *p.* 99.

93.—IODIDE OF POTASSIUM IN SYPHILIS.

By the EDITOR OF THE MEDICAL TIMES AND GAZETTE.

e value of mercury in syphilis has been so amply vindicated by Mr. hinson, that we may be excused from looking longer on this side of the re; but there is another which is of equal or greater interest. As well ed out by Mr. Hutchinson, syphilis is a malady more or less approxi- ıg to the characters of an eruptive fever, and tending like these to ter- te of its own accord, with the evolution of certain symptoms. What Iutchinson has laid down as regards mercury, and what in another co- Dr. Wilks corroborates, is that mercury distinctly interferes with the tion of these symptoms, cuts the various stages short, and acts as an ote to the specific poison which gives rise to the symptoms in question. vhether the due course of the malady be interfered with or not, it tends nit itself and to come to a spontaneous end. But when the end has the individual does not cease to be liable to suffering; he ceases to be rce of danger to others, for the disease in this stage can no longer be ıgated by him—except, indeed, we accept the theory recently advanced r. De Méric, that a simple sore in such a patient is capable of propa- g syphilis. But he becomes liable to certain evils of no mean impor- such as periostitis, disease of bones, deposits of gummy matter in y all the organs of the body, rupia and spreading ulcers of the skin and parts, to say nothing of waxy degeneration of many different organs. these evils are directly consequent on syphilis, yet they are not, strict- eaking, due to syphilis; they are sequelæ, and must be treated in a to- different fashion from syphilis itself. During the period of syphilitic ion it is questionable whether iodide of potassium is of the slightest , whilst mercury undoubtedly exercises a most important influence on volution of the disease; but in this stage, which is commonly spoken of e tertiary stage of syphilis, the value of iodide of potassium is just as estionable as is the value of mercury in the earlier phases of the mala- It is a very important question for us to settle—if settle it we can— is the value of mercury in this after-stage of syphilis? Suppose we patient with well-marked rupia, with periostitic pains, and other signs rtiary syphilis, who, nevertheless, has not taken mercury, what should o? The first thing to note is that such symptoms, though usually se- e, may occur in the active stage of syphilis; and as long as syphilis is active, mercury will be of use; but as soon as the active symptoms have passed away, and the so-called tertiary stage begins, we must abandon the attempt to cure by mercury—we must give iodide of potassium. It is not always easy to say where the one stage ends and the other begins, but broadly speaking, tertiary syphilis or the sequelæ of syphilis may be laid down as beginning with the stage of gummy deposits; and wherever these exist, iodide of potassium, and not mercury, should be given.

There is one matter of great interest with regard to the giving of iodide of potassium in such cases. Under ordinary circumstances, if we give a patient a dose of fifteen grains three times a day, we shall soon have him running at the nose and eyes, and with a rash all over his skin; but in the sequelæ of syphilis we may give twenty, thirty, or even sixty grains every four hours, and only benefit accrue. There is no rash and no other symptoms of iodism. But iodide of potassium is not the only remedy to be given in such cases. Iron and quinine are always of service, as we might almost conclude *à priori* from the pallid and anæmic look of such patients; but cod-liver oil seems often of even greater value, as it is in chronic rheumatism. But, over and above these, sarsaparilla is of undoubted efficacy. Many people think little of its effects, and are inclined to sneer at its use. This most frequently arises from the mode in which it is given, for the decoction should be administered, not by the ounce, but by the pint; and, so given, its value is great.

It is therefore of the first importance to be able to recognise the stage in which a patient is at the time when seen. Whatever the nature of the symptoms of syphilis, if the disease is in process of evolution, mercury will do good; but if that be past, and only the sequelæ left, it will as certainly do harm. Then is the time for iodide of potassium and sarsaparilla.—*Medical Times and Gazette, Feb.* 7, 1874, *p.* 154.

94.—IODIDE OF POTASSIUM AND CARBONATE OF AMMONIA IN THE TREATMENT OF SYPHILIS, INTERNAL ANEURISM, AND CHRONIC RHEUMATISM.

By Dr. JOSEPH P. M'SWEENY, Carlow, Ireland.

Sir James Paget was the first to call the attention of the medical profession to the following interesting fact; viz., that carbonate of ammonia greatly increases the therapeutic action of iodide of potassium. I have had extensive experience in the treatment of syphilis, and have tried it with the best results, and find that five grains of iodide of potassium, combined with three grains of carbonate of ammonia, are equal to eight grains of the potassium salt administered in the ordinary way. The following case is a good example.

John ——, aged 50, consulted me about a sore situated on his left arm. There was a profuse discharge from it, and the smell was intolerable. On asking him a few questions, I got the following history. He had been a married man, his wife having died a short time ago; he had no children. Some years ago he contracted syphilis, and was treated by mercury, pushed to excessive salivation. The secondary symptoms had been well marked, and the sore about which he consulted me was of eight months' standing. He consulted several surgeons, and could get no relief. I ordered him five-grain doses of iodide of potassium, combined with three grains of carbonate of ammonia. After taking a few tablespoonfuls of the bottle, the bad smell altogether disappeared, as a man told me who was sleeping in the same room; at first he could not bear the smell, but after taking a few tablespoonfuls of the bottle he could detect no smell. This man remained under my care for about a month, and in that short time was perfectly cured, and in very good health and spirits. I could publish five cases with almost similar results. I have also found it of the greatest service in the treatment of internal aneurism, by relieving the pain and helping to consolidate the tumour. Dr. Frerichs has recommended iodide of potassium in the treatment of the waxy liver occurring in syphilitic patients. I think that the preceding facts are well worth the notice of the profession; but I would caution medical men how they increase the dose of the iodide of potassium, for, if the carbonate of ammonia be good, it will greatly increase the therapeutic action of the iodide.—*British Medical Journal*, *Jan.* 10, 1874, *p.* 39.

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95.—THE BROMIDE OF POTASSIUM IN THE TREATMENT OF GONORRHŒA.

By Dr. JOHN W. BLIGH, Montreal, late House-Surgeon, Kidderminster Infirmary.

[Dr. Bligh does not wish to speak too authoritatively upon this subject, as there must always be some difficulty in distinguishing the *post hoc* from the *propter hoc*, in a disease which *per se* tends towards recovery.]

Its Modus Operandi.—As the result of careful and repeated experiments, bromide of potassium is said to have the power, amongst its various other properties, of—

1. Diminishing the secreting functions of all mucous surfaces.
2. Acting as a direct sedative to all the nerves which supply the mucous membranes.
3. Increasing the flow of urine.
4. Acting as a direct sedative to the organs of generation.

It will be found to act beneficially in all the four foregoing ways. By acting as a direct sedative to the organs of generation, it prevents an increased flow of blood to the parts, thus preventing engorgement, with subsequent increased inflammation, so often followed by exudation and its train of ugly after-symptoms. Thus, through its power of diminishing the secreting functions of all mucous surfaces and acting as a direct sedative to all the nerves which supply them, it limits the gonorrhœal discharge and relieves the pain in micturition, by rendering the urethra less susceptible of irritation. In common with other remedies which augment the secretion of urine, the bromide of potassium renders this fluid less stimulating, by increasing its flow. It will thus be found serviceable throughout the whole course of the disease, but more especially in the inflammatory stage, in which so many other remedies are debarred.

This salt seems to have the power of diminishing the irritability of mucous membranes, not only when taken internally, but also when applied topically, for, according to Dr. Ringer, some writers aver "that merely brushing the pharynx and soft palate with a solution is sufficient to quell the irritability of these parts, so as to enable the laryngoscopic examination to be made with ease." Assuming such to be the case, it may naturally be inferred that a similar effect would be produced by its topical application to the inflamed urethra. This, I believe, will be found to be the case, for which reason an injection is recommended, as will be seen when its mode of administration comes to be considered.

Complications.—It may be used with advantage in some of the complications to which gonorrhœa gives rise, especially in that distressing and frequently troublesome one, chordee. For by reason of its power as a direct sedative of the organs of generation, it subdues the sexual appetite and prevents any tendency to erection, which, as is well known—when inflammation has extended to the corpus spongiosum urethræ, and the cells are glued together by the effusion of coagulable lymph—is sure to produce this painful effect.

It will also be found useful in the third stage, when the disease has degenerated into a gleet.

Mode of Administration.—As soon as a patient complains of gonorrhœa, the bromide of potassium should be immediately commenced, and continued throughout the duration of the disease. As it is said to increase the acidity of the urine, a condition not at all desirable, some alkaline bicarbonate should be combined with it to counteract this tendency. The following formula has been found useful:—

℞. Potassæ bicarbonatis gr. 60; potassii bromidi gr. 99—120; tincturæ hyoscyami f. ℥ss; aquæ camphoræ f. ℥vss. Mis. fiat mistura.

One-sixth part of this mixture to be taken three times a day, and once during the night, should the patient happen to be awake.

Care should be taken not to administer a dose whilst a meal is in process of digestion in the stomach, as it may, by neutralising the gastric juice, interfere with the conversion of the food into chyme.

If the disease is in the first stage, an injection of the salt is ordered and recommended to be used as frequently as opportunities allow. The following is the usual form and strength in which I employ it:—

℞. Potassii bromidi gr. 120; glycerini f. ℥ss; aquæ distillatæ f. ℥vss. Mis. fiat injectio.

One syringeful to be used every four hours.

When the discharge has assumed the form of a gleet, a similar injection, associated or not as may be thought advisable with some astringent, will be found useful. In addition, I am accustomed to administer during this latter stage from fifteen to twenty grain doses, three times a day, combined with fifteen minims of the tincture of the perchloride of iron, and dissolved in some suitable menstruum.

[N.B.—I see no reason why an injection of the bromide should be contraindicated in the inflammatory stage, but, on the contrary, consider its anæsthetic properties (when applied locally) as likely to be of much service. I am, however, unable to speak from experience, never having made use of it in this stage.]

When there is any disposition to painful erections or chordee, a draught containing about half a drachm in an ounce of camphor mixture, administered at bedtime, will be found to allay this tendency almost to a certainty. In this complication its effect seems magical, and has only to be tried to be recognised as a boon of inestimable value.

There are certain accessories which should not be neglected in this, any more than in any other plan of treatment. The bowels should be carefully regulated, the proper diet prescribed, and a total abstinence from beer and other stimulants insisted on. Rest should be enjoined, and over-exertion strictly avoided. The testicles should be supported by a suspensory bandage, and the genitals bathed from time to time, especially before retiring to rest. The flow of urine may be increased by the free use of diluents, as linseed tea, barley water, &c.

My object in being thus precise in detailing its mode of administration has been to endeavour to induce others, who enjoy a far wider field for experiment than mine, to make use of the drug; when I feel confident its value as an additional agent towards the cure of a loathsome disease will be appreciated, and my views herein expressed verified. In the whole range of the Pharmacopœia there is no drug which in my opinion *primâ facie* promises such happy results in the treatment of gonorrhœa. For, as has already been pointed out, not only do we find it to have the power of diminishing the secretions and assuaging the pain of all mucous membranes, but also to produce a *special* sedative effect upon the organs of generation. Undue administration has been said to give rise to debility, but exhibited in the doses here recommended, all fear of this kind may be laid aside. Indeed, from considerable experience in its administration in large doses, continued over a lengthened period for the relief of epilepsy, I am inclined to think that its ill effects have been greatly exaggerated.—*Practitioner, Feb.* 7, 1874, *p.* 100.

96.—THE TREATMENT OF GONORRHŒA BY LOCAL REMEDIES ONLY.

By Dr. Philip Foster, Leeds.

Some most eminent authorities having condemned the use of injections in the first stage of gonorrhœa, I feel that I am simply discharging a duty in recording how exceedingly successful, during an extended trial, this mode of treatment has proved in my hands. That cases do occasionally occur, as Case

2, in which the immediate use of injections would not be prudent, I willingly admit, but experience has assured me that the great majority of cases may be thus treated with the most perfect safety and success. Where mischief has followed this mode of treatment,—and I have seen instances of it, although not in my own practice,—I believe it to have arisen from injections of too powerful and irritating a nature having been used. My success in the treatment of this complaint I consider mainly attributable to the use of very mild injections; at first, say one or two grains of sulphate of zinc to the ounce of water, strengthening or changing them in a few days if necessary. And here let me distinctly state that it is not to any particular form of injection,—for one will frequently answer where another has failed,—but to the principle of treatment itself that I wish to direct attention. Three or four days of this treatment usually suffice to effect a cure, but I consider it advisable, and always recommend, that it should be persevered with for a few days after the discharge has entirely ceased. The occurrence of gleet and stricture would, I believe, be exceedingly rare after this mode of treatment.

I beg to observe that the following are not selected cases, but the only complicated ones that have come under my care since I commenced my present mode of treatment. Case 1 is inserted because it illustrates the mischievous effect of strong and irritating injections—the reason, I believe, of local treatment having fallen into disrepute.

Case 1.—Mr. —— called upon me, complaining of great pain and uneasiness in the urethra, especially when passing water. The meatus looked red and irritable, and there was a slight glairy discharge issuing from it. He had the day before consulted a surgeon at Manchester, where he resides, who had prescribed an injection of nitrate of silver, gr. j. ad ℥j., to be used three times a day. He said that this injection, which he has used twice, had made him decidedly worse. I ordered an injection of sulphate of zinc, gr. j. ad ℥j., to be used six or eight times a day. The next day he wrote to inform me that all the symptoms had entirely disappeared, and I have since heard that there has not been any return of them.

Case 2.—In this case the whole penis was exceedingly swollen, and there was great constitutional disturbance. Ordered antimony and saline aperients, sugar of lead lotion, rest, and low diet. A week of this treatment having sufficed to get rid of the swelling and fever, an injection of sulphate of zinc, gr. j. ad ℥j., was directed to be used four times a day. As, however, a week passed by and the discharge still continued, although the zinc had been increased to gr. ij., cupri sulph. gr. j. ad ℥j. was substituted for the zinc. On the fourth day after commencing with the copper the discharge had completely ceased, and the patient has remained quite well.

Case 3.—This commenced as an ordinary case of gonorrhœa. An injection o zinci sulph. gr. ij. ad ℥j., was prescribed four times a day. On the third day the patient was decidedly better, but on the fifth inflammation showed itself in the left testicle. The injection was at once discontinued, and the usual remedies for orchitis were commenced with. On the fourth day, the orchitis having disappeared, the injection gr. j. ad ℥j. was resumed, and in a week the patient was quite well. This patient, whom I have not seen since, informed me that he had suffered from inflammation in the same testicle during a previous attack of gonorrhœa, although he had not then been treated by injection: so that the orchitis cannot be reasonably attributed to the injection.—*Medical Times and Gazette, Oct.* 25, 1873, *p.* 461.

AFFECTIONS OF THE EYE.

97.—ON A NEW PHOTO-PERIMETER.

By C. S. **Jeaffreson**, Esq., Surgeon to the Eye Infirmary, Newcastle-on-Tyne.

[There is considerable difficulty in eye practice in taking an accurate chart of the field of vision with rapidity. In hospital practice most surgeons continue the old custom of making the patient fix his eye upon their own, and then rapidly moving the fingers in the different parts of the field of vision. This practice is rapid, but must leave a doubtful impression on the mind of the surgeon.]

The instrument I have devised, and which is represented in Fig. 1, enables the operator to map out the field of vision with as great rapidity as, perhaps greater than, it could be done according to the old method described above, and which is in general vogue. With its help, the greatest accuracy can be obtained; and, if necessary, the limits of the field can be immediately transferred to a chart, and kept for future reference. But it not only fulfils the duties of a perimeter in an ordinary sense; it will also test the field of vision for colours, transmitted or spectral; and, by a very simple addition, it can be made to indicate the presence of astigmatism, and show the meridian in which this exists, if present. Some other indications it can fulfil, which I shall speak of by and by, as they will be more readily understood when the instrument has been fully described.

The perimeter is a hollow hemisphere of light metal (zinc or copper), hammered out with great accuracy, its diameter being twenty-four inches. The concavity of the hemisphere is painted white, and divided into degrees of latitude and longitude. The meridians of longitude commence at the pole, and between each is a space equal to 15 deg. The parallels of latitude encircle the pole; the first being at 10 deg. from it, and each successive one placed at an interval of 10 deg. The pole itself is occupied by a circular opening about three-quarters of an inch in diameter, the purpose of which will be shortly explained, as will also some further markings on the surface of the perimeter.

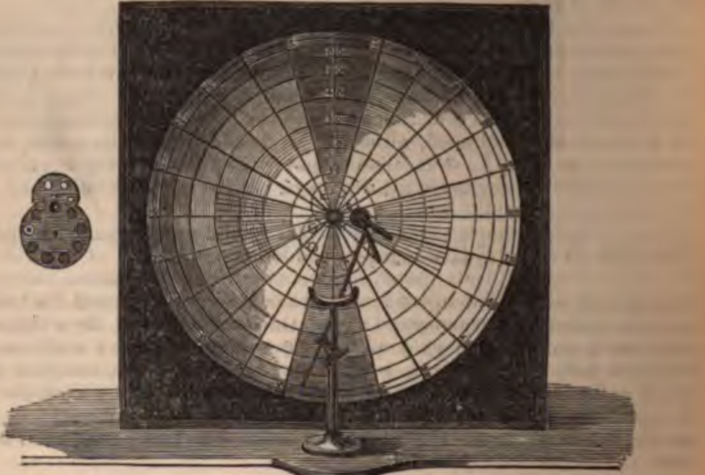


Fig. 1 represents a front view of the Perimeter, with the chin-rest and mirror *in situ*.
Fig. 2 (side figure). Upper part represents the Diaphragm, containing various sized apertures. The lower circle contains the Colour-Discs: these are attached behind the Perimeter, and are drawn in Fig. 1 with dotted lines.

In order to measure the field of vision, it is necessary, whatever the instrument employed, to have a fixation-point, and an object which can be made to travel over the parts of the retina which do not occupy the fixation-point. In the instrument which I am describing, these two indications are fulfilled in the following manner: A gas lamp upon a movable upright stand is so adjusted behind the perimeter, that the rays from it pass through the aperture which occupies the pole of the hemisphere in a diverging beam. These rays fall upon the mirror, which is (as seen in Fig. 3) attached by a rod to the chin-rest; and this mirror, being ground to a focus of a little less than twelve inches, will form a small and bright dispersion-circle upon the surface of the perimeter. To enable this disc of light to be thrown in any direction required, the mirror is attached to its connecting rod by a ball-and-socket joint. To use the perimeter for testing the field of vision, we proceed as follows. The chin-rest is placed opposite the concavity of the hemisphere; its height is so adjusted that, when the patient's chin is placed upon it, the eye to be examined is opposite the aperture at the polar extremity of the hemisphere. (Fig. 3.) The reflecting mirror is now adjusted and made to occupy a position slightly below and to the outside of the eye to be examined. The other eye is covered by a shade. By a little management, the beam of light, which passes through the aperture in the perimeter, is made to fall, not directly upon the eye which it would dazzle, but upon the mirror which reflects it, as before said, in the form of a bright dispersion-circle, upon the perimeter. The light, not falling directly upon the eye, renders the fixation-point less dazzling, and more easy to look at for some length of time.

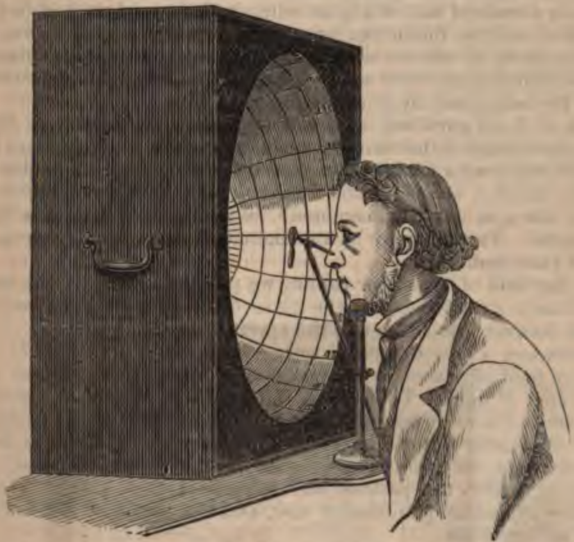


Fig. 3 shows the patient in position for taking the field. The position of the mirror is not quite exact: it should be nearer and more to the side of the eye.

When a patient is placed in the position above indicated, his eye being directed upon the central aperture, an image of the perimeter is projected upon his retina; and it is, so to speak, artificially mapped out into exactly the same divisions as are present on the instrument—the central aperture lying exactly at the yellow spot, the peripheral parts of the instrument occupying the peripheral parts of the retina. The concavity of the retina becomes, therefore, an exact counterpart (of course inverted) of the concavity of the instrument. Now, it is evident that, if a disc of light be made to travel over the surface of the perimeter, this will also travel over the surface of the retina (in an opposite direction to that in which it is passing over the instrument); and that, if it should come to a portion of the retina whose sensibility is lost or impaired, or to the natural limit of the sensitive retina, we shall know by the degrees of latitude and longitude the exact bearings and position of the spot whose sensitiveness is impaired, and where the natural limit of the sensitive retina terminates. Let us illustrate this. A patient is placed in position, with his right eye opposite the perimeter, fixed upon the central aperture spot; we wish to examine the spot or optic nerve entrance in this eye; we know that its situation anatomically is slightly to the inner (nasal) side of the yellow spot; we therefore gradually move the travelling disc outwards; and, when it arrives at a position about 15 deg. to the outer side of the central aperture, it disappears.

If we wish to ascertain the limits of the field of vision, we cause the disc to travel along the various meridians of longitude, marking the exact position in which it ceases to be visible. As in many cases it is necessary to measure the exact size and limits of the optic nerve entrance, the surface of the perimeter, in the region which this portion of the fundus occupies, has been carefully divided into spaces of 3 deg. each.

In order to enter the cases examined, charts of the perimeter can be obtained, drawn up on a scale of half or a quarter the real size. Attached to the back of the perimeter, will be found two sets of diaphragms (Fig. 2). One, the inner, contains apertures of various sizes; and the other, the outer, contains discs of variously coloured glass; also a disc which has a fine slit and revolves upon its own axis, for testing for astigmatism.

The use of these parts will now be explained. Of course, in many cases, where the sensibility of the retina is much impaired, a small aperture and a small disc of light will not be available for measuring the field; hence the large apertures. The smaller, especially the smallest, is for taking exact measure of the blind spot, the area of which can more easily be measured by a fine point of brilliant light. The intensity of the light may be varied by regulating the amount of gas burnt.

To test the field of vision for colour, we proceed in exactly the same way as under ordinary circumstances; the glass discs of the colour required being interposed between the flame and the aperture in the perimeter.

The processes above described only enable us to test what is termed the quantitative field of vision, or that portion in which a person can distinguish roughly a spot of light. To measure the quantitative field of vision, a stencil letter is inserted in an inverted position in the aperture of the perimeter. Immediately the mirror forms a bright erect image of it upon the perimeter; and the operator thus ascertains the distance from the fixation-point, in different directions, at which the patient still recognises this letter. This forms a severe but very useful quantitative test, and the shape of the test-object can be varied *ad infinitum*.

I must now show how the instrument is used in the diagnosis of astigmatism. In astigmatic persons, as is generally known, all the rays of light falling upon the eye (owing to some changes, congenital or acquired, in the refracting surfaces) are not brought to a focus on the retina at the same point. Hence if an object made up of radiating lines, like a wheel, be looked at from a distance, all the lines will not be distinctly visible. If the refraction of the eye be normal in one plane, the lines which lie at right angles to that plane will be distinctly visible, whilst those occupying it will be indistinct, or perhaps almost invisible. Now these changes, which are well marked when looking at a drawing such as we have named, become much more obvious when a luminous streak is employed. In the perimeter, a diaphragm with a slit equal in thickness to No. xx. Snellen, is placed in the aperture at the back of the perimeter, and is illuminated with the gas through an intervening portion of ground glass. This slit can be made to revolve upon its own axis, and thus pass through all the planes in which incident rays can fall upon the cornea. If the sight be normal, the slit at twenty feet has a well defined margin in all directions; but if astigmatism be present, when the slit is passing through the plane at right angles to which it exists, its contour will become blurred, and assume a shape which will vary according to the degree of the astigmatism. Thus, supposing we produce an artificial degree of myopic astigmatism in the horizontal direction by placing a + 30 cylindrical lens before the eye with its axis occupying the vertical meridian, and we then rotate the slit in front of this eye, we shall observe that when it occupies the horizontal direction its form of a slit will be retained, though it may appear somewhat elongated horizontally, but when it occupies the vertical meridian it assumes the form of a thickened band. A small point of light may be used, if required, for the same purpose, by bringing the smallest aperture of the diaphragm into the centre of the perimeter. The value of testing the field of vision for colour is greatly enhanced when the pure colours of the spectrum are employed; and I find that, with sunlight, this can very readily be accomplished. In a darkened room, a beam of sunlight is allowed to enter by means of a slit in a shutter; this beam is made to fall upon a prism, and a large spectrum is immediately formed. The perimeter is so adjusted that the band of any one colour in the spectrum shall fall upon the central aperture at the back of the perimeter; the rays of this colour will then pass through and be reflected by the mirror in the same way as usual, forming a disc of the colour which at that point corresponds to the back of the central aperture. A spectrum, when necessary, can be produced by artificial light, and then thrown upon the back of the perimeter; but, although it answers its purpose fairly, it cannot compete with the light of the sun. In the vertical meridian, it will be observed that the perimeter is marked off by fine lines into spaces each of one degree. This is to enable the operator to mark the distance of the double images from each other when diplopia is present. To use the instrument for this purpose, the perimeter is placed upon its side, so that the vertical meridian becomes the horizontal one. By means of a wand, the patient is made to indicate the position of the second image, and the number of degrees which separate it from the central image will indicate the angular displacement.—*Brit. Med. Jour., Dec.* 27, 1873, *p.* 752.

98.—CASES ILLUSTRATING THE TREATMENT OF AMAUROSIS BY HYPODERMIC MEDICATION.

By Dr. C. Bell Taylor, Surgeon to the Nottingham and Midland Eye Infirmary.

Case 1.—Henry H., aged twenty-five, moulder, Rock Valley, Mansfield, was led into my consulting-room by his mother. He appeared quite blind, and could not distinguish large articles of furniture, such as mirrors, &c., with which he was in close proximity; he could not guide himself, though he made out with some difficulty the large "A" of Snellen's type, a letter four inches long. On examination I found he was suffering from simple white atrophy of the optic discs in both eyes, and at once injected the twelfth of a grain of nitrate of strychnia under the skin of the brow; this was done night and morning for four days. On the evening of the fourth day he was rather behind time, and apologised by saying that he had been to see his mother off by the train, as he had no further occasion for her services; in fact, his sight improved so rapidly that he desired to return to work at the end of a week's treatment; I persuaded him, however, to stay a fortnight longer, after which he resumed his occupation. This young man had never learned to read, but he could readily make out letters of "pearl" type and see a considerable distance after a fortnight's treatment. He had been blind for six months and there was no constitutional cause or cerebral defect to account for his loss of vision.

Case 2.—Thomas A., aged twenty-seven, private in the 14th Hussars; discharged from the service on account of imperfect sight two years ago. At the time of his discharge he could just guide himself, but could not see to salute his officers. After his discharge he got gradually worse, until he applied to me on Friday, June 12th, 1873, when I found that he could not distinguish the large "A" in Snellen's type, that he required a guide, and that, in fact, although he had good perception of light, all useful vision was abolished. I at once treated him by the hypodermic injection of strychnia, and he speedily recovered sufficient sight to walk about alone, and was able to earn a little money by touting for a photographic establishment. After five weeks' treatment he had improved so much that he was able to take a situation as groom with my friend Dr. Forbes, of Eastwood. Here he remained until the meeting of the British Medical Association in London in August last, where he was examined by several gentlemen. In London he joined some of his former associates in the army, did not return to his situation, and I have not been able since to hear any tidings of him. The case was one of simple white atrophy of the optic discs—a diagnosis confirmed by my friends, Mr. Bader, of Guy's Hospital, and Mr. Brudenell Carter, of St. George's, who examined the patient with me.

Case 3.—Chas. E. F., aged thirty-five, from Uttoxeter, captain of a merchant vessel. Sight began to fail in July, two years ago, when at sea between Liverpool and London. On arriving in port he consulted Mr. Walker, of Liverpool; was somewhat improved at first, but was ultimately obliged to give up his profession. When I first saw him he could not see across the road or read anything less than letters a quarter of an inch in length. The lateral field of vision inwards was very limited, and there was partial white atrophy of both discs revealed on ophthalmoscopic examination. He was treated by hypodermic injections of strychnine twice daily for five weeks, after which I taught him how to use the hypodermic syringe himself, and he returned home, able to read Bradshaw or "diamond" type, and with excellent distant vision. This gentleman, who had quite given up his profession, has resumed work and gone to sea again as captain of a vessel.

It will be seen by the above cases that the treatment of amaurosis by hypodermic medication occasionally yields results almost as brilliant as those sometimes obtained by surgical operations. In many, however, apparently exactly similar we are doomed to disappointment, and it is impossible to say beforehand to which category any particular case may belong.—*Lancet, Dec.* 13, 1873, *p.* 838.

99.—TREATMENT OF INFANTILE PURULENT OPHTHALMIA.

By J. W. Hulke, Esq., F.R.S., Ophthalmic Surgeon to the Middlesex Hospital.

[The mothers of infants attacked with this disease have so commonly at the time of their confinement a puriform vaginal discharge that it seems more than probable that the ophthalmia is often due to the direct inoculation of the child's eye with the maternal discharges. For the production of the ophthalmia there need not be gonorrhœa.]

It would seem almost superfluous to tell you that you will not wisely employ alike in every case one inflexible mode of treatment. Ever keeping before you a guiding principle, you must always adapt your measures, whatever these may be, to your patient's circumstances. Thus, topical applications, which can only be safely applied by the skilled hand of a competent medical attendant, are out of place where, as in many country districts and in large poor law and club practices, great distances and large demands on the practitioner's time make it impossible for him to see the patient daily. For this reason we seldom use the pure or diluted solid stick of nitrate of silver—a remedy recommended by foreign surgeons. This, again, is the reason why ice, an auxiliary much in favour with our foreign *confrères*, is so little used by us in these cases, because, unless the ice or ice-water is constantly applied, it promotes a greater reaction—its proper use demands such incessant attention as few patients in the working and in the lower middle classes can bestow or command; besides which, ice is mostly beyond the reach of the poor in towns, and not generally procurable in the country. The treatment which has been followed for more than a quarter of a century with much success at the Royal London Ophthalmic Hospital, the oldest and largest institution in this country specially devoted to eye diseases, consists in the frequent use of alum-water (generally gr. vj. ad ℥ j. aq. dist.). First, the eye must be thoroughly cleansed from the discharge; and, as this cannot be done without some pain and struggling, it is well for the safety of the child's eyes, and in order to lessen the chance of the accidental inoculation of the nurse's eyes, to have the child completely under control. For this, it is neither necessary nor proper to give chloroform. A simple and efficient way is to place the child with its arms straight by its sides upon a shawl or on a long towel, and then swathe it round a few times in this, leaving only its head out. So swathed it cannot move, and one person, unassisted, can do all that is required to the eyes. The eyelids being now gently separated without pressing on the eyeball, the discharge should be wiped away, and the eyelashes cleansed with tepid water and small pieces of rag, which should be immediately burned. Next, if the nurse is intelligent and has not clumsy hands, the conjunctival pouch under the upper and lower eyelid should be carefully syringed out with tepid water; for this a common pewter squirt will do. When the pus is thoroughly removed, some of the alum-water should be dropped into the eye, and diffused beneath the eyelids by moving these lightly over the cornea, or it may be injected under them with the squirt. After this the eyelids are dried, and a little simple ointment is smeared along their edges in order to prevent the eyelashes becoming glued together. If the ointment is softened to the consistence of cream by warming it, it may be very neatly applied with a brush. In an acute case the use of alum-water should be repeated frequently. At first every second hour will not be too often, and then as the ophthalmia yields the intervals may be increased, until, in a couple of days or so, six or four applications in the twenty-four hours will suffice. If the surgeon can see the child daily, and he prefers to use nitrate of silver, the eyelid should be everted and cleansed in the same way, and the exposed conjunctival surfaces should be lightly wiped with the caustic-stick, or (which I think preferable) brushed with a solution of the salt—gr. x. ad ℥ j. One such thorough application a day is enough, and in the intervals a little alum-water, pure or mixed with ext. belladonnæ (in the proportion of ℥ viij. ad ʒ j.), may be dropped three or four times into the eyes. The more powerful astringents or stimulants are to be used at longer, the milder at shorter, intervals. If you employ alum-water in the way I have described, you will rarely have to regret the loss of an eye. Such an occurrence is quite exceptional, and it seldom happens unless the cornea is already sloughing when the child is first brought to you. Implication of the cornea does not necessitate the discontinuance of the astringent remedy; its use should be persevered in, because in the speedy arrest of the ophthalmia lies the best hope of averting or limiting the destruction. A small hernia of the iris through a limited breach in the cornea may generally be left to itself; it will nearly always shrink as cicatrisation proceeds. Surgical assistance is seldom required except a considerable part of the cornea has perished, and a large piece of the iris, perhaps distended by the lens, protrudes. Under these circumstances the removal of the lens will hasten cicatrisation, and give a better scar; the eye is greatly damaged or wholly lost as regards sight. For this no special operative skill is required. Chloroform may be given; the iris should be cut with a thin, sharp knife, on which the lens will escape—if not, it may be removed with any small convenient scoop. After this the eyelids should be closed, and a small pad of cotton-wool secured on them with a roller. This is only exceptionally required. Not unfrequently, however, young children from two to four years old will be brought to you who have suffered from infantile purulent ophthalmia, and have the cornea replaced by a bulging opaque white scar, termed an anterior staphyloma. Such a prominent scar may prevent the eyelids closing, it is very liable to become inflamed, and it is very disfiguring. On these grounds the abscission of the front of the eyeball may be required. For this anæsthesia is almost necessary. The eyelids are held apart with a speculum, and the eyeball being fixed with a hooked forceps, a small puncture is made into it with a pointed knife behind the line of the ciliary processes, large enough to admit one blade of a scissors, with which the bulging part is cut off, care being taken that the line of the incision lies just behind the ciliary region. The lens is generally included in the piece cut off, but if not it must next be removed. If towards the horizontal equator of the eyeball the line of the incision is taken a little further backwards in the sclerotic, the lower and upper half-circles will fall together more neatly, and will not form the troublesome angle resulting from a truly circular abscission, which sometimes makes the fitting of an artificial eye difficult, and its use painful. In little children I never sew up the wound, and seldom apply anything but water-dressing.

An occasional and very troublesome sequel of infantile purulent ophthalmia is inflammation of the lachrymal sac. I think it is more frequent where an acute ophthalmia instead of thoroughly ceasing has lapsed into a subacute chronic form. If the sac suppurates the abscess cannot be opened too early, and if after the subsidence of the inflammatory swelling, epiphora and a chronic mucous discharge continue, the lower lachrymal punctum and canaliculus should be slit up into the sac. Through this the contents may be gently pressed out several times daily; and once a day after emptying the sac in this manner a few drops of a solution of sulphate of zinc (gr. ij. ad ℥ j.) may be dropped into the corner of the eye. If the nasal duct is strictured you will best adopt an expectant treatment until the child is older.—*Med. Times, Dec.* 6, 1873, *p.* 629.

MIDWIFERY,

AND THE DISEASES OF WOMEN AND CHILDREN.

100.—ON CHLORAL AS AN ANÆSTHETIC DURING LABOUR.

By Dr. W. S. Playfair, Professor of Obstetric Medicine to King's College Hospital, and Examiner in Midwifery to the Royal College of Surgeons.

The means at our disposal for lessening the sufferings of our patients during labour must always be a subject of great practical interest to the accoucheur. The administration of chloroform during the second stage has become so established a custom among many that it is perhaps hardly necessary to say much with regard to it. The more experience, however, I have of its use, the less, I feel bound to say, do I like it as an anæsthetic during labour; and this, not because it does too little, but on account of its tendency to do more than we wish. While in certain cases, when given with judgment, only during the pains, and not until these have become strong and forcing, it answers admirably, soothing the patient's suffering without retarding her labour or producing complete anæsthesia; in others, it has an unquestionable tendency to diminish the force and frequency of the uterine contractions. I know not what may have been the experience of others, but my own certainly is that in a large number of cases it has a very marked effect in diminishing the strength of the pains, and thereby very materially lengthening the continuance of the labour. Over and over again, when the administration of chloroform has been commenced, I have observed the character of the pains completely to alter, and again recover their former efficiency as soon as the inhalation was suspended. Besides this, I have no doubt that a very continuous use of chloroform during labour has a marked effect in predisposing to post-partum hemorrhage, inasmuch as the tendency to undue relaxation of the uterine fibres continues for a time after the birth of the child. Although I by no means intend by these remarks to advise you not to use chloroform during labour, I certainly do think that it ought to be given with a greater degree of caution, and perhaps more sparingly, than the recommendations in many of our text-books would lead you to believe to be needful. The susceptibility of patients to its action seems to vary much, and therefore it is all the more necessary that its effects should be carefully watched in each individual case, and the amount administered regulated accordingly.

While, in my judgment, chloroform is apt to be too freely and incautiously used, the administration of chloral as a means of lessening the pains of labour is, I think, by no means as yet appreciated at its proper value. It has this immense advantage over chloroform, that it does not seem to diminish the strength and intensity of the pains, while it very markedly diminishes their painfulness. It has also another great recommendation, that it is chiefly applicable at a period when we would not think of administering chloroform—towards the termination of the first stage of labour, before the complete dilatation of the os, and when the sharp grinding pains perhaps produce more suffering and are less easily borne than the more forcing pains of the later stage. There is a type of labour very common, especially in women of a highly-developed nervous organisation, such as constitute a large proportion of our patients among the higher classes, in which I have found it to be especially valuable. In these, before the rupture of the membranes and the complete dilatation of the cervix, the pains are very severe, but short and ineffective, chiefly limited to the back, and producing little or no effect in dilating the os. Hours and hours of really intense agony often elapse, until the patient is wearied and exhausted by her fruitless sufferings. In cases such as these, a common and very useful practice has been to administer a considerable opiate, so as to produce some hours of refreshing sleep, after which we expect the labour to recommence with fresh vigour and effect. The disadvantage of this plan, however, is that during the action of the remedy the labour is suspended, and much time is thus lost. If, however, chloral is administered instead of the opiate ordinarily employed, the probabilities are that the same refreshing rest will be obtained without any suspension of the pains or protraction of the labour. The character of the uterine contractions will be observed to alter; they will become steady and useful, but they are not suspended. Another condition frequently associated with the former is rigidity and spasm of the cervix. Very generally in this class of cases the cervix is thin and rigid, with a sharp edge. Soon after the chloral has taken effect the tissues seem to relax, and I have not unfrequently observed a thin os, which had remained unaltered in character for many hours, dilate rapidly under the influence of the remedy, far more so than when chloroform is inhaled for this indication. It is not, however, only in cases of this kind, which may be classed among abnormal labours, that the use of the drug is of value, although it finds perhaps in them a more special application. It may, I think, be very generally and advantageously exhibited in perfectly natural labour, for the specific purpose of lessening the sufferings of the patient. When judiciously given the patient falls into a drowsy state, not quite asleep, but nearly so. She is roused as a pain begins, but suffers comparatively little; and experienced women, who have the recollection of former labours to guide them, bear strong witness to the immense relief thus obtained. I have given the remedy in this way for the past two years in most cases I have attended, and I have no reason to think that any bad effects have followed its administration. I have very carefully watched the intensity of the contractions, and I have not the least ground for thinking that it has any effect in diminishing either their frequency or their force.

The way I give the drug is as follows:—I order a six-ounce mixture, containing a drachm and a half of the hydrate of chloral. When the pains are becoming severe, and I deem it advisable to employ the anæsthetic, which is generally not until the first stage of labour is approaching completion, I give one-sixth part of the mixture,—*i.e.*, fifteen grains of chloral. This I repeat in about twenty minutes; and usually after the second dose enough has been taken to bring the patient sufficiently under the influence of the remedy. Its farther administration must now be regulated by its effects. If the patient is drowsy and relieved, a third dose need not be given for three-quarters of an hour or an hour; and then half the quantity will probably suffice to keep the patient in a sufficiently somnolent state. It is seldom necessary to give more than a third dose; and I have never given more than a drachm of chloral during the entire labour. In this way, lessening the quantity after the second dose, and increasing the intervals between their administration, a full and sufficient effect can usually be kept up for many hours. I feel certain that any who give this method a fair trial will appreciate its value.

The exhibition of chloral in this way is no novelty. It has, I believe, been recommended more than once in our journals; but, so far as I know, it has never come into anything like general use as an anæsthetic Bear in mind that it need not at all interfere with the exhibition of chloroform. When the pains get strong and forcing, that may be inhaled just as if chloral had not been given, only a smaller quantity will probably suffice. As our patients suffer less, they are also less urgent in their demands for the commencement of the chloroform inhalation; and thus there will be less likelihood of those evils I have mentioned to you being produced.—*Lancet*, *Feb*. 21, 1874, *p*. 263.

101.—THE ADMINISTRATION OF CHLORAL IN THE FIRST STAGE OF LABOUR.

By William Berry, Esq., Manchester.

On August 31st, 1870, I was summoned to attend Mrs. A., aged thirty-five years (primipara). I saw her about ten p.m., and finding she was suffering from the sharp, short, and grinding pains peculiar to the commencement of labour, the os not admitting more than the tip of my index-finger, I gave her forty minims of tincture of opium, and left her. I was sent for again an hour afterwards, but found no improvement as far as regarded the progress of the case. I ordered thirty grains of chloral every half hour until relieved. Three doses had been given when I was sent for again (this would be about one a.m., and ninety grains of chloral had been taken). I found the os was dilating, the head could be felt presenting in the first cranial position, the membranes were entire, and the pains were regular and pretty strong. The pains continued, and got stronger as labour advanced; the os was fully dilated, and membranes ruptured about 3.30 a.m. The pains gradually grew stronger, and the child was born about five a.m. Placenta came away easily, and both mother and child did well. During the whole of the time I was with her she slept soundly between each pain. The accession of a pain was marked by being aroused, and the efforts made at expulsion with a slight groan. When the pain had passed off she again relapsed into a slumber, which continued during the interval.

During the whole course of the labour there was nothing to lead one to suppose that the chloral had retarded the progress of the case. On the contrary, the dilatation of the os, the descent of the head, the character and frequency of the pains, were all that could be desired, and there appeared to be great relief of suffering.—*Lancet*, *April* 11, 1874, *p.* 532.

102.—A FEW REMARKS ON THE HYPODERMIC EXHIBITION OF ERGOTIN.

By Dr. Francis E. Clarke, M.A., Drogheda.

I have lately, in two cases of severe hemorrhage occurring with abortion in the third month of pregnancy, been led to try the effects of ergotin injected subcutaneously, and being very favourably impressed with those produced, though in so very limited an experience, I think, perhaps, it might possibly be interesting to the readers of the Medical Press and Circular, briefly to bring the topic under their consideration. In each case that I adopted this procedure, hemorrhage had occurred of such magnitude as to completely prostrate the patient and imminently imperil life. Two injections in both instances were employed. In the first case, a grain of Bonjeau's ergotin in ten minims of distilled water twice, at an interval of seven hours prior to the expulsion of the ovum, each exhibition appearing completely to control the hemorrhage; and during the intermediary period there was no hemorrhage whatever. In the second, almost total collapse had accrued; hemorrhage had existed eighteen hours; the vagina had been carefully and most completely plugged; one ovum had come away and yet the hemorrhage continued. The pulse was imperceptible in both wrists; the stomach failed to retain even iced-water. Everything imaginable had been tried, when I injected subcutaneously, a grain and a half of ergotin in eleven minims of distilled water. Not fifteen minutes had elapsed before plug and a second ovum were forcibly extruded and hemorrhage immediately subsided. About eight hours subsequently flooding again commenced, owing, most likely, to relaxation of the uterus, and perhaps retention of some portions of membrane. Again I injected the ergotin, this time two grains, with equal success toward effecting control of the hemorrhage. In case A, the skin about the seat of puncture remained inflamed and tender for a day or two. In case B, no discomfort whatever ensued. This seems to have been a source of inconvenience experienced by many of those who have hitherto resorted to the hypodermic method of administering ergot. Indeed, were it constant, it would be a serious objection to the practice in any but cases of the most exquisite emergency. The absolute purity of the drug employed and its solvent, the scrupulous cleanliness of the needle and its penetration sufficiently far enough to have the aperture freely under the skin in the subcutaneous cellular tissue, all seem to me to warrant the most scrutinising attention on the part of the operator. Hildebrand, of Königsberg, when he first suggested the subcutaneous employment of ergotin for the removal of fibrous uterine tumours, added glycerine to the formula. The liquid extract of ergot with glycerine has also been used. I cannot help thinking, however, that the simpler in form all fluids be prepared for hypodermic use, the better, and surely the less irritating; though doubtless, cherry-laurel water has lately been suggested as a vehicle on several grounds, amongst which are its non-irritating properties when in contact with the cutaneous structures. Langenbeck and Eulenberg first called attention to the subject, and I think it was ergotin they employed. It is a matter of the utmost importance to determime the most efficacious and certain form to administer the active principles of ergot hypodermically, if a more prolonged experience and its favourable reception by observers of competency should lead us to adopt this method of administration more extensively. Mr. Long, in the interesting paper published in this journal in 1870, at the time when his then new and admirable "liquor ergotæ" was being introduced, tells us how widely different are the samples of ergotin to be found in commerce. He states that Bonjeau's is an aqueous extract, excluding the poisonous oil previously thought by Dr. Wright to be the active principle, but really the agent which produces all the dangerous symptoms of that state commonly known as ergotism. If such be the case, one would decidedly imagine it to be superior to anything else of the kind. Mr. Long's liquor ergotæ, I opine, is admitted now to be the very best form for the internal administration of ergot. For my part, I don't see any other equal to it on several grounds, but it is decidedly inapplicable for subcutaneous exhibition. That the alkaloids, morphine, atropine, strychnine, are more active with respect to their physiological characters when exhibited hypodermically, there can be no question; and if ergot, likewise exercising specific power over the excitomotor functions of the spinal cord, and hence capable of modifying the contractility of the arteries generally, is similar, we have undoubtedly another great therapeutic addition to our assistance. In addition to the superiority of its action when so administered, if such superiority exist, we meet with many cases where it is impossible to administer it by the mouth; and secondly, the nausea and vomiting so frequently thus produced, might not be found to follow it subcutaneously. Its action also seemed to me to be quicker, to follow more promptly, and hence, better suited it to cases of emergency, such as those severe uterine, gastric, pulmonary, or nasal hemorrhages that are occasionally to be met. Regarding gastric irritability, it appears rather to subside than otherwise after using the ergotin. Of course, I cannot say that this was consequent thereon, but it strikes me very forcibly that it tended to supplement the action of the spinal ice-bags I had applied, with the view of modifying the reflex excitation of the spinal cord as suggested by Dr. Chapman, ergot being generally admitted to contract its vessels and diminish the afflux of blood. There is no more effectual treatment for obstinate vomiting, hiccough, or such like alarming phenomena than the hypodermic administration of morphia over the epigastrium—frequently succeeding when everything else fails; yet this seems paradoxical, when we remember how opium congests the cord, and hence might be expected rather to aggravate the visceral irritation than otherwise, by increasing its polarity and the consequent reflex excitability. This difficulty may be overcome either by showing that the physiological powers of opium over the spinal cord are not possessed by morphia or its salts, or else by adopting the hypothesis that when used in such cases it produces hypodermically only a localised action, causing relaxation of the abdominal muscles and a consequent quiescence of the internal viscera in proximity; but surely, this must be through the medium of spinal influence. The mechanical effects resulting from the morphia so administered predominate over the physiological. The removal of spastic pressure by the relaxation of the muscles, has greater power in suppressing vomiting, than the physiological effects over the cord have to the contrary; but in the case of ergotin, even if the diminished spinal vascularity is not sufficient to counteract peripheral nervous reflex excitability, it is possible that it may be regarded supplementary to the ice-bag, if used with that intention, and in any case capable of administration where its gastric retention is out of the question, as well as in the treatment of any great hemorrhages, such as uterine, pulmonary, &c. In subcutaneous application of ergot, if it proves on further trial than I think has as yet been accorded it to be worthy of reliance, we would have another means at command for "the anticipation of post-partum hemorrhage," lately so ably discussed by Dr. Lombe Atthill in the British Medical Journal, and in cases of such, owing to exhaustion of the nervous system of the mother, as laid down by that author, to be more difficult to anticipate, hence, to treat, than simple exhaustion of the uterus, immediate action of ergot (if so obtainable) would be highly important, and small quantities of strychnine, suggested by Dr. Atthill, could with equal facility be added to the hypodermic solution as to one for internal administration, and with a quicker and, most probably, a more powerful result. The hypodermic syringe is comparatively a recent introduction—the suggestion of ergotin so applied still more so; we have, therefore, to guard against being led away by novelty. Our experiences, however, so far justify continued trial, in which neither prejudice on the one hand, nor enthusiasm on the other, should be allowed to mislead the calm and reflective judgment of a thoughtful and unbiassed investigation.—*Medical Press and Circular, Jan.* 28, 1874, *p.* 65.

103.—ON A NEW OBSTETRIC FORCEPS.

By Dr. H. Lowenthal, New York.

Some of our most distinguished writers and teachers on obstetrical subjects have recommended that the study of the forceps be more cultivated, because they are the means of saving in extraordinary cases one or two human lives. The consequence of these recommendations has been the invention of a host of new forceps, all for the purpose of facilitating the application and of increasing the effect of this valuable instrument.

Some made a new lock, others changed the length of the blades, the size of the shafts, the move of the handles; others, again, thought small fenestræ in the blades would be more efficient, or broad blades would hold the fœtal head more securely than narrow ones; others finally changed the curve of the blades, made two curves, or one curve, or none at all, and so on. But to show to the profession that those hints were not lost on me, I tried to find out how it were possible to apply a forceps without subjecting the mother (and often the operator) to the difficulty of applying the second or right blade and locking the forceps; in other words, I tried to find a forceps whose blades could be introduced at once, and by which the head of the child could be caught by rotating one blade around and away from the other; and I give you in the accompanying cut an idea of the instrument I had made. I have tried it on the phantom, and finding it perfectly successful, I offer it to the profession, knowing full well that there is a great deal of room for improvement, as regards the length and width of the blades, and the curves of the same.

I shall now endeavour to show in what way my instrument is superior to all the others.

I do not think that there is any necessity for speaking of the effect of the obstetrical forceps in general, since almost every medical journal of the last year or two contains essays on the same subject, written by more able and more experienced authors, but I shall only mention that just these essays brought me to the idea of having my forceps constructed.

The forceps should act as tractor, as compressor, and as lever, and I think the arrangement of the lock of my forceps meets these claims best of all.

The lock, being solid and stationary, is more apt than any other to procure for the operator a steady traction; the blades, working around a joint, will give a higher degree of compression than any other (without being too much in that direction, because if the head were too large, the rotation of the blades could not be accomplished), and the whole instrument being straight, and the handles, working as it were like one piece, will give a greater amount of leverage, than any instrument the handles of which have to be compressed by the hands of the operator in order to steady the instrument.

So much in regard to the general application of the forceps; but there are a few new points to be remarked in my instrument, which are well worth being mentioned. First of all, it does away with the introduction of the second blade. Every medical man who has been called upon to apply the forceps will agree with me that the greater difficulty is always in the application of the second blade and the locking of the instrument. After you get the first blade in position, you will have to entrust its handle to some assistant (usually an inexperienced woman) to hold and steady it, and at the same time to keep it out of your reach, not to hinder you in applying the other blade. While you are busy in doing so, this assistant will move or let go the handle, and thereby necessitate adjustment of it again, before you can proceed with your work. Finally you have both blades in position, and try to lock them, but you find some difficulty in doing so; you will have to move either the first or the second blade, and sometimes not with inconsiderable force, thereby risking the making of abrasions on some part of the child's head. All these difficulties are done away with: you introduce your instrument as you do the first blade of any of the other instruments. There is usually very little resistance; then seize hold of both handles with one hand, and rotate the instrument, either to the right or to the left, either around the anterior or the posterior half of the pelvis, and as soon as the two blades come opposite to each other, with a click the lock will close, and you can commence traction, or leverage, at your discretion, as the case might be, without fear lest the instrument might open, or the blades might move, and without using any force to compress the handles, as with other instruments. This instrument, as I present it now, could be made of any length, but I intend to use it as a straight forceps, and shall always follow the old rule of putting the mother across the bed on her back, bringing the nates close to the edge, thereby giving the most space to the operator.

In closing this explanation I do not need to give any further description of the forceps, as the construction of the blades, or handles, or shafts, has nothing new, nor is it necessary to give further directions for the use of the instrument, since it is so simple that every practitioner who has ever handled a pair of forceps before, will at once understand the workings of this new one: the only point to be noticed, if you want to rotate the outer blade around the posterior half of the pelvis, is to steady the instrument on both sides at the horns of the handles with one hand, and rotate the instrument by grasping the lower end of the handles with the other; on the other hand if you want to rotate the inner blade around the anterior half of the pelvis (behind the os pubis) you steady only the one handle and rotate with the lower end of the other.

[This forceps resembles in its principal features one described by ourselves at the meeting of the British Medical Association in Leeds in 1869. (See *Retrospect*, vol. lxi. p. 273.) We consider that the plan of fixing the handles together so that they cannot be separated after introduction is disadvantageous, as it considerably lessens the ease with which the blades are glided round the head.—Eds. *Retrospect.*]—*New York Medical Record, Feb.* 15, 1873, *p.* 94.

104.—THE DELIVERY OF THE PLACENTA BY SUPRAPUBIC PRESSURE.

Dr. Goodell, in the Transactions of the Medical Society of Pennsylvania, writes that, judging from his own experience and from the number of laudatory papers on this subject, Credé's method of delivering the placenta, or some slight modification of it, bids fair to take the place of every other. The plan which he adopts is as follows. At the maximum of the first uterine contraction after birth of the child, the fundus of the womb is grasped through the abdominal wall, between the thumb in front and the fingers behind. It is then both forcibly squeezed, and at the same time pressed downward and backward. By means of this uterine expression, the placenta and membranes are usually at once detached and extruded; sometimes suddenly, just as the stone escapes when a cherry is compressed between the finger and thumb; occasionally it will require two or more pains to effect this. The sooner this plan is resorted to after the birth of the child, the more easy in execution will it be. Those who practice this method contend that it offers many advantages over any other. The risk of communicating any puerperal disease is lessened. The expulsion of the placenta and membranes by a *vis a tergo* is more likely to be complete than by traction on the cord, which cannot be broken, as no traction is made on it. Adherent placenta is less frequently met with. The introduction of the hand into the womb is avoided, and so also, as a consequence, is the ingress of air. Finally, the tonic and energetic contraction of the womb, following this manœuvre, prevents the occurrence of hemorrhage or of unruly after-pains.—*British Medical Journal, Feb.* 7, 1874, *p.* 175.

105.—CASE OF CEPHALOTRIPSY AFTER TURNING.

By Peter Brotherston, Esq., Alloa, Scotland.

In the following very short notes of a recent case of crushing and extraction by the cephalotribe of the head of a child left in the uterus, I have nothing to state in the way of experience which has not already fallen under observation. From this case, however, those engaged in midwifery practice, more especially in the country, may find their confidence strengthened in an instrument which has afforded a safe and easy assistance in cases of fœtal decapitation, or any case of difficult delivery requiring the head to be crushed, when other means of remedy might involve a much more tedious and dangerous operation.

Dr. Keiller has related, in the Transactions of the Edinburgh Obstetrical Society, Session 1868–69, a similar case to that of my own, which saves me recapitulating what he has written.

On the 16th of June last, I was called suddenly to assist a friend with a case in which the child's head had become detached from the body during efforts at extraction, and had been left in the uterus. With such instruments as were in use previous to the cephalotribe, the extraction of the head would have involved a tedious process, aggravating the mother's sufferings, running the risk of wounding delicate parts, and incurring delay, which is always more or less dangerous in such cases.

I fortunately had previously seen the cephalotribe in the late Sir James Simpson's hands, and was sufficiently struck with the useful appearance of the instrument, to purchase one at Mr. Gardner's, South Bridge, Edinburgh.

The patient I found rather depressed and anxious, and in no condition for undergoing a lengthened and difficult operation. The contraction of the uterus had ceased, and the uterus, with the enclosed head, had receded into the cavity of the abdomen.

Before proceeding, it was thought expedient to administer chloroform. Carefully inserting one blade of the instrument into the uterus, and then the other, I locked the blades, and proceeded to tighten the compressing screw. On withdrawing the instrument it enclosed the placenta, having slipped from its hold of the head. My friend then placed his hand on the uterus, and exerted sufficient pressure to throw the head more into the axis of the pelvis, and at the same time seized hold of the lower jaw to steady the head. Once more the blades of the instrument were inserted, locked, and the screw tightened. On this trial the head was firmly caught over the ears, and without any difficulty crushed and withdrawn from the uterus.

The antero-posterior diameter of the pelvis was much contracted, scarcely admitting four fingers. I learned that the woman had previously been delivered of a small premature dead child. She afterwards made an excellent recovery, and was moving about in her usual health in ten days.

Having obtained a photograph of the head in the grasp of the cephalotribe, the manner of the action of the instrument is discernible enough. The pressure exerted is sufficient to crush the cranium and face into a frightfully distorted form; but that is of little consequence when so successful an operation can be so easily achieved.—*Edinburgh Medical Journal, Feb.* 1874, *p.* 715.

106.—THE ANTICIPATION AND TREATMENT OF POST-PARTUM HEMORRHAGE.

Abstracts of Papers by Mr. Quirke, Mr. Thestrail, Mr. J. Thompson, Mr. Turner, Mr. Houghton, Mr. Gurney, Mr. Knight, Dr. Boulton, Dr. Edis, Mr. Parsons, Dr. Hadden, Dr. Donaldson, Mr. Woodman, Mr. Date.

Mr. **Joseph Quirke** of Birmingham states that he has employed the intra-uterine injection of iron as a *dernier ressort* in three cases of *post-partum* hemorrhage, with the effect of immediately arresting it in each case, the patients recovering without bad symptoms. In spite of firm and continuous pressure and manipulation of the uterus, the employment of cold, the application of the child to the breasts, the administration of brandy, the introduction of the hand and the injection of cold water into the uterus, the formidable loss continued, and death was imminent. The injection of a solution of iron (1 to 20) caused the uterus to contract with great energy, expelling the hand that had been passed in, together with a number of clots, not another drop of blood being lost. He considers that the risk of embolism is obviated by the firm contraction of the uterus, and the consequent closure of its vessels; and that metritis and peritonitis are not likely to occur, if too strong a solution be not employed. He thinks its employment should be restricted to those cases where all ordinary remedies fail.

Mr. **H. Ernest Trestrail** of Harston endorses the views of those who employ ergot as soon as the head is born, and carefully follow down the uterus with the hand as the child is expelled, continuing to support it until the application of the bandage after the placenta is removed, the binder having been placed in position before delivery. Where these means fail, and cold injection is not efficacious, in place of relying on pressure on the aorta or on galvanism, he advocates the injection of a solution of iron or some other powerful styptic, the danger being in using too strong a solution. He prefers chloralum, being less likely injuriously to affect the tissues with which it comes into contact, and being at the same time a more powerful antiseptic. He thinks that ergot is given in too large doses generally, having seen violent shaking, a sense of sinking, sickness, &c., and even symptoms of collapse and thrombosis, as results. Again, one of the most distressing results is powerful contraction of the uterus, producing "continuous pains," and threatening injury to the womb. He has also seen it produce irregular contraction, the cervix acting so forcibly as to interfere with the extraction of the placenta. He considers five to ten minims of the liquid extract of ergot sufficient in all ordinary cases as a safeguard against *post partum* hemorrhage.

Dr. **J. Ashburton Thompson** thinks that sufficient stress has hardly been laid, if it have not indeed been entirely overlooked, upon the administration of a full dose of ergot at such a stage of labour as, it may be calculated, will allow the drug to take effect just before the final act of expulsion. It should be given twenty or thirty minutes before delivery is expected. He has been in the habit of giving it invariably to every case in which there is no obvious mechanical contra-indication. No harm accrues to the fœtus; and the ergot has on many occasions obviated a flooding which there was every reason to anticipate, and also diminished the severity of the after-pains. If there be any septicæmic element in the case, its influence must probably be diminished by the more efficient and lasting contraction of the uterus. Special precaution should be taken to prevent any sudden evacuation of the uterus after the administration of ergot; otherwise the placenta may be with difficulty extracted. Grasping the uterus by the hand, after immersion in cold water, will both expedite the expulsion of the placenta and also lessen the tendency to hemorrhage. He then gives the outlines of eight cases occurring in his own and Dr. Brunton's practice, in which flooding had taken place in previous labours, but was obviated by the administration of a full dose of ergot in the present deliveries. In one case, the intrauterine injection of a solution of iron was resorted to without permanent effect, continuous pressure ultimately succeeding. Ergot prevented flooding on the next occasion.

Mr. John Sidney Turner of Norwood thinks *post partum* hemorrhage occurs more particularly in women of relaxed habit, where labour is long protracted, and there have been long intervals between the pains in the last stage. He inculcates avoiding haste in delivery, and waiting patiently for the extraction of the placenta, giving ergot prior to the birth of the child. Compression of the aorta should be resorted to where practicable. Galvanism with hand-kneading is the special means which he has found of very decided advantage; one hand being placed in the uterus, and the galvanism being applied externally. He considers this far more philosophical treatment than that by injection, intensifying the physiological mode by which uterine hemorrhage is arrested, and imitating Nature's process in the closest degree.

Mr. J. Hyde Houghton, of Dudley, a pupil of the late Dr. Rigby, writes in praise of the binder applied before the birth of the child, tightened as soon as this takes place. He says that, out of a very extensive midwifery practice, he has had only one fatal case during thirty-three years. During the last eight or nine years, he has carried about a binder with buckles, and has not had a single case of hemorrhage that has given him the least anxiety.

Mr. Thomas Gurney of Stoke Newington advocates ergot and pressure by the hand externally on the uterus, together with the application of cold plates dipped in water, or a sponge saturated with cold water introduced into the uterus; failing this, the introduction of a Barnes's bag and its distension with cold water.

Mr. H. J. Knight of Rotherham endorses Dr. Mushet's views on the value of cold-water uterine injection. He has employed it in several cases, and has never seen any ill effects follow this simple and effective mode of treatment.

Dr. Percy Boulton replies to Dr. A. B. Steele's strictures on his previous communication respecting the abdominal binder, and states that it is not the *bandage* for which he claims any special advantage, but *perfect rest* after delivery. He expresses also his appreciation of the value of the injection of iron-solution when hemorrhage with inertia of the uterus occurs.

Dr. Edis thinks that, in the multiplicity of methods of *treatment*, we are apt to lose sight of the importance of *anticipating post partum* hemorrhage, to which Dr. Whittle's original paper and Dr. Lombe Atthill's remarks were almost exclusively confined. Attention to the condition of the health of the patient preceding parturition is as important as the training of the athlete, and yet how seldom is it thought of! Iron and strychnia are of great service in improving the general tone of the general system. "Where we have any reason to expect hemorrhage, every precaution should be taken to obviate this, by refraining from the administration of chloroform except just at the completion of the second stage; by applying the forceps, if there be any unusual delay from inefficient uterine action—not waiting for the natural powers to exhaust themselves; by assisting the expulsive efforts by means of the abdominal bandage; by giving ergot just before the final throes; and by not hurrying the extraction of the child or of the placenta, but following the uterus down with the hand externally, allowing an interval to elapse between the birth of the child and the expulsion of the placenta. Then, again, anticipate your wants; have a second skilled assistant, to attend to the child, and so leave you free to devote your attention entirely to the mother; have plenty of ice as well as ice-water at hand, with a proper syringe and tube, and your iron-solution ready *mixed. Have some ergot* poured out, and brandy within reach. See that the *arrangement of the bed* is such as to leave you room to move. Have a hot water bottle, ready filled, to place under the lower back, thus increasing the determination of blood to the spinal cord, and so intensifying the nervous supply to the uterus. Forget not your transfusion apparatus, including a subject able and willing to supply the life-giving stream if requisite; compression of the aorta; ligatures to the extremities; hips raised above the level of the pelvis; free ventilation; and exclusion of all unnecessary attendants. Above all, a quiet and confident manner on the part of the attendant will inspire hope and ensure success; and, if every one who practises obstetrics will *think* beforehand and *act* when called upon in such emergencies, we shall less frequently have to deplore the sad issue of these anxious cases."

Mr. **Francis J. C. Parsons** of Bridgewater states that his experience is diametrically opposed to that of Dr. Lombe Atthill and Dr. Steele. He thinks that, the placenta acting as a foreign body, its speedy removal is of vital importance; and the rule he invariably adopts is to remove the placenta as soon as the child is born and the umbilical cord secured. "Placing the left hand on the fundus uteri, and gently moving my fingers backwards and forwards, accompanied by slight pressure, I feel the uterus, thus excited, to contract, and almost immediately the placenta is expelled. I continue my command of the uterus for ten or more minutes, and then substitute a pad in place of my hand beneath the binder, before applied, during the second stage. By these means *post partum* hemorrhage is with me of most rare occurrence." He agrees with Dr. Barnes that "the removal of the placenta is the first great end to be attained as a security against hemorrhage," and refers to the practice of Hardy and McClintock as corroborating this view.

Dr. H. R. **Hadden** of Rathmines, Dublin, narrates the particulars of a case where Cæsarean section was performed. "On the removal of the child and placenta, severe *post partum* hemorrhage set in. Here there was no difficulty in the fullest application of the hand to the interior of the womb, nor even to grasping the organ in both hands, and firmly compressing it; but neither this, the application of cold, nor any other means, was of the smallest avail in controlling the hemorrhage or stimulating the organ to contract and close the gaping orifice which my incision had made, till I resorted to the diluted solution of perchloride of iron, when immediately the exact appearances described by Dr. Barnes were seen by the eye; the uterus contracting; the styptic serving to seal up the mouths of the vessels and corrugate the inner surface of the uterus."

Dr. **James Donaldson** of Glasgow writes to record his experience of these cases when senior civil medical officer on the Neilgherry Hills, 7,500 feet above the sea-level, where local homorrhages were common. Epistaxis was frequent and at times troublesome, even serious and copious oozing ensuing after extraction of teeth; and the ordinary monthly flow in women, often excessive, became at times dangerously profuse. During nearly four years' tenure of office, *post partum* hemorrhage was of almost constant occurrence; but he did not lose a single case. Thoroughly alive to the great danger and responsibility, he always went prepared. In one case of rapid labour, "everything went on well, the placenta being expelled, and the uterus contracting. In less than an hour, the patient became suddenly restless and began to shiver. On removing the binder, the uterus was found to be relaxed, and internal bleeding going on. All ordinary means failing, the hand was inserted, and the clots extracted. This had to be repeated thrice, the hand being kept *in utero* for nearly an hour. The patient recovered. In a second case of natural labour, a portion of retained membrane was found to be keeping up hemorrhage, and was removed. In another case, a full dose of ipecacuanha, bringing on rapid emesis, caused strong contraction after inertia, where introducing the hand was not thought desirable. In another case, where the uterus was well contracted, plugging the vagina with sponge steeped in perchloride of iron lotion arrested hemorrhage from the cervix. Where it is necessary to introduce this lotion into the uterus, it is much more effectually and safely done in most cases by the hand guiding a sponge and applying it as required. Many cases are lost where the hemorrhage occurs from the lower part of the uterus and upper part of the cervix, and is unsuspected. The plug with iron lotion is the best and surest remedy for this."

Mr. **John Woodman** of Exeter writes to corroborate the advantages and success of the treatment by free injection of cold water, as advocated by Dr. Mushet. He has tried it in several cases, and always with success, and has found it to produce contraction of the uterus when galvanism and all other means had failed. It is essential that the water be quite cold. Occasionally he has used a little vinegar mixed with the water, but he believes that the good effects are entirely due to the *direct* application of cold to the uterus. He has never had any bad results from this treatment.

Mr. **William Date** of Crewkerne, Somerset, states that he has attended over two thousand cases of labour, and has never lost a patient from *post partum* hemorrhage. He advocates grasping the uterus and keeping up pressure —kneading it if necessary—until firm contraction ensues; pressure being kept up for an hour, if requisite. A firm broad binder is then applied. He refers to the fact of ergot not being under all conditions absorbed by the stomach, and thus failing to exert its well known action.—*British Medical Journal, Dec.* 27, 1873, *p.* 753.

107.—ANTICIPATION AND TREATMENT OF POST PARTUM HEMORRHAGE.

By Dr. W. Boyd Mushet.

I have watched with interest the remarks and opinions of leading men as to the use and efficacy of perchloride of iron and other means in the control of uterine hemorrhage. I am induced to describe the remedy adopted by myself, as I have not seen it definitely mentioned. I therefore deem it novel. I employ an ordinary stomach-pump or enema apparatus, and *inject cold water freely into the uterus*, passing the long tube well up for the purpose. I throw in the water again and again, having previously introduced my hand to ascertain that the uterus is free from retained fragments of placenta. The fundus should, of course, be grasped externally as firmly as possible.

Where there is a suspicion of the occurrence of hemorrhage, I have the apparatus prepared, water ready in a basin, and a dose of ergot on the mantelpiece; the last given as the head passes the perinæum. In one patient, on two occasions, where the flow, without hyperbole, was appallingly like a cataract, I succeeded in arresting the discharge instantly. In this case, against my wish, chloroform was each time inhaled for some hours, which, I believe, increases the proclivity to hemorrhage.

The virtues of perchloride of iron are mainly, I think, to be ascribed to its vehicle, cold water, when used with the latter. I have no dread of cold water injections; no compunctious fears of pyæmia or metritis from caustic or irritant influence on the uterine tissues or sinuses, as I have never witnessed any injurious effect from their employment, which cannot be affirmed of the perchloride, if we trust the evidence adduced at a former meeting of the Obstetrical Society.

Whilst on this subject, I would add that, in a protracted case of scirrhus of the uterus, in which periodical attacks of severe bleeding occurred, attended with great anxiety on the part of the patient, I always found injection of cold water more promptly valuable than ergot and less distressing than the plug—so markedly, that the poor emaciated sufferer, to whom an euthanasic dose would have been an act of grace, invariably begged for the injection, in which, after experience of the several remedies, she reposed utter confidence, though it produced very severe temporary shock on her attenuated system.—*British Medical Journal, Nov.* 22, 1873, *p.* 601.

108.—THE TREATMENT OF POST PARTUM HEMORRHAGE.

By Dr. Robert Barnes, Obstetric Physician to St. Thomas's Hospital.

The choice between cold and styptics depends upon our appreciation of two *entirely different* physiological conditions. Stypics will act when cold will not. When the diastaltic system is dead to irritation, when life is almost ebbing away, when no known power can compel the uterine muscle to contract and thus to close its bleeding vessels, styptics, which act chemically, will still seal up the mouths of those vessels and corrugate the inner surface of the uterus.

For want of experience, perhaps, of the more desperate forms of hemorrhage, this fundamental distinction has not made sufficient impression upon some men. I frankly accept their assurance that they have never failed to control hemorrhage by compression of the uterus or aorta, by ergot, or by cold. I hope they will equally accept my assurances that I have seen many cases where men of the highest ability and experience had failed to subdue hemorrhage by these means, and where, at this juncture, the perchloride of iron has instantly rescued the patient from impending danger.

In my *Obstetric Operations*, I have insisted as forcibly as I could upon this classification of cases, because I think that upon it rests the justification for urging a new plan of treatment. So long as cold acts, use it; but when cold fails, use iron, which will not fail. Each has its use; each must have its turn. In my book, at p. 461, 2nd ed., is the following passage. "Cold is more effectual if applied internally. Levret was, I believe, the first who used ice in this way. Perfect, says Levret, 'hit upon a very odd and ingenious expedient; he introduced a piece of ice into the uterus, which, being struck with a sudden chill, immediately contracted and put a stop to the hemorrhage.'" A graphic clinical description of reflex action before the theory was known! Tyler Smith, to whom the theory and practice were alike known twenty-five years ago, advocated the injection of iced water; and notwithstanding his early teaching and long experience, this keen observer and sagacious practitioner in the latter years of his life resorted to perchloride of iron.

I long used cold-water injections. At p. 472 of my book is the following passage. "At one time, it was my habit to wash out the uterus with iced water first. I now prefer not to do this. . . At the period when the perchloride is especially indicated, the exhaustion is generally so great that the injection of cold water is ill borne. I am inclined to think that, under the circumstances, the injection of cold water is more hazardous than the injection of perchloride of iron."

This leads us to the consideration of Dr. Mushet's statement that "he has no dread of cold water injections; no compunctious fears of pyæmia or metritis, or irritant influence on the uterine tissues or sinuses, as he has never witnessed any injurious effect from their employment, which cannot be affirmed of the perchloride, *if we trust the evidence adduced at a former meeting of the Obstetrical Society*." I have italicised the concluding sentence in order to point my assertion that the "evidence adduced" cannot be trusted. Few were there who *heard* the discussion referred to, but were satisfied that the positive evidence of good done by the perchloride far outweighed the doubtful evidence against it. A report of the proceedings of a learned society is often very like a newspaper report of a trial in a court of law. One must see and hear and know the witnesses in order to judge of the value of their evidence; and, even when we have formed a just estimate of evidence, we must be careful not to build a larger structure of deduction upon it than it will bear.

I will not contend that an injection of perchloride of iron may not be followed by pyæmia. But it is one thing to show that pyæmia has followed such injection, another to prove that the injection was the cause. But, let it be granted that the injection has caused pyæmia, is the remedy thereby absolutely condemned? Is no account to be taken of the numerous cases—for numerous they now are—in which life has been saved by it? Certainly, after severe hemorrhage, under any treatment, or no treatment, the liability to pyæmia is great. That perchloride of iron adds sensibly to that liability is not proved: unless, indeed, we take the case of a woman who, but for the use of perchloride, must have died of hemorrhage. If she have pyæmia, it may truly be said that, but for the perchloride, which enabled her to survive the immediate peril of flooding, she would not have had pyæmia. But pyæmia has often occurred after the use of cold water. Will it be said that the cold water caused the pyæmia? I will not affirm that it did; but of the frequent sequence can be no doubt. Scarcely a week passes without my having the opportunity of seeing some case of pyæmia after labour at term or abortion where iron had not been used. In some of these, experience justifies me in the belief that pyæmia would not have taken place had the iron injection been used in time to lessen the loss of blood, and thus to spare the conservative forces of the system.

I still feel justified in repeating the formula expressed at p. 474 of the work quoted.

"We have three stages of hemorrhage to deal with. 1. There is hemorrhage with active contractility of the uterus. Here the diastaltic function may be relied upon; excitants of contraction find their application. 2. There is the stage beyond the first, when contractility is seriously impaired, or even lost. Here excitants of contraction are useless; our reliance must be upon the direct application of styptics to the bleeding surface. 3. There is the stage beyond the first two, where not only contractility, but all vital force is spent, where no remedy holds out a hope unless it be transfusion; and even this will probably be too late."

The practical directions are as follows. Take a Higginson's syringe, to which is connected a uterine tube nine or ten inches long. Mix in a deep basin four ounces of the strong liquor ferri perchloridi of the *British Pharmacopœia* with twelve ounces of water; pump through the delivery-tube two or three times to expel the air; then pass the delivery-tube into the uterus, so that its end touch the fundus of the uterus; then pump gently and slowly; the styptic fluid will thus bathe the whole inner surface of the uterus.

Had I the smallest misgiving as to the value or general safety of the practice for which I am mainly responsible, I would, without hesitation, retract all I have said, and avow my error. I have not the vain ambition to set up a "currus triumphalis" of iron. But knowing that lives dear to me, and lives dear to others, have been saved by it; and still believing that the benefits far outweigh the evils springing from it, I again commend the practice to those who have the courage to try a new remedy in desperate circumstances, and who fear to let a woman bleed to death relying too long upon means sanctioned by routine.

As I have said at the beginning, this momentous question must be decided by facts. From every quarter of the compass assurances flow in that lives have been saved by it. I earnestly appeal to every man who has had a fatal case of flooding after the use of perchloride of iron to publish as full an account of it as possible. At some future time I shall endeavour to sum up the evidence, giving faithfully the records of my own observations and reflections. —*British Medical Journal, Nov.* 29, 1873, *p.* 627.

09.—ON THE USE OF PERCHLORIDE OF IRON IN POST PARTUM HEMORRHAGE.

By Dr. LOMBE ATTHILL, Fellow and Examiner in Midwifery, King and Queen's College of Physicians, and Obstetric Physician to the Adelaide Hospital, Dublin.

Since I commenced the practice of injecting this styptic salt into the uterus in the class of cases under consideration, I have not had a fatal case of *post-partum* hemorrhage, while I believe I have saved several lives which were in great jeopardy. In one case only did death subsequently occur. The patient was a lady, a member of a highly strumous family, who herself, prior to the occurrence of labour, was in very bad health. Sudden and uncontrollable hemorrhage set in immediately after the birth of the child. Not a moment was to be lost, or my patient would have been beyond the reach of medical aid. What the exact strength of the solution I employed may have been I know not, for it *was made by guess.* I injected about four ounces of it. The *hemorrhage was instantly* arrested, and the patient did well for five days; then symptoms of pyæmia set in, and she finally died. I do not believe that her death was due to the injection of the perchloride; but I relate the case as it occurred. Of this I am sure—that she would have died in a few minutes from hemorrhage, had it not been restrained by the action of the perchloride. I now always carry the perchloride of iron with me, and, if any excessive loss occurs, use it as directed by Dr. Barnes in his work on "Obstetric Operations." I believe the satisfactory results which I have met with are due to my using the solution promptly, and in not waiting till the patients were in a state of collapse. For the purpose of injecting it, I use a common syphon syringe, to which an ordinary vaginal flexible nozzle is attached. This should be carried fairly up to the fundus of the uterus. I generally find that four or five ounces of the solution are sufficient. As to the strength, Dr. Barnes recommends that four ounces of the strong liquor ferri perchloridi be added to twelve of cold water. I generally use it stronger—about two parts of water to one of the liquor. One precaution only is needed, namely, to take care that the tube is passed well into the uterus before any of the fluid is injected; otherwise the vagina will be corrugated and the os uteri contracted from the effects of the iron, the styptic will not reach the interior of the uterus, and great difficulty will be experienced in any subsequent efforts to introduce the tube. The same reason, namely, the effect produced by the action of the styptic on the vagina and os uteri, renders the application of the iron by means of a sponge difficult and unsatisfactory.

In conclusion, I believe the injection of the perchloride of iron to be not only a justifiable, but, in general, a safe mode of treating severe cases of *post partum* hemorrhage, and that it, or the use of some similar styptic, can alone be relied on in severe cases.

The great injury to which steel instruments are liable, when kept in a bag which also contains a solution of the perchloride of iron, has induced me to carry the salt in the solid form, the bottle containing it being enclosed in a boxwood case.—*British Medical Journal, Nov. 29, 1873, p. 628.*

110.—SOME REMARKS ON POST PARTUM HEMORRHAGE.

By Dr. J. Braxton Hicks, F.R.S., Lecturer on Midwifery and the Diseases of Women, and Physician-Accoucher to Guy's Hospital.

How can we arrest *post partum* hemorrhage?

Let us clearly understand what is the clinical condition with which we have to contend.

The most common, doubtless, is an uterus more or less uncontracted. The causes of this condition are many, but they may be resolved into two major classes; one, in which the uterus, from a long labour, or from a readily exhausted nerve-power on the part of the patient, is tired out and has used up its contractility, a state of things very frequently seen after craniotomy, &c.; the other, where the uterus has not been sufficiently excited, either the child has been withdrawn too quickly, or it has been expelled with too great ease. Anyhow, there has not been irritation or pressure sufficient to keep up the proper amount of reflex action after the placenta is delivered.

But how much contraction is needed to prevent hemorrhage? Certainly, the most secure condition is the little hard uterus likened to a cricket ball. But it has not so very rarely happened to me, and from conversation I find the same has also to others, that an uterus may be any thing but hard and firm, and yet no hemorrhage may result. I presume that there is a degree of contraction sufficient to arrest blood-loss, short of the hard and most desirable state. I think also much depends on the size of the sinuses patent on the inside of the uterus, a variable condition, as variable, indeed, as varicose states are. Probably, something may be owing to the state of the vascular system, both on the left side and the right side of the heart; and, again, something may be found to explain in the power of the abdomen called by Dr. M. Duncan "retentive," which, although much neutralised by the flaccidity of the walls, yet in some might neutralise the outflowing from the uterus, and this action certainly might be increased if the abdomen were bulky and pendulous, hanging over to the side. However any one, or some of them combined, may be the cause, it is certain that in some cases the uterus is considerably relaxed without the occurrence of flooding.

But, if an uterus be contracted and in an active condition, this does not necessarily ensure immunity from flooding; for, if it be distended by a piece of placenta, or a clot, or anything, or if it be prevented from well closing, as by a distended bladder, as was pointed out by the late Dr. Lumley Earle, or from adhesions, as remarked by Dr. Graily Hewitt, in any case, hemorrhage is pretty sure to arise, unless the uterus be in powerful action.

What a common case is this. A small clot, formed, perhaps, before the extrusion of the after-birth, is not expelled; the uterus, notwithstanding it is firm and hard, increases in bulk; a slight gush takes place on firm pressure, and some watery blood, or nearly clear serum, flows in small quantities from the vagina. Notwithstanding the firm and even painful contraction of the uterus, it slowly enlarges, and, upon a considerable quantity of blood escaping, the hand is introduced, and a hard firm clot is found. As soon as this is removed, the oozing ceases, and the case is safe; or should the practitioner, ignorant of the import of an increasing though hard uterus, leave the patient, he may be recalled to witness the worst terrors of flooding; for, the uterus having relaxed, all check over the oozing ceases, and the blood pours forth from every stretched out opening.

Another case occurs at times, and gives much trouble; namely, a partial relaxation of the uterus, an irregular contraction; I mean an uterus contracting on one side only at first, then that part relaxing, and the opposite side contracting, and so on. This state permits a certain amount of bleeding, which, coagulating within the uterus and gradually added to, tends to the dangers common to the presence of clots above-mentioned. These cases cannot, I suppose, be very common. I have seen about three well-marked instances.

Another condition one finds by no means rarely, namely, the alternate relaxation and contraction of the uterus. Of course, this is the natural state of the uterus after labour, but in these the intermissions of the contractions extend to more complete relaxation. A great deal of blood may be lost this way: indeed, I have seen some fatal cases, and very many women who have been in great jeopardy by it. It is, of course, a less anxious state than that of absolute relaxation; but it is, perhaps, more deceptive.

Again, where placenta has been prævia, or near to the uterine opening, we have sometimes a very troublesome loss. For the lower zone of the uterus after labour does not contract well, indeed it is generally more or less relaxed; if on this part the placenta have been inserted, it can readily be perceived how hemorrhage can arise. But sometimes it arises after placenta prævia, though in a different way. If the bleeding from the vessels be slow, and clots become adherent to the lower uterine opening, the hemorrhage from the upper part is retained, and danger from flooding thus arises; and I may remark, incidentally, that this blocking up of the uterus, although it may not produce hemorrhage, yet retains decomposing secretions, and thus adds to the *post partum* dangers of placenta prævia.

Hemorrhage after delivery occasionally occurs from lesions of the uterine walls. The more frequent of these rarer cases is the lateral laceration of the vaginal portion of the cervix. In these the blood is generally bright, particularly in the latter variety. Lastly, hemorrhage after delivery may occur from inversion of the uterus, partial or complete. If incomplete, the cause may be overlooked. I was a year or two since asked to see a case of flooding, and to bring my transfusion apparatus. The patient was dead before I arrived; I however, passed my hand to ascertain the cause, and found the upper half of the uterus inverted, feeling like a polypus just within the os. I had no trouble in reducing it.

If these causes be *duly* borne in mind, the principle of treatment is clear *enough. If the uterus* be inert, we must induce it to contract by the various plans well known to all; if we cannot do this, we must do what we can to coagulate the blood and to cause a contraction of the inner surface of the uterus by styptics locally applied. If a clot of blood, or fragment of placenta, be the cause, then its removal, either by external pressure or introduction of the hand, or by these two plans together, is the clear indication. If irregular action be present then our attempts must be directed to ensure a more perfect action.

If lesion be present in the body of the uterus, it is clear it would be highly dangerous to use any styptics if the laceration extended to the peritoneum; but, if the hemorrhage came from the vaginal portion of the cervix, if cold and pressure failed to stop it, a sponge dipped in styptic and firmly pressed on the rent would be the most sure way of arresting it.

But the categorical account in many text-books of the various modes of checking *post partum* hemorrhage fails to convey the clinical assistance required in the case; because, in most cases, either from the suddenness of the attack or from carelessness of observation, the practitioner is brought face to face with serious loss, and action must be prompt. But is everyone prepared to meet this possibility? How do many go to a case of midwifery. Of course, if, in one's rounds, one be called to a case, complete provision is not to be expected; but, when called straight from home, how few take anything with them. Thus, if the bladder be full, there is no catheter; if a languid uterus suggests the possibility of subsequent inertia, there is no secale; and if hemorrhage actually occur, then the practitioner has to meet it only with cold water and his hands. But, if we thus undertake a case, are we much better than the simple woman-midwife, who is not allowed to use secale, and has nothing besides? No one should go to a case without a catheter, secale, opium, ammonia, a good syringe, and (as far as my present experience has shown me its value) the perchloride (or a kindred preparation) of iron.

When, then, the placenta is delivered, a careful practitioner, instead of leaving the bedside, will take charge of the uterus, either by his own hand or by closely watching his assistant; and also will either keep the vulva in view, which can be done without exposure to any extent, or will frequently examine the napkins; and, if he finds the uterus relaxed in part or wholly so, if it be still enlarging, notwithstanding its firmness, or if he see hemorrhage arise, he is instantly ready to act. He will, however, have already unobservedly prepared measures in anticipation—cold water in plenty, some already poured out in a basin; and broken ice if obtainable; with the syringe charged ready for instant service. He will have some fluid preparation of secale at hand. If he possess only powder, then he will have already made a decoction. It may be that in ninety-nine per cent. of cases these arrangements will not be wanted. So much the better for the ninety-nine, but so much the safer for the remaining one to have them ready.

But, besides these precautions in ordinary cases, there are some which should be adopted in cases where we suspect, as the case approaches termination, that hemorrhage may arise. I mean after very long labour, much exhaustion, a languid action of uterus, where the interval between the pains is long, the pain short, but the progress during it rapid. In these cases, it is always better to give secale just before the head comes on the perinæum; and, where the child glides out rapidly with slight pains, it is better to carefully check the descent of the head, so as to rouse the uterus by an impediment. It is also better in these cases not to hurry away the placenta. Its presence acts as a stimulus to the contracting power of the uterus; and, if the uterus be not expanding, nor hemorrhage occurring, no reason exists for its removal. But of course the medical attendant must not leave the uterus to any one; it must be under his grasp the whole time; and specially he must not drag on the funis, but be ready, as soon as hemorrhage appears and external grasping fails to extrude the placenta, to pass the hand boldly in and remove it. I think that, among the causes of after-relaxation of the uterus, is the removal of the placenta from the hesitating uterus. Of course, if the hand in removal be passed rather roughly, and the internal surface excited by the free touching, it compensates somewhat for the removal; otherwise it, like a premature removal of the fœtus, is apt to leave a flaccid uterus.

Supposing, then, that after the placenta has been delivered, and we have left the uterus in charge of the nurse's hands, or after the application of the binder (which latter has the great fault of concealing rather than revealing) we find the patient pale, and the uterus largely distended, we should instantly pass the hand on to the uterus firmly and quickly, and press it till, the blood having escaped, it is sufficiently small enough to be grasped, which should be done firmly, accompanied by friction of its surface. I always pass the hand into the vagina, and remove the clots; and, if the flooding have ceased, I merely keep up the grasping and friction on the uterus; but, if any hesitation to contraction be present, I apply cold, externally, by the hand or napkin dipped in cold water; at the same time I inject cold water into the vagina. Should, however, these plans fail, or the hemorrhage be profuse, I pass at once the hand within the uterus, and remove every clot, examining the whole inner surface to see if any portion of placenta or membrane have been left attached. It seldom occurs but that the uterus contracts and remains so, but should it relax, I repeat the clearing out (for clots are almost sure to be there), and carry the syringe jet to the fundus and throw in cold water; or, if ice be at command, I carry in a lump and rub the inside of the uterus all over with it; this is generally a most efficient remedy. If the uterus fail to contract or to remain contracted, the condition of the case is very urgent. The hand should be carried in again, and the uterus well titillated; of course, during the whole of this action the exterior is to be firmly grasped. Unfortunately, the blood-loss has lessened the chance of contraction; but, fortunately, in very many cases at this crisis, syncope steps in, time is given for coagulation in the uterine vessels, and the patient is saved. It is generally those patients who never faint that die, if we cannot stop the flooding. Only two measures in this desperate state of things, remain to us; the first of which is pressure upon the aorta and vena cava. A diaper can be folded hastily into a firm pad, and pressed on the centre of the abdomen, about the navel; the patient, if not already on her back, should be so placed. I would recommend that the legs be not elevated, because when the return current of the vena cava to the heart is stopped, if the legs were lifted, the blood would regurgitate to the uterus, and still more be lost. I think this plan should be more frequently adopted than it is. Like fainting, it permits coagulation.

Hitherto, I have been considering the more severe kinds of flooding, where we have no time to temporise, no time for secale, galvanism, and emetics. But in all cases of flooding, where grasping and friction externally, coupled with cold injections into the vagina of cold water, and secale, fail to produce cessation of the flooding, the best and most secure plan is to pass the hand into the uterus, and remove anything that there may be found distending or tending so to do; cold can then be carried within the uterus by syringe or ice, when in most cases the bleeding ceases. If, however, we find a clot with an active uterus, we may spare the patient the passing the hand within the uterus, if we break up the clot as far as two fingers can reach, and employ pressure from without. If, however, the bleeding continue, it is shortest to pass the hand inside the uterus, and remove anything which may keep up the bleeding; and if we find no fragment of placenta or adherent clot, we should examine for a lesion. If there be none, and the bleeding still continue, after the application of cold, particularly if there be partial or intermittent relaxation, then we must try aortic pressure; and this failing, our only resource is to apply a styptic to the interior of the uterus. But I see that a writer in the Journal, in antagonising the use of perchloride of iron, says that, with one or the other plan, we can secure contraction, and therefore have no need of a styptic. I must say this remark fills me with astonishment; and I think it must also others, quite as alive as I am, and as he is, to the importance of establishing reflex action; and using all the remedies as sedulously as he recommends. Like Dr. Barnes, whose area of work embraced much of the same classes as my own, I hailed with a feeling of, I may say, extreme satisfaction, a plan which would give some hope of help, when every other attempt to establish uterine contraction had failed.

In my own practice, I employ a rather weak solution at first—one of the weaker liquor ferri perchloridi to six or eight of water. If that fail, then I make it stronger; but, if the flooding be rapid, I use the full strength of the weaker solution or the tincture, because much of it is lost in the blood, or is prevented from coming easily into contact with the walls. The tube of the syringe should be carried to the fundus, and the fluid gently injected; or it may be conveyed with a sponge, which should be passed over all the internal surface. Perhaps this is the safest mode; but it must be removed promptly, or the uterus may retain it, as occurred in one case I know. I have tried it in all kinds of *post partum* hemorrhage, after removal of clots, which is important, or they become coagulated and firm, and cannot be removed afterwards without trouble. I have found it particularly satisfactory in cases of irregular and intermittent relaxation, causing the uterus to assume its proper form at once.

I do not enter into the discussion as to how it acts. It stops blood in hemorrhage from a wound on the outside; I presume it acts similarly on the inner side of the uterus—of course not requiring general uterine contraction. I have no doubt, in the less inert conditions, it rouses the remnants of contraction, and acts as cold and the presence of the hand. In conclusion, I will add that, till any one can show a better remedy, or till I find it of greater disadvantage than extreme loss or death, I shall continue to use it in the fullest confidence, or till it has been clearly pointed out that it is dangerous to use.—*British Medical Journal, Jan.* 17, 1874, *p.* 74.

111.—THE TREATMENT OF POST PARTUM HEMORRHAGE.

By Dr. D. M. Williams, Liverpool.

I have had eighteen years' extensive midwifery practice, during which time I have attended about four thousand cases—yet *I never lost one* from *post partum* hemorrhage; never had occasion to introduce ice into the vagina, nor to inject cold water. I remember only one case in which I was compelled to introduce my hand into the uterus to check hemorrhage. The means I have employed are the usual ones already referred to by other correspondents, with two trifling additions. 1. After the birth and separation of the child, I grasp the fundus uteri, turn the patient on her back, and, with the first pain, generally succeed in expelling the placenta, and thus completing the labour in from five to ten minutes' time. Then I make the patient comfortable, remove all wet clothes, and put on a tight calico binder next the skin. 2. If hemorrhage occur, and remain unchecked by the usual means, I flex the leg (nearest the edge of the bed) on the thigh, get the bed-clothes slightly raised, and fan the patient freely with cold air. She feels the air colder than the water, for obvious reasons, and the hemorrhage is soon checked.—*Brit. Med. Journal, March* 21, 1874, *p.* 379.

112.—SOME REMARKS ON UTERINE HEMORRHAGE DURING THE PUERPERAL PERIOD, ESPECIALLY WITH REFERENCE TO THE TREATMENT BY THE INJECTION OF STRONG STYPTICS.

By Dr. T. Snow Beck, F.R.S., Member of the Royal College of Physicians.

[The injection of strong styptics into the cavity of the uterus, with the object of arresting hemorrhage after parturition or abortion, has of late attracted considerable attention. Dr. Hugh Norris, of South Petherton, was one of the *first*, in this country, who directed attention to the use of these styptics. Dr. Norris remarks: "The injection was only recommended in cases of urgency and peril." The opinions of other writers on the effect produced by the injection on the uterus may be gathered from the following quotations:]

"In a few minutes the uterus, and especially the os, was firmly contracted" (Dr. A. B. Steele).—"In two minutes the uterus was felt gradually contracting around the hand; with the contraction the bleeding ceased, and the coagula passed out of the uterus with the hand as the uterus contracted. In five minutes the uterus had freely contracted" (Dr. T. Chambers).—"As I injected I felt the uterus contracting around my hand, and not a drop more blood was lost" (Dr. W. S. Playfair).

In cases where a fatal result has followed the injection of the perchloride of iron into the cavity of the gravid uterus, and where an opportunity has been afforded of examining the uterus after death, it was evident that only partial contraction had followed the use of the styptic. Sufficient contraction had been induced to arrest the flow of blood from the arteries; but the veins or sinuses remained open and pervious, and were found to contain an ink-black coloured fluid, which gave abundant evidence of iron by the usual chemical tests.

Although this means of inducing contraction of the uterus, and thus arresting post-partum hemorrhage, was at first recommended and used as a "dernier ressort" in cases of great peril, and where other remedies were said to have been employed and had failed, yet of late it has been employed in all cases of post-partum hemorrhage to the exclusion of all other remedies. It has also been employed to prevent the occurrence of hemorrhage which had been anticipated from the experience of former labours in the same individual. And so far as we at present know, no reasonable objection can be urged to the employment of these strong styptics; provided the one essential condition be attained—the complete and permanent contraction of the gravid uterus. For it appears a matter of little consequence by what means the contraction is produced; so long as it be complete and permanent, as to close the arteries, and obliterate the canals of the veins or sinuses, and prevent any injurious absorption, the woman is safe, and will make a good recovery. But the danger in the use of these agents, as shown by the fatal cases which have followed the employment of them, appears to consist in only partial contraction being produced, by which although the hemorrhage was arrested, yet the veins or sinuses remained open, and permitted the perchloride of iron to be taken up and conveyed into the general system. The consequence of this, as shown by the fatal cases, has been that symptoms of blood-poisoning supervened in the course of a few days, ending in the almost inevitable death of the individual. For according to our present knowledge, once these substances have been introduced into the general system to any amount, there are no means by which the fatal effect can be arrested. In some cases no perceptible contraction has followed the injection of even a strong solution of the perchloride of iron; and in others the uterus has again relaxed. Hence arises the question, Are we justified in the employment of these substances, which may fail to produce contraction, or, in other cases, complete contraction, and in failing be followed by such deadly results, when other means may be employed, not followed by such serious consequences, and by which the same end may be attained—the complete and permanent contraction of the gravid uterus?

I have endeavoured to show that when the means in ordinary use, as they are termed, are efficiently and properly used, complete and permanent contraction of the gravid uterus may be effected, the hemorrhage arrested, and all injurious absorption prevented.

As an illustration of the manner in which the usual means of inducing contraction are frequently employed I may quote the following case from several similarly treated which have come under my notice.

Mrs. D., aged forty years, a Mexican by birth, of small stature, dark hair, and dark complexion, was confined of her seventh child in September, 1870. All her previous labours had been natural except the second, at which the perineum had been ruptured, and extended into the rectum. She had not any control over the rectum, yet had been living with her husband in this state, and having children. She was very well in general health, only could not eat anything; was low, weak, and very low spirited; and so persuaded she *would not survive* her approaching confinement that she made all *arrangements for her decease*. Lingering pains commenced about 5 a.m., and at 10 a.m. the physician accoucheur who had engaged to attend her was sent for. On his arrival, he said all was going on well, and ordered her to take champagne and some food; the latter of which, however, she could not take. At twelve o'clock the physician accoucheur returned, and in consequence, he said, of finding her so low, administered chloroform, applied the forceps, and at 1 p.m. extracted the child apparently dead, but in about half an hour it recovered. The lady remained quiet, being still under the influence of the chloroform, and at the end of half an hour the afterbirth was removed. About ten minutes after the removal of the afterbirth flooding of a bright red blood began in torrents—"it simply poured out." Ice was freely passed into the vagina until it would not admit any more; all the clothes were taken off the bed, and ice applied to the outside of the abdomen; the doors and windows were opened; and another physician accoucheur was sent for. After his arrival the galvanic battery was used, but without any effect. The body was now stripped naked, and water poured upon the abdomen from a height until the bed and the room were swimming with blood and water. Quantities of brandy were given as long as she could swallow, and when she no longer could do so, brandy and ergot of rye were twice injected into the stomach. But the hand of the accoucheur was never passed into the uterus after the hemorrhage had commenced; nor was there any pressure at any time applied to the uterus from the outside of the abdomen; when about 3 p.m. the lady was declared dead, but continued to gasp at intervals until 4 p.m.

The most strenuous advocates for the use of styptic injections do not deny that fatal cases have occurred after the injection of the perchloride of iron to suppress post-partum hemorrhage, but every possible excuse has been put forward to palliate these deaths when they have taken place. It is a frequent excuse to say that death arose from "pyæmia" or "septicemia," which might arise from various causes; at other times it is said to have been caused by "puerperal fever," or "puerperal peritonitis," and "other grave complications;" it has also been attributed to "shock," or to any other cause the circumstances of the case would admit of. I have already recorded one case, with a somewhat detailed account of the appearance presented by the uterus and appendages, where the death could not reasonably be attributed to any other cause than the absorption of the perchloride, and consequent blood-poisoning, and I may now add two other cases.

Mrs. A. H., aged twenty-eight years, was confined of her third child on August 25th, 1870. The confinement was perfectly natural, the afterbirth large, but readily removed, about 1 p.m., by the physician accoucheur in attendance, and accompanied by the loss of a great deal of blood. A bandage was applied round the abdomen, and all considered perfectly satisfactory. About 3 p.m. the nurse observed the lady looking pale; apparently sleepy; and then noticed a stream of bright red-looking blood trickling from the bed to the floor. She directly called the physician accoucheur, who was still in the house, and who endeavoured to arrest the hemorrhage, by introducing into the vagina three pieces of ice successively, each about the size of a goose's egg, and flapping the abdomen with wet cloths. This treatment was continued about fifteen or twenty minutes; but no pressure was at any time applied to the uterus. The lady was becoming exhausted, and the respiration sighing; when, at the end of the time named, the uterus was injected with the perchloride of iron. The hemorrhage was stopped at once, and never returned. Brandy and champagne were very freely administered. The lady soon recovered, and was pretty well for two days and a half, sitting up in bed, and making doll's clothes for her children. The perchloride of iron was injected only once; and what afterwards came away from the womb was of a dark reddish-brown colour, destroying everything. No coagula passed. The lady was very well on the night of the second day; but about 8 p.m. of the third day, or about sixty-five hours after the injection, she called the nurse and simply said she was dying and sinking fast, and requested the physician to be sent for *directly*. As the lady did not appear to be ill, they tried to delay sending, but *finally* did so. The physician accoucheur came, but did nothing, saying there was a clot in the uterus to come away; but none came. There was not any *pain* complained of. In the afternoon of the same day the abdomen began to swell, attended with pain, and hot flannels were applied. The swelling of the abdomen and pain increased, with great flatulency. A long elastic tube was passed into the bowels from time to time, to remove the flatulency, when much wind passed, and gave great relief for a time. Each time this tube was introduced, a thin, quite black fluid escaped with the wind; sometimes as much as half a chamber-pot full at one time. The abdomen kept gradually swelling; then both arms gradually swelled to as large as the thighs; the hands being also puffed up. The legs swelled, but never to much extent. She could not move the arms, nor herself in bed. The swelling kept gradually increasing during six weeks. She was incessantly talking; fearfully irritable; and screaming to be moved every few minutes, day and night, in consequence of the severe pain after being a short time in one position. She could not move any part of the body; slept very little; took very little nourishment; but quantities of brandy and champagne. Very little lochia, or any discharge, passed from the uterus, except some pieces of membrane at the early part of the illness. Up to the last fortnight she knew the nurse and her children when her attention was forcibly directed to them; but during the last fortnight she knew very few around her; though still dreadfully irritable, and desirous to be moved every few minutes. Towards the end she lay in a muttering, talkative delirium, but knew no one. During the last twenty-four hours the swelling extended to the throat, and she died in her sleep, without a word, on October 4th, 1870; or the fortieth day after her confinement and the injection of the perchloride of iron.

This case, which was also conducted by a professor of midwifery in one of the largest medical schools, presents most, if not all, of the objectionable points in treatment noticed in the preceding case. It was also one of a numerous class of cases where post-partum hemorrhage takes place; cases where the uterus contracts sufficiently to expel the child and throw off the placenta, yet afterwards gradually relaxes, and within a space of a few minutes, is so far relaxed as to allow the blood to escape from the torn utero-placental arteries. A healthy young lady was confined with a perfectly natural labour, though accompanied by a considerable loss of blood, which is usually indicative of a want of efficient contraction of the uterus. No precaution was taken, beyond the application of a binder, either to maintain the contraction which had taken place, to render it more complete, or to prevent the organ becoming relaxed. About two hours afterwards a stream of bright red-looking blood was noticed trickling down to the floor. The professor was immediately recalled upstairs, and endeavoured to arrest the hemorrhage by introducing three pieces of ice into the vagina and flapping the abdomen with wet cloths. Had I the least doubt as to the accuracy of this account, I should question whether any one in the position of this gentleman could be so ignorant of what was required to be done as to pursue such practice. Brandy and champagne were freely administered, which, by stimulating the action of the heart, would only enable it to force out more blood, to augment and maintain the hemorrhage. As might be expected from such treatment, the hemorrhage continued, and in about fifteen or twenty minutes symptoms of exhaustion, with sighing respiration appeared. As in other cases, the usual formula would, without doubt, be reiterated, that the remedies in ordinary use had been diligently applied, and had failed—whilst in point of fact nothing which ought to have been done had been done, and much which ought not to have been done, as the free administration of brandy, had been done. At this juncture a solution of the perchloride of iron was at once injected; the hemorrhage was directly arrested, and all appeared *couleur de rose* for two days and a half. At the end of this short time an inward feeling told her she was dying and sinking fast, though no appearance of illness was observable to those around her. It does not appear why a clot should have been supposed to exist in the uterus; none, however, came away, nor was any pain complained of. In the afternoon of the same day symptoms of blood-poisoning commenced, the abdomen began to swell, attended with pain. The swelling and pain increased to such an extent that it was considered desirable to introduce a long elastic tube into the colon to relieve the distension. Quantities of brandy were given. The arms gradually swelled up until they became as large as the thighs, the swelling finally extended to the throat, and the lady remained for six weeks in a distressing state. She took little nourishment; was incessantly talking; fearfully irritable; incapable of moving herself in bed; and screaming every few minutes, night and day, for her position to be altered in order to gain a little relief from the change of position. For some time she knew those around her when her attention was roused; but during the last fortnight she knew very few, lying in a muttering talkative delirium, and finally passed quietly away in her sleep.

No post-mortem examination was wished for, and none was made. Yet there can be no reasonable doubt, from the experience of other cases where an examination of the body was permitted, that the general system was poisoned by the absorption of some of the perchloride of iron which had been injected into the relaxed gravid uterus. And though the symptoms which were developed greatly resembled those which attend pyemia or septicemia, yet there were destructive characters which render it evident they were not produced by the same cause. Swelling of the arms has been noticed in other fatal cases after the perchloride of iron had been injected; also the escape of thin ink-black colour fluid from the bowels. But the quantities which came away in this case were unusually large, and so far as I know, this peculiar black fluid has not been noticed except in those fatal cases which have followed the injection of the perchloride of iron.

Dr. Graily Hewitt, during the discussion at the Obstetrical Society, referred to "one case, in his own practice, where the perchloride of iron injected had been used to restrain hemorrhage, and the patient had subsequently died," and remarked, "after three days pain set in, the lochia became arrested, and the patient died from puerperal peritonitis and other grave complications five weeks after delivery. Whether this result was in any way due to the action of the iron was a question." (Obstetrical Journal, April, p. 45). But where are there any evidences of "puerperal peritonitis," in such cases as these? What "other grave complications" could so suddenly spring up in a healthy young woman, if not those consequent upon her confinement? Can there be a reasonable doubt that the death in this case was caused by the perchloride of iron which had been injected into the uterus, which, whilst it arrested the hemorrhage, was afterwards taken up into the system, and caused the death of the individual more certainly than the hemorrhage it stopped? Had means been adopted to insure and maintain perfect contraction in this case immediately after the use of the styptic, it is most probable the fatal hemorrhage might have been prevented; but the danger of employing these substances is still very apparent.

Reasoning upon the experience obtained by the injection of the perchloride of iron, and considering that the chief danger appeared to arise from the serious effects of this substance when conveyed into the general system, the question was naturally suggested, whether some stimulating astringent could not be thought of which would excite the uterus to contraction when injected into the cavity, but which would not be followed by such serious consequences supposing a portion did become taken up into the general system. In large towns it is generally possible to procure a supply of ice at all seasons of the year; but in the country it appeared desirable to have some agent which could be generally obtained, and which might be resorted to as an additional means of inducing contraction. It occurred to me that ordinary vinegar might fulfil these indications; and with this view I have employed it on some six or eight occasions, in the proportion of one part of vinegar to two or three parts of water, with very good effect and encouraging results. For example:—In a case of prolonged labour with the third child, which I saw after the young woman had been in labour many hours; where the uterus was acting strongly, and the "woman quite worn out;" the forceps was employed in consequence of the large size of the child's head, and the relative small capacity of the pelvis. The placenta was thrown off by the uterine contractions; but after removal of it from the vagina, considerable, but not serious, hemorrhage followed, which was not arrested by the injection of cold water, pressure with the hand from the abdomen, aud two doses of ergot of rye. One part of vinegar, with two parts of water, was then injected into the uterus, and the hemorrhage was at once completely arrested. And so complete was the uterine contraction induced, that the nurse affirmed there was none of the usual lochial or red discharge afterwards. Certainly none was apparent at the subsequent visits. In an abortion between the third and fourth month, where considerable hemorrhage had been going on for more than four hours, and which had not been influenced by the remedies employed, one part of vinegar with three parts of water were injected into the cavity of the uterus. The hemorrhage was at once arrested, and the woman had an excellent convalescence. In this case also the usual lochial or red discharge was again absent; and the same result has been observed in other cases.

I do not mean to say that this is the most suitable agent which can be thought of as an additional means of inducing contraction; for it is not improbable that when attention is directed to the point, a more certain and more efficient means may be found out. But it certainly has acted very well so far; and it is not probable it will be followed by the same serious consequences as the solution of perchloride of iron. I would also suggest the desirability of using a weaker solution with cold water after every labour, in conjunction with the administration of the ergot of rye, to induce complete and permanent contraction of the gravid uterus; and thus to obviate, as far as possible, those distressing puerperal complications which too frequently follow childbirth.

[Dr. Snow Beck is evidently not aware that vinegar has been used some time since in the way he here mentions. The late Professor Davis, in his admirable work on Obstetric Medicine published in 1836, p. 1065, writes:—"The principal chemical irritant usually employed for the subduction of uterine hemorrhage after delivery is an injection made with one part of vinegar and two of water conveyed into the uterus with a syringe, which, according to the testimony of some of the most respectable members of the profession in our own country, must be possessed of considerable power to suppress floodings."—Ed. O. J.]—*Obstetrical Journal, Feb.* 1874, *p.* 712.

113.—CASES WHERE DEATH HAS FOLLOWED THE INJECTION OF THE PERCHLORIDE OF IRON INTO THE UTERUS TO ARREST POST PARTUM HEMORRHAGE.

By Dr. T. Snow Beck, F.R.S.

Case.—Mrs. J. H., aged 23, of rather small stature, of fair complexion, blue eyes, and light hair, was taken with lingering pains on the evening of May 19th, 1872. Her husband had left her some time previously. She was very desponding; did not care what became of her; complained of a general feeling of lassitude; had pains in the limbs, headache, and no appetite. Her urine was thick, high coloured, and small in quantity. The pains continued much the same till the evening of the 21st, when they became stronger, and the orifice was dilated to the size of a florin. She slept pretty well during the night. In the morning the pains increased, and the membranes broke; after this the pains became strong and rapid, and the child was born about noon on the 23rd. The vagina was hot and tender. It was necessary to introduce the catheter. A good deal of blood was lost, and the placenta did not come away; after waiting about half an hour, the hand was introduced; the orifice was not contracted, and the placenta was situated at the upper part of the cavity; the uterus did not contract strongly in the hand, and some blood was lost in withdrawing the placenta. The pulse was very quick, about 130. A binder was applied. After about half an hour, the patient was observed looking very pale; and, on inquiry, said "something warm seemed to be running away." On examination, considerable hemorrhage of a bright red colour was taking place from the vagina. Firm pressure was now made by grasping the uterus; cloths dipped in cold water were applied to the vulva, and flapped on the abdomen; ice could not be procured. The hemorrhage lessened, but still continued; the patient became very faint, and the respiration sighing; cold water was now injected into the uterus. As the blood still continued to flow, about half-a-pint of solution of perchloride of iron, one part of strong solution to six parts of water, was injected into the cavity of the uterus. The hemorrhage was very soon stopped; the faintness passed away, and the binder was reapplied. Everything seemed doing very well until the evening of the 26th, the third day, when she complained of feeling very ill; an extra blanket was put on the bed; a dose of chloric ether, compound spirit of ammonia, and lavender was given. The patient became warmer; perspired freely, and slightly delirious in the night. About 4 a.m., a very black clot came away.

In the afternoon of the 27th, the fourth day, the pulse was very small, and so quick it could not be counted. She lay on the back, with the knees drawn up. The abdomen was full and tender. Light nourishment, with milk, was given; warm fomentations, with turpentine, were applied to the abdomen; and an enema with castor-oil and turpentine was administered.

On the 28th she was much better. The abdomen was not so distended; the enema brought away much flatus. On the two following days she was not so well.

I saw her on May 31st, the eighth day. She was lying on the back, with the knees drawn up; the face was slightly flushed, the expression anxious. The eyes were hollow and surrounded with dark areolæ. She was quite conscious, but, the nurse said, wandered and talked all night. There was slight pain at the top of the forehead; the skin was warm and dry; the pulse small, 140, slightly wiry; the abdomen much distended, smooth, tympanitic; there was considerable tenderness all over, but more so at the hypogastrium and left iliac region. She could not extend the legs, on account of the pain, nor turn on either side. The lips were dry; the tongue was coated with a thin yellow fur. She was very thirsty; had no sickness; the bowels were moved twice, the stools being loose, very black, but not offensive; much flatus passed from the bowels, and the lochia were free and of a dark brown colour. She was taking light nourishment—beef-tea and milk. Five grains of carbonate of ammonia were given in infusion of calumba every four hours; and turpentine stupes were applied to the abdomen.

I saw her again on June 3rd, the eleventh day. She had been getting worse. She was lying on her back, with the shoulders low and the knees drawn up. Respiration quick, 42 in the minute; pulse 135, very small; skin warm, dry; face slightly flushed. She did not notice anything, yet could be roused; apparently dosing, muttering, and talking, occasionally giving a deep sigh and crying out. She took any drink when offered, but did not ask for anything. There was no sickness. The bowels were moved into the bed, the stools being fluid, and very black. The abdomen was greatly distended; it was apparently tender, as she cried out when it was pressed. The upper part of both arms was swollen, and appeared to be tender when pressed.

She continued in much the same state, becoming more difficult to rouse, appearing to lie in a dosing, muttering, delirious state, occasionally crying out, and quietly died on the morning of June 7th, the fifteenth day after her confinement.

The body was examined on the afternoon of the following day, June 8th. The weather was rather warm. There were no signs of decomposition. The abdomen was much distended, tympanitic; the arms were swollen, and the veins near the chest full. The intestines were much distended with flatus, especially the small intestines; their coats were thin, not injected. The stomach was apparently natural. Only a little brownish-yellow fluid was found in the abdomen; there was no exudation of lpmph anywhere in the peritoneum. The uterus lay in the pelvis; it was removed for examination. The blood which flowed from the pelvic veins when divided was of a deep brown colour. The lungs, heart, liver, and kidneys were all apparently healthy, but were only superficially examined.

The uterus was large and doughy; it measured nearly 5 inches long by 4¾ inches broad at the fundus. There was some slight leaden marking at each side; the veins were not much congested. The Fallopian tubes and ovaries were apparently healthy. On cutting through the posterior walls of the uterus, different sinuses were divided, which contained a thin, opaque, ink-blackish fluid. The inner surface of the organ was covered with dark brown mucous secretion; the tissue beneath was of a deep reddish brown colour mottled with black. When washed with a stream of water, the surface was uniform, with the usual reticulated appearances of the inner portion of the contractile tissue. Studded over the previous situation of the placenta were numerous larger and smaller round black spots, which proved to be the orifices of the sinuses, containing a similar opaque ink-blackish fluid. Incisions made into the substance of the walls, at and near the seat of the placenta, divided several sinuses, all containing more or less of a similar opaque ink-blackish fluid. No remains of any coagula were anywhere observed. Some of the arteries were picked up with the fine point of a needle, and were found to be open, free, and without any appearance of coagula in their canals.

Case.—In the afternoon of October 2nd, 1872, I received a small box by railway, from a medical friend in Yorkshire, containing the uterus and appendages of a patient, and accompanied with the following history.

"The wife of a farmer in this neighbourhood was confined of her fourth child on September 7th, 1872. She was a healthy woman, and had been quite well up to the time of her last confinement. The labour was lingering; and, as I had been with her a long time, I applied the forceps and completed the delivery. As the placenta did not come away, I introduced my hand and took it. I applied a binder as usual, and all appeared going on nicely. I was just leaving the house, when the nurse called me and told me my patient was flooding, and I found a good deal of bright red blood running away. Mrs. W. was pale, but not faint; I passed my hand into the uterus and removed the clots. I then gave a full dose of ergot, applied cold water externally, and, after a time, injected cold water into the uterus, but the flooding still went on. After a time, I made the nurse press upon the uterus, and mixed a strong solution of the perchloride of iron, which I had with me, with six or eight parts of water, and passing a large male catheter into the uterus, injected half-a-pint or more of the iron mixture. The flooding soon stooped. I applied the binder again, gave a drachm of the fluid extract of ergot, with some brandy and water, and left. All went on very well till the third day, when Mrs. W. said she did not feel at all well. She did not look bad; but the abdomen was full and tender when pressed, and the pulse very quick. She took a dose of castor-oil, and I sent her some carbonate of ammonia and chloric ether, in a bitter infusion, to be taken three times a day. The next day she was better; the day after, not so well. Then the abdomen became more swollen, and was very tender; the pulse very quick and small. She was very thirsty; had no sickness; the bowels were loose, moved twice a day, and very dark coloured; and a great deal of talking, and wandering during the night. She took her food very well; but lay on her back, with the knees drawn up, in a dreamy state, mumbling and wandering. She gradually became worse and worse, until she knew no one, and died quietly on September 28th, the twenty-first day after her confinement."

The uterus was large and doughy; it measured full 5 inches long by 4¼ across. There was no exudation of any kind on the peritoneal coat; the veins were a good deal congested; the Fallopian tubes were a good deal injected, but otherwise healthy, as were also the ovaries. On cutting through the posterior wall, different sinuses were divided, which contained some thinnish black-looking fluid in their cavities. Some dark red brown coloured fluid was found on the inner surface, which, when washed away, left the tissues of a dark red brown colour, mottled with ink-black. The seat of the placenta, and the space round it, were spotted over with black spots, which were the openings of the sinuses, filled with a similar black fluid. Incisions into the walls divided several sinuses containing a similar black-coloured fluid. No coagula were found anywhere, so far as the examination was made. The arteries were not examined.

It is singular how these cases are a mere repetition of the same facts, with only slight modification; but this probably arises from their being a repetition of a natural process occurring in healthy women, and not being the result of any previous disease. After the completion of a natural labour, the uterus becomes sufficiently relaxed to allow the blood to escape from the open ends of the torn arteries. The means which are adopted to induce the contractile tissue to again contract and close the canals of these vessels, either fail to provoke contraction, or only induce partial contraction; and in either case the hemorrhage continues, when a solution of the perchloride of iron, of varying strength, is injected into the cavity of the uterus. After the injection of the perchloride, the hemorrhage ceases at once, or very soon afterwards; the patient revives; and all things appear to be going on favourably till about the third day, when the usual series of fatal symptoms commence by the woman complaining of feeling very ill. The symptoms, once begun, continue gradually to increase in gravity, and at the end of two, three, or four weeks, the patient appears to fall asleep as death closes the scene. In the examination of the body afterwards, all the organs are found in a healthy condition; the intestines are greatly distended with flatus; there is little injection of the peritoneum, or effusion into the peritoneal cavity; the uterus is found large, and the uterine sinuses more or less filled with an ink-black coloured fluid, which, from its similarity to the secretion on the inner surface, leaves no doubt that some of this fluid has entered these canals through the open orifices at the inner surface of the uterus; and, as fluids are known by experiment to pass readily from the vena cava along the uterine sinuses, and to escape at the inner surface of the organ, it appears to admit of no reasonable doubt that the fluids at the inner surface have followed a contrary course, have entered the open orifices of the sinuses, been conveyed into the general system, produced blood-poisoning and death; all the other uterine tissues being found in a perfectly healthy state. A similar sequence of events, and similar *post mortem* appearances, are known to follow in cases of puerperal septicæmia—so long known under the term of puerperal fever—the only difference between the two conditions being in the nature of the fluid taken up from the inner surface. In septicæmia, it is evidently the natural secretions formed at the inner surface of the uterus which are found in the sinuses. "On cutting through the uterus at the side of the placenta, a large quantity of puriform matter flowed out, as though an abscess had been laid open; this followed each section." (Obstetrical Journal, p. 443.) After the injection of the perchloride of iron, it is generally the secretion at the inner surface, mixed with the perchloride, which is found in the sinuses; and it is not improbable that some of the symptoms observed in cases where death has followed the injection of the perchloride, are the effects from these noxious secretions, modified and rendered more fatal by the addition of the perchloride of iron which had been injected. I do not know of any instance where the perchloride of iron has been probably taken up without being mixed with the secretions from the inner surface, except in the case which occurred in Dr. Barnes's own practice, and which no doubt his modesty prevented him recording, where the injection, in a woman of strong wiry constitution, was directly followed by severe pain in the hypogastrium, and death in about seven hours. The deadly effects of the perchloride, when introduced into the general circulation, are also shown in those cases where a few drops have been injected into a nævus in infants, and death has followed in a few minutes. In one case, the "*post mortem* examination showed that the point of the springe had penetrated the transverse facial vein, and that the blood in the right cavities of the heart had been immediately coagulated."

The whole mischief in these cases of *post partum* hemorrhage appears to turn upon the single point of the want of proper contraction in the gravid uterus; and the sole question in the treatment upon the best means of inducing and maintaining complete contraction. Want of contraction, or subsequent relaxation, which practically is the same thing, allows the blood to escape from the arteries; want of proper contraction permits the canals of the sinuses to remain pervious, the noxious fluids to enter them, and death to result. Hence the effect which immediately follows the injection of the perchloride into the uterine cavity becomes of considerable importance. In the previous cases, all the tissues of the uterus were perfectly healthy; there was not the least appearance of any local action, beyond some of the tissues being stained of an ink-black colour. The perchloride is known to be a powerful stimulant to the contraction tissue, and to induce permanent contraction. And I conclude that the good effect which follows the use of it arises from the effective contraction which is produced; for, when complete and permanent contraction is induced, the woman appears to be safe from all ill consequences, and will make a good recovery. But it is also known that even the use of the perchloride is sometimes not followed by any perceptible contraction; or by only partial contraction, by which the flow of blood is arrested, and the way to injurious impregnation of the general system left open.—*British Medical Journal, March* 7, 1874, *p.* 301.

114.—THE ANATOMY OF THE PLACENTA.

By Dr. T. Snow Beck, F.R.S.

The account originally given by Dr. William Hunter, and known as the Hunterian doctrine, is perfectly accurate, that the cavernous structure of the placenta is in direct communication with the canals of the sinuses or veins, and that these vessels carry the blood away from the placenta, which is brought there by the utero-placental arteries. Such being the facts, there is no such thing existing as this "feeble wall" or "delicate membrane," either *at* or *forming* the line of separation; nor are there any structures, whatever name may be applied to them, or wherever their exact situation may be said to be, which in any way obstruct or prevent the flow of the blood direct from the placenta into the uterine sinuses or veins. Nor is there any such membrane ever seen *at* or *forming* the inner surface of the uterus after parturition, which has any influence on "possibilities of hemorrhage," or obstructs or prevents the absorption of any fluids which may exist on or at the inner surface.—*Obstetrial Journal, Feb.* 1874, *p.* 743.

115.—A CONTRIBUTION TO THE THEORY AND PRACTICE OF TRANSFUSION.

By Dr. Robert Barnes, Obstetric Physician to St Thomas's Hospital, Examiner in Obstetrics and Diseases of Women to the Royal College of Surgeons.

[Although by the topical application of perchloride of iron the cases in which transfusion becomes necessary may, we hope, become very rare, yet it is a subject the consideration of which is still important.]

The following case, although not successful, is not without interest in several points:—A short time ago I met Dr. Devereux, of Tewkesbury, at a case of extreme exhaustion from secondary post partum hemorrhage. At one time it was thought the patient was dead, so utter was the prostration. The pulse rallied now and then, but often flagged so that it became imperceptible; the respiration was laborious and frequent, so loud that it could be heard in the adjoining room; the face and extremities were cold; the voice almost extinct. She swallowed beef-tea and port from time to time, but soon vomited all. This was about 8.30 p.m. The extreme prostration, and the hopelessness of getting any nutriment absorbed either by stomach or bowel, led us to look upon transfusion as the last hope. Discussing the means of accomplishing this, we found the best we could do was to use an aspirator-syringe. This had to be fetched at a distance of five miles. In the meantime I had got everything ready. The gardener offered an arm. At 10 p.m. the operation was carried out. The syringe being made on the principle of the stomach-pump, with reversible action, answered extremely well. We filed down the point of the aspirator-trocar, and this served very fairly for insertion into the patient's vein. *As circumstances left no choice between immediate and mediate transfusion, so were we compelled to defibrinate* the blood. The man who yielded the blood was fat, and his veins small, so that it was with difficulty we obtained four ounces. This was defibrinated by whipping with a silver fork and filtering through a cambric handkerchief. Believing that one factor in the danger attending these cases of prostration is the merely dynamic one arising from the absence from the heart and vessels of fluid to act upon, and considering that we had so small a quantity of blood at our disposal, I first filled the syringe with a solution of phosphate of soda, carbonate of soda, and choloride of sodium, at a temperature of 100° F. Pumping this through the syringe also served to get rid of air from the apparatus. The vein in the patient's right arm was found by pinching up a fold of skin, transfixing it transversely to the course of the vein, and then dissecting through a little fatty cellular tissue. It was then pinched up by forceps, and a sufficient opening made into it. The trocar was passed one inch and a half into it. The injection was then slowly made. The blood was partly mixed with the saline solution; altogether about six ounces of fluid, including all the blood, was thrown in. Almost instantly the pulse improved, warmth spread over the body, the face and manner became more natural, the respiration became tranquil, and we were tempted to hope that she might pull through. We prepared everything for repetition of the transfusion in the morning should the condition be favourable. But the rally was short; the respiration again became moaning, rapid, and laboured; the pulse flagged, and the patient sank at 1.30 a.m., about three hours after the injection.

Amongst other reflections suggested by this and similar cases are the following:—

1. The vomiting of the beef-tea and wine, which had been accumulating in the stomach from repeated exhibition in small doses, in an unchanged state, and the obvious failure of any restorative effect, are proofs that the vital power is so low that nothing is to be hoped for in the way of replenishing the empty circulation from absorption.

2. The rapid and laboured respiration is evidence of the craving of the system for oxygenated blood. Air goes in and out of the lungs with great rapidity, but it has nothing to act upon. Its obvious effect is to accelerate the cooling of the body.

3. When we see things at this point—extreme exhaustion, feeble or imperceptible pulse, rapid laboured breathing, non-response of the system to nutriment and stimulants administered by the stomach or rectum, and vomiting and great agitation,—we may fairly conclude that direct injection into the vessels is indicated.

4. Blood may be used either whole or defibrinated. The choice will be governed by circumstances. If we have Aveling's or other proper apparatus to practice transfusion from arm to arm, so that the blood passes direct from giver to receiver without ever being exposed to the air or allowed to rest, this will often be the preferable plan; but where the proper apparatus is wanting, as it is likely to be, defibrination is better.

5. The experience of Dr. Little in cholera cases, and other considerations, show that saline solutions may be injected into the veins with advantage. I believe that they may, with great utility, be used to supplement or to dilute the blood used in transfusion for loss of blood, and that not only is the saline fluid useful by virtue of its chemical properties, but also by its physical quality of bulk, supplying a volume of fluid upon which the heart and vessels can act more effectually.

6. That transfusion will be more and more extensively resorted to appears to me certain. Its extended use, however, must greatly depend upon a general understanding of the conditions that call for the operation, and of the various modes of performing it. So long as it is considered necessary to have a special apparatus, the operation must be performed rarely. But let it be understood that blood either whole or defibrinated will do, and that almost any syringe can be made to answer, and the great obstacle to the extension of the operation will have vanished. Nothing can be more easy than to furnish every stomach-pump case with a flexible tube to which a transfusion-canula is attached. As no practitioner of medicine ought to be without a stomach-pump, the *necessary* apparatus will never be wanting. I should not hesitate to use an ordinary Higginson's syringe.

7. Here, as in all great emergencies, an essential condition is to *act in time.* Just as the perchloride of iron may be used too late, so may transfusion or injection be delayed until the nervous centres will have lost their power of responding to the fresh pabulum supplied.—*Lancet, Jan.* 3, 1874, *p.* 6.

116.—THE TREATMENT OF VASCULAR GROWTHS OF THE URETHRA WITHOUT OPERATION.

By Dr. Arthur W. Edis, Assistant Physician to the Hospital for Women, Soho Square.

[The treatment of these vascular growths, which are generally near the urethral orifice, and are productive of great suffering, is exceedingly unsatisfactory.]

The application of the pernitrate of mercury is attended by much pain and inconvenience, and cannot always be resorted to. Nitrate of silver, in my experience, is not sufficiently potent; it produces too much irritation, and does not sufficiently destroy the vascular growth—in many cases seeming to increase rather than diminish it. The actual cautery cannot well be applied unless the patient be at home; and the various other agents, such as potassa fusa, nitric acid, &c., are too painful to be of general application.

To obviate this difficulty, I was induced to try the effects of *chromic acid*, and was surprised to find how little inconvenience it occasioned, and at the same time how efficacious it was in destroying the growth. I have in numerous instances applied a saturated solution, and with the most marked benefit. My usual plan is to roll a little cotton-wool round the end of a small piece of wood (an ordinary match will do); to soak it for a quarter of an inch in the acid solution; and then to carefully yet firmly press it upon the vascular growth until the surface becomes distinctly shrivelled; the surrounding parts being guarded by cotton-wool soaked in a solution of carbonate of soda, which is also applied to the growth subsequently, to neutralise any excess of the acid. Slight burning pain is produced, but seldom sufficient to need any employment of morphia, either hypodermically or as a local application.

The sensitiveness of the tumour is almost completely destroyed, some soreness being all that is complained of. In about a week's time, a second application should be made; and, depending upon the size of the growth, two or more subsequent applications, at a week's interval, will generally be sufficient to complete the cure, which, as far as I have been able to observe hitherto, is permanent. *Carbolic acid* in the solid form, or as a saturated solution, also serves to mummify these growths, and causes far less inconvenience than many of the numerous agents recommended. My experience with this is not yet sufficiently great to enable me to speak more positively, but I believe it will be found to be a very useful addition to our list of remedies in these cases. I append a few cases where the chromic acid had been employed.

Case 1.—J. C., aged 37, married sixteen years, without children, was first seen on October 16, 1873. She complained of smarting pain on micturition, was obliged to get up several times in the night to pass urine, experienced much pain on walking or exertion, had a slight discharge, occasionally tinged with blood, and suffered much inconvenience *in coitu*. On examination, a vascular growth was discovered bulging from the urethra; it was sessile, being attached to about two-thirds of the circumference of the passage; it was exquisitely sensitive, and bled readily when touched. A saturated solution of chromic acid was carefully, but thoroughly, applied, the pencil being pressed firmly on the surface. Some slight inconvenience was caused at the time, but this passed off after a few hours. A mixture of bicarbonate of potash and tincture of belladonna was prescribed, together with a soothing lotion.

On October 23rd, she reported that the pain on micturition was less; the urethra being still vascular, the chromic acid was reapplied. Bromide of potassium was given in place of the other mixture.

On November 6th, the chromic acid was again resorted to, and caused little or no inconvenience.

On November 20th, the urethra being still somewhat vascular, a strong solution of carbolic acid was applied, after which no further local means were resorted to.

Case 2.—A. H., aged 63, married forty years, without children, on admission, September 18th, 1873, complained of scalding pain on micturition, which had been present two years. She had a constant discharge, and the linen was often stained with blood; she lost a good deal a week since; she thought she had a "gathering in the womb." On examination, a vascular growth was detected at the orifice of the urethra, very sensitive. Chromic acid was applied; very little inconvenience was caused by it, and, when seen on October 3rd, she stated that she had been better ever since her first visit. The growth was much smaller. The acid was again applied.

On October 16th, she had no pain at all on micturition, and felt better in every respect. The urethra was still somewhat vascular, but as there were no symptoms, nothing further was done; and up to January 1874, she had had no relapse.

Case 3.—E. R., aged 47, married sixteen years, the mother of three children, was admitted July 3rd, 1873, complaining of pains in the womb and lower part of the back, pain in micturition and also difficulty, and leucorrhœa. The uterus was found to be normal, but the urethra was very vascular and sensitive. There was no distinct growth projecting, but a general injected condition for about two thirds of the circumference of the urethra. Chromic acid was applied, and again on the 17th, when she stated that the pain on micturition was very much less than it had been before she came.

On the 31st, she had very slight inconvenience on micturition, and, as the vascularity had greatly diminished, no further application was made.

When seen in September, she had no pain whatever on passing urine. She was suffering from a threatened miscarriage.

On October 2nd, the urethra was found to be perfectly normal. She is now nearly six months advanced in pregnancy.

Case 4.—A. J., aged 63, widow, mother of eight children, on admission on April 24th, 1873, complained of smarting pain on passing urine and difficulty in doing so, bearing down pains, and discharge. She has had "pressure and cutting pain with the water for the last five or six months, disturbing her very much at night." The urethra was found to be very vascular, the mucous membrane much hypertrophied. A zinc point was inserted on two occasions, and left in the urethra for some minutes. This caused some pain at the time, but afforded relief afterwards. However, as the vascularity still continued, on June 12th, the pernitrate of mercury was applied.

On July 12th, the burning and smarting pain in micturition still continuing, chromic acid was applied, and produced much less inconvenience than the pernitrate had done.

On July 24th, she stated that she had experienced little or no pain after the former application, and had been much better since. The urethra was much less vascular, but not quite healthy. The acid was reapplied, since when she has had no further symptoms.

Case 5.—M. S., aged 66, widow, when admitted in October 1872, complained of "pain in her privates," desire to micturate every ten minutes, the act being accompanied by much smarting pain. There was a discharge purulent in character, and other symptoms referable to the uterus. On examination, a large vascular tumour, sessile on the posterior wall of the urethra was discovered; it bled readily when touched, and was very sensitive.

An operation being deemed requisite, no local application was made. Belladonna with an alkali was prescribed internally, and chloral and morphia at night. No bed being obtainable, the pernitrate of mercury was applied on two occasions, at a week's interval.

On November 15th, chromic acid was resorted to in place of the pernitrate, as the pain from the latter lasted several hours after its employment. It was repeated at the end of a fortnight, again on December 12th, and on January 9th, 1873, and February 11th. After this, the growth disappeared entirely, and required no further application, though the frequency of micturition was still more than natural during the night. She was seen in May 1873, and was very much improved, there being very little irritation now.

Possibly this may be nothing new to some present; but as it seems to be a very effectual mode of dealing with these troublesome little growths, I thought it not unworthy of the attention of the Society, as I am convinced it is not generally known, and it deserves a more extended trial.

The cases I have selected are only a few of those in which I have employed it; in many instances, the patients have attended some few times and then ceased to do so; it is, therefore, fair to conclude that they were relieved from their sufferings, otherwise they would have again presented themselves.—*British Medical Journal*, April 4, 1874, *p.* 449.

117.—THE TEACHINGS OF THE GENERAL INFIRMARY AT LEEDS WITH REGARD TO THE OPERATION OF OVARIOTOMY.

By C. G. Wheelhouse, Esq., Senior Surgeon to the Infirmary.

Bearing in mind that many of these tumours are of a mixed character, partly solid and partly fluid, we may yet, as a rule, classify them generally under these two heads. Accordingly, in examining an ovarian case, I always direct my attention, in the first instance, to this point; for I know that, according as I have the one or the other kind of tumour to deal with, not only will widely different lines of treatment be required, but that as different an opinion also, as to the advisability of any operation being performed, will probably hang thereon.

Take, for instance, the case of a purely solid tumour first, and consider how great are the difficulties which, on the very threshold of our inquiry concerning it, may await our diagnosis. Consider how many things there are which *may* be, and every one of which *have* from time to time been, confounded with ovarian tumour; pregnancy, extra-uterine fœtation, uterine fibroma, or the many forms of internal disease or displacements to which the uterus is liable; periuterine hæmatocele, fœcal accumulation, mesenteric tumour, enlargement and disease of the kidney, enlargement and disease of the spleen—and you will not wonder when I tell you that I have learned, whenever a purely solid tumour is presented to me for examination, that I must never set the case down as one of ovarian disease, until, by an exhaustive process of elimination, I have assured myself that it can be nothing else.

The degree of fixity or mobility of such a tumour is a point to which I have learned to attach great importance, and also the presence or absence around it of ascitic fluid. I long ago learned to believe, and, up to the present time, I see no reason to doubt the correctness of the conclusion that, with a view to ovariotomy, no more favourable arrangement of circumstances can present itself than a solid ovarian tumour, freely movable, and surrounded by ascitic fluid; whilst on the other hand, a solid ovarian tumour, fixed and immovable, I invariably approach with the most serious consciousness of impending difficulty. It is easy, I think, to understand why this should be so. If a solid ovarian tumour be tied in position by adhesions, the probability is, that such adhesions will be pelvic and posterior; they will be situated at the very points at which it will be most difficult to separate them, and, even supposing this successfully accomplished, the raw surfaces produced will occupy the very positions which are fraught with the greatest after-dangers to the patient. It will be into the pelvis, and to the back part of the body, that blood and effused fluids will gravitate; and from this part should they undergo decomposition, they will find the readiest and most direct entrance into the circulation, to the almost certain death of the patient.

I have said that a solid tumour, movable and surrounded by ascitic fluid, has, in my hands, proved the most favourable of all forms for operation; and that leads me to an observation which, I believe, all practical ovariotomists will endorse, though few attempts have been made directly to explain the reason why, after such operations, the patient generally progresses towards recovery in so remarkably *quiet a manner*. The question whether it is better to perform the *operation whilst the patient* is in robust health, or to wait until she is somewhat *enfeebled by the disease*, has been ably discussed both in this country and

America; and, whilst all the earlier operators recommended that it should be done under the former condition, nearly all are now agreed that the greatest success has attended such as have been undertaken under the latter. Many reasons have been given for this, but I have nowhere seen it distinctly stated that the difference in the result is mainly dependent, as I believe it to be, upon an altered condition of the peritoneum.

Peaslee comes nearest to this point when he says, "At all events a long distended state of the peritoneum very much diminishes its sensibility and the danger of traumatic peritonitis" (Ovarian tumours); but neither he, nor any of the authorities whom I have consulted, lay any stress upon the altered structural conditions of the membrane itself. If you consider for a few moments what is the normal condition of the peritoneum, both in structure and as to function, you will have no difficulty, I think, in understanding the ground upon which I assert that I would rather operate in a case where I know that it has undergone structural change than in one in which it is presumably perfectly healthy.

Physiologically the largest lymph-sac in the body, it is, as you are aware, perpetually engaged in the process of absorption; and if, in its unaltered condition, septic fluids be presented to it, their immediate absorption is, of course, the result; and structurally the largest serous membrane in the body, it is, of all others, most liable to become inflamed under conditions of provocation. Thus, in the performance of ovariotomy with a healthy peritoneum, we have the greatest liability to the two conditions which, of all others, are fraught with the greatest danger to the patient, viz., peritonitis, which alone destroys one-fourth of the cases which die, and septicæmia, which carries off at least as many. If, however, the membrane have undergone such changes, as the result of disease, as either weaken its power of absorption, or so change its structure that it is, in reality, no longer an active serous membrane, by so much, according to my experience, is the chance of the success of an operation improved; and this is a fact which, I think, my colleagues will bear me out in saying was very early recognised in this institution, and has oftentimes served usefully to assist us in forming our prognosis as to probable results in individual cases. I always hail the presence of ascitic fluid in the peritoneum as of highly favourable augury; and even adhesions of lymph, so only that they be soft and easily broken down, generally impress me favourably rather than otherwise. I can in no way, other than on this supposition, account for the perfectly calm recovery of many cases I have seen—which, to a stranger, would have appeared absolutely hopeless—where adhesion after adhesion has had to be broken down or cut through, and in which, notwithstanding, the patient has progressed afterwards with most singular felicity.

In the case of fluid or cystic tumours, considerations of another order regulate my judgment, and, in a measure, my method of procedure also. Adopting the commonly accepted classification of these cysts into uni- and multi-locular, I have learned to deal very differently with them, as they belong to one or other of these orders. In the case of an uni-locular cyst, I have long held with Mr. Southam of Manchester, that it is advisable, in all cases, to give the patient the benefit of tapping once, at all events, and that for more reasons than one. 1. It occasionally happens in some rare cases, that such a cyst, once fully emptied, may never refill, and the patient may, by this simple proceeding, be permanently cured. Two such cases have happened in my own practice, and I think my friend and colleague Mr. S. Hey will be able to support me in this doctrine by the result of a third case which I saw in consultation with him. A young lady tapped two years ago, has, up to this time, shown no liability to return of the disease, and has since married. 2. When the cyst *does* refill, it often does so very slowly, and, by a repetition of the tapping at long intervals, the patient may not only be kept in tolerable comfort, but the necessity for the major operation, with all its attendant risks, may be altogether staved off. Mr. Southam mentions several such cases, and one in particular, in which the patient only required the operation twice in nineteen years (Ovarian Tumours, 1869). 3. It should be done as an aid to diagnosis. It not unfrequently happens that a case may be so involved in doubt that an inspection of the contained fluid may prove of material assistance to us in this direction.

As a rule, it is not difficult to distinguish between the fluids of ascites and of ovarian dropsy, and thus a single tapping will often serve to clear up any doubt that may exist on this point. I say *often*, because it will not always do this. Cases have come under my own observation, and others are recorded, in which it has been exceedingly difficult, even with the fluid extracted, to say which of the two it is.

To tap a manifestly multilocular cyst is, in my judgment, to run a very serious risk, and one which is not generally counterbalanced by the amount of relief likely to be obtained by the operation; and hence it becomes a matter of considerable moment to be able to form a probable diagnosis as between the two forms. To a practised eye and a practised hand, this is, I believe, generally possible, but not in all cases. Hence it sometimes happens that the simple operation of tapping leads to very serious consequences, and occasionally even to death, and the next point to which I wish to advert is the lesson I have recently learned on this point.

If, after tapping a given cyst, it should turn out that we have not been able to empty it completely, what are the dangers which may arise from the procedure? The cyst may inflame; it may suppurate; or it may gangrene; and, under any of these circumstances, the life of the patient may be brought into imminent danger. In the very last case in which I operated in this institution, my patient positively refused to listen to any proposal to tap her, because, as she said, her sister, who had been, like herself, the subject of ovarian disease, had been tapped and had died. She was quite willing to submit to the major operation, and take all the risks attendant on it, but the minor one she steadily declined.

Now, bearing these risks in mind, I think it should be an invariable rule to take the patient fully into our confidence, to explain to her all the danger that may arise even from tapping only, and to see that she is prepared, should any of the accidents I have named unfortunately supervene, at once to submit to any further procedure that may be deemed advisable for her safety. That we have it in our power, even in that extremity, to make a manful fight in her defence, has been very recently proved by my friend Mr. Teale, who will be able, I believe, this evening to bring forward a case which, I think, he may proudly detail to you as one of the most brilliant triumphs of modern surgery.

A patient, the subject of ovarian disease, was tapped; the half emptied cyst failed fully to collapse; rapid decomposition of the remaining contents ensued, and the case was clearly about to end fatally in a very few hours. At a consultation, it was determined, notwithstanding the apparently desperate condition of the woman, that her only chance lay in the immediate performance of the major operation, and that was forthwith done. By Mr. Teale's permission, I am enabled to show you the cyst. In the posterior part of it is a large patch of gangrene, which, in a very short time, must have separated or have given way, and must have caused immediate death; and I am sure that you will join with me in congratulating my friend that, notwithstanding this desperate condition of affairs, he has succeeded in carrying this patient through her difficulties, and that she is now quite recovered.

With regard to the operation itself, I desire explicitly to record my feeling in favour of a full and free primary incision. For a long time the comparative merits of the large and the small incision were vigorously debated, and each could claim many supporters. On the one hand, it was argued that, if, after tapping and emptying a cyst, it could be withdrawn through a small opening, it was so much gain to the patient; but, on the other hand, it was soon ascertained by those who performed the operation most frequently that, in reality, the risk of peritonitis was quite as great after the small as after the large opening; and, moreover, it was so frequently found necessary to enlarge an opening, originally small, to meet some unexpected complication, that, slowly but steadily, the larger opening has been gaining ground, until I think it may be said to be very generally adopted. At all events, I would speak for myself, and I have no hesitation in saying, that I undoubtedly prefer a large opening.

I invariably feel, in commencing an ovarian operation, however well I may have thought the case out beforehand, that I may have to deal with difficulties and complications which could not be foreseen; and, in dealing with such, I greatly prefer to be able to see clearly all I have to do, and I feel certain the advantage is great to the patient, as well as to the surgeon. When, for instance, he has some unlooked-for adhesion to separate, or some bleeding vessel to secure, or some unexpected difficulty to encounter, how much more safely he can do it all when he can see what he is about, than if he be groping about in the dark; and I do not believe that, when once the peritoneum is opened, it makes the slightest ultimate difference whether the opening be three inches long, or six.

Moreover, I think, much less harm is done to the peritoneum itself whenever a tumour is lifted without strain through a free opening, than when one is liberated only after force has had to be used to drag it through a small incision. I do not think this position could be better illustrated than it was in another case in which I recently assisted Mr. Teale, and where through the whole operation, up to a certain point, all went as smoothly as we could any of us desire. A monocyst, easily emptied, turned out almost of its own accord, and seemed just on the point of escaping altogether from the body, when at the very furthest point behind, it was held back by a firm adhesion. To liberate it, this had to be divided; in doing so, a large feeding vessel was severed, and, unless we had had a tolerably free opening, I think Mr. Teale will agree with me in thinking that, to secure that vessel would have been almost, if not quite impossible. I do not consider it necessary, in all cases, to make an incision from ensiform cartilage to pubes; but judging by the size of the tumour or cyst I have to remove, I always so calculate its length as to give myself free space for all reasonable manipulation.

As to the best method of dealing with adhesions; if they be recent and soft, they are easily broken down; and, following them carefully with the finger and the eye, to detect any vessel that may seem inclined to bleed, I generally so deal with such; but, if the tumour be firmly adherent to the abdominal walls, or to any of the viscera, this cannot be done. If they be old and well organised, and pretty certain to live, I much prefer to cut out the portions of the cyst, and leave them attached than to attempt to dissect them away. In doing this, it is impossible to avoid a great deal of diffused bleeding at the time, and the oozing is very liable to continue after the completion of the operation. There is also the further risk, in attempting to loosen such adhesions, of injuring the organ to which they are attached; it is easy thus to tear into an intestine. I have seen the bladder lacerated, and I have heard of other accidents which have all proved fatal. On the other hand, I have adopted the method of leaving portions of the cyst attached, and I have never, in any instance, seen any evil arise from doing so. Upon the best method of dealing with the pedicle, more than anything else, hangs the success or failure of most cases. We have dealt with the pedicle in this institution, I believe, in every known way. We have used the clamp in all its forms; the ligature, both cut short and left hanging out of the wound. We have retained the stump in the lower part of the incision, and we have let it go free. We have tried the ligation of the whole pedicle, and of individual vessels, the searing iron, and torsion. Every method has succeeded in some, and failed in other cases; and so, feeling thankful that I have so many strings to my bow, I try to deal with each case according to its special requirements. I am careful, above all things, if it be possible, to avoid having any drag on the stump; and when, having severed a tumour, and arrested all hemorrhage in the stump, I feel that I can safely drop it into the pelvis, and see it no more, I am the most easy about it.

The two methods from which I have decidedly seen the happiest results have been "searing" and "torsion." The former is effected by seizing the pedicle near the uterus with a clamp, and then cutting off the tumour with the searing iron, and carefully burning away every atom of the eschar; the latter by simple twisting. The uterine portion of the pedicle, having been grasped by a clamp made specially for the purpose, is firmly held, and the end from which the tumour has been detached is enclosed in a second clamp, and the portion is then slowly and deliberately twisted off.

In using either of these methods, I am still careful, when the clamps have been removed, to search carefully over the end of the stump, and if I see any bleeding point to tie it; and having done this, and assured myself that all is safe, I delight to feel that, henceforward, I have done with the pedicle.—*British Medical Journal, March* 21, 1874, *p.* 373.

118.—A PRACTICAL POINT IN THE OPERATION OF OVARIOTOMY.

By Dr. Atlee, Philadelphia.

Dr. Atlee calls attention to the following very important practical point in the operation of ovariotomy. It is this: *immediately after making the incision through the walls of the abdomen, the index finger should be passed up to the region of the umbilicus, and if it can be swept freely across from side to side it must be within the abdomen.* This, of course, is an easy matter when no adhesions exist. It is always possible, in parietal adhesions, when the finger is inside of the peritoneum. It is not possible, without the most unwarrantable violence, when the finger is between the layers of the abdominal parietes. The non-observance of this rule has led to the separation of large portions of the peritoneal layer of the walls of the abdomen, even when no adhesions existed, the operator having mistaken the peritoneum itself for an adherent cyst-wall. When, however, parietal adhesions do exist, the mistake may be more excusable and more readily made, particularly in such a case as the one just related, where the peritoneum is thickened and more strongly incorporated with the cyst-wall than with the wall of the abdomen. The most convenient and infallible test of being within the abdomen is *the ability to freely move the finger to and fro past the umbilicus.—Philadelphia Medical Times.—Edinburgh Medical Journal, Nov.* 1873, *p.* 460.

119.—CATELECTROTONUS OF THE OVARIES IN THE TREATMENT OF AMENORRHŒA.

By Dr. Julius Althaus, Physician to the Hospital for Diseases of the Nervous System.

Amenorrhœa is a condition which may—according to the causes by which it is produced, and the circumstances in which the patient is placed—have a very slight or a very considerable importance. We know that it is in many cases simply a symptom of insufficient sanguification, as when it occurs in chlorosis, phthisis, or after severe hemorrhages or long-continued discharges, which have exhausted the system. Under such circumstances, common sense, and also the highest gynæcological authorities, teach us that any local stimulation of the sexual organs, in order to produce a menstrual flow, would be improper; and that the general condition, of which amenorrhœa is merely an expression, must be rectified in the first instance. Very frequently the ovarian function is re-established simultaneously with the restoration of the general health. In other instances, however, the catamenia are suddenly stopped in consequence of a chill, fright, anxiety, and depressing emotions; and the function may then remain in abeyance long after the cause by which it had been arrested has ceased. We can then no longer be sanguine that we shall be able to restore the menstrual flow by acting on the system generally; and local stimulation of the sexual organs is generally found requisite.

If amenorrhœa occurs in spinsters who are no longer likely to marry, or in married women who have already borne a number of children, the condition is not one of very great importance; but where we meet with it in married and childless women the aspect of the case is considerably altered. As it may be assumed that when the catamenia are arrested, ovulation has likewise ceased, the woman with amenorrhœa is, as it were, prematurely unsexed; and the re-establishment of the menstrual flow then becomes a vital object for the would-be mother. In cases of this kind, where the general health is as a rule excellent, the usual medicinal and surgical treatment of amenorrhœa often fails to rouse the dormant function of the sexual organs; and the various forms of electricity then step in as the right thing in the right place. The most effective mode of applying electricity I believe to be the induction of catelectrotonus of the ovaries; and the following may be considered a typical case:—

An American lady, aged 37, had her first confinement in March, 1871. The child was full-grown, but still born. The fœtal movements had suddenly ceased about a week previous to delivery, without any accident having occurred to the mother. The body of the child was found to be decomposed when born, and forceps had to be used. The mother was very ill for a considerable time afterwards, and she never saw any further trace of the menstrual discharge, although previous to her pregnancy she had always been perfectly regular. She underwent much treatment, and ultimately consulted Dr. Fordyce Barker, of New York, who pronounced the case to be one of "atrophy of the ovaries," and said that nothing could be done to restore the function of those organs. In May last the patient came to England, and being very anxious to have children, she consulted Mr. Spencer Wells, who discovered a slight degree of retroflexion of the uterus, which he replaced by the sound, and kept up by a pessary. He believed the amenorrhœa to be owing to a torpid condition of the ovaries, and sent the patient to me with the request to use electricity for rousing their function.

The general health of the patient was at that time quite satisfactory, and therefore no medicine was given. There had never been any vicarious menstruation or molimina. I induced catelectrotonus of the ovaries by placing the negative electrode of the constant battery alternately to the right and left ovarian region, putting the anode alternately to the lumbar spine and to the os uteri by means of an insulated sound. A current of from fifty to sixty cells of Daniell's battery was used, and the action of it kept up for fifteen minutes at a time. After a few such applications, the patient said that she felt the same sensations which she had always felt just before the period used to come on—viz., persistent uterine pain and frontal headache, chiefly in the right side of the head. A slight mucous discharge from the womb began at the same time. The galvanic current was now used on twenty-two different occasions, and the general sensations of the patient at the end of that period were such as to fully convince her that the catamenia were impending.

This was towards the end of June last. External reasons now obliged her to leave London, and I did not see her again until November 7, when she informed me that she had been disappointed in her expectation. The sensations which she thought premonitory of the occurrence of menstruation had disappeared within the first few days after discontinuing the galvanic treatment; the discharge from the womb had likewise ceased, and no sign of the period manifested itself. She was anxious to give the treatment a further trial, and it was therefore now resumed. Within the first few days the symptoms, which she had felt in summer, returned—viz., uterine pain and frontal headache. Shortly afterwards a thick mucous discharge from the womb was established, which was on one occasion sanguinolent, and so acrid that it made the inside of the thighs sore; for the relief of which cold cream was not sufficient, but free applications of the benzoated ointment of oxide of zinc were found requisite. The current was now used thirty times. The sensations previously described ceased, while the discharge persisted.

The treatment was now interrupted for ten days, and resumed on December 22. After three more applications, the period came on Christmas-day, to the intense gratification of the patient. The flow was, for the first twelve hours, quite as abundant as it had ever been before. It then diminished, and ceased altogether four hours afterwards; but was succeeded by a dark mucous discharge which lasted two days longer. As it appeared to me that this first response of nature had been very imperfect, I advised another short course of treatment just previous to the time when the next period would be expected. The patient accordingly came to me again on January 13, and had eight more applications of the current. Menstruation returned on January 22; it was of a particularly good character, and went on abundantly for four days. As, therefore, not only a satisfactory quality, but also decided periodicity of the ovarian function appeared now to have become established, I discharged the patient from further attendance, and she left England for Italy shortly afterwards.

The result of the treatment in this case was all the more satisfactory as the amenorrhœa had already lasted nearly three years, the patient was no longer very young, and there seemed to be no prospect of other means proving effectual. Indeed, so able and experienced an observer as Dr. Fordyce Barker had pronounced the case to be incurable. Its ultimately successful issue, however, shows that the induction of catelectrotonus of the ovaries is a powerful remedy, by means of which, if perseveringly employed, the function of the ovaries may be restored even after having been in abeyance for years.—*Medical Times and Gazette, March* 14, 1874, *p.* 287.

120.—ON UTERINE THERAPEUTICS.

By Dr. LOMBE ATTHILL, Fellow and Examiner in Midwifery, King and Queen's College of Physicians, Obstetric Physician to the Adelaide Hospital, Dublin.

[In the following paper the writer speaks only of that important class of remedies which are applied directly to the vagina and uterus.]

Syringing the vagina with water or with medicated fluids is an old and popular remedy for nearly every form of uterine disease, a remedy which, though often of great value if properly performed, and practised in suitable cases, is as often utterly useless, and occasionally positively injurious. Thus, an elastic enema-bag, capable of holding from six to ten ounces, is commonly employed for the purpose; such an instrument is quite useless. But occasionally a worse, because a positively dangerous instrument is employed, namely, a glass syringe, the end of which is perforated with five or six holes. Not long since I was requested to see a woman to whom such a syringe had been supplied. The glass being thin, the instrument broke in the vagina, and several pieces of broken glass remained in that canal, causing intense pain to the patient. By slowly and carefully introducing a Ferguson's speculum I was enabled to extract one by one through it the fragments of the syringe, and no serious consequences followed.

The syringe to be employed for the purpose of vaginal injections should be one capable of throwing up a continuous stream. Such syringes are commonly know as "the syphon syringe," or "Higginson's syringe."

When using the syringe the patient should, if possible, lie on her back, the hip resting on a bed-pan, which receives the fluid as it escapes from the vagina, but the majority of patients object to this plan, which necessitates the presence of an assistant, and then you are obliged to permit her to inject the fluid from a vessel placed in front of her, or in a foot-pan, or bath over which she sits. This is a very much less efficient method, for the fluid escapes from the vagina too rapidly, and does not distend that canal, as it is desirable it should. In cases where there is not any urgent reason for the use of medicated lotions, it is often a good plan to direct the patient to use her syringe while sitting in a warm hip-bath. I have found this method most efficacious in allaying vaginal irritation.

Having thus pointed out to you the method of syringing the vagina, it is further important for you to consider of what temperature the fluid to be injected should be, the quantity to be used at each time, the medical agents to be so employed, and their strength.

As a rule, I recommend you not to inject any fluid into the vagina which is perfectly cold; doubtless a perfectly cold fluid is more tonic, if I may use that expression, than a warm one could be, but the object of injections is, as a rule, to allay irritation, and not to give tone to the vaginal walls; that will soon follow as a result if you remove the local affection. Besides, I have seen very unpleasant and even serious consequences follow the injection of cold water into the vagina. Thus, severe uterine colic, and intense pain above the pubes occurred as an immediate result in one case, and in another so grave were the symptoms that life was endangered from an attack of pelvic cellulitis which followed the injection into the vagina of cold water, ordered with the view of checking profuse menstruation. I recommend you, then, to direct that the fluid to be employed be used at about blood heat, and when vaginitis is present, at even a somewhat higher temperature. The quantity to be employed should be considerable. If tepid water be used no better plan can be adopted than to make the patient use the syringe while sitting in a warm bath. If this cannot for any reason be done, she may sit over a foot-pan. Having a large supply of the water to be injected in a vessel placed in front of her, as already mentioned the recumbent posture is the best, for if it be adopted we may feel sure that the fluid will reach the highest part of the vagina, which I am satisfied is seldom the case when a sitting posture is adopted; but this method is too troublesome to be carried out easily in private practice, and is objected to by most females. Nor am I sure that the results are commensurate with the trouble.

The medical agents employed for vaginal injections are very numerous. I, however, restrict myself to a few. I have found the solutions of alum and of the sulphate of zinc so frequently to aggravate the patient's sufferings when vaginitis was present, that I now in such cases seldom or never employ either. They coagulate the albumen which enters so largely into the composition of leucorrhœal discharges. If you examine a patient any time within twenty-four hours after she has used an alum injection you will find in the vagina a number of hard masses formed by the coagulation of the discharge, and these often cause much discomfort. Borax is a better agent, but it sometimes causes irritation, though in a less degree. A drachm of borax to the pint of water is the strength I usually direct to be used.

Where the object is to soothe and to allay irritation this is best effected by the use of an infusion of tobacco. Tobacco must, however, be used with caution. Some patients are peculiarly susceptible of its action, especially those in whom the orifice of the vagina being narrow some of the fluid is retained in the canal. Begin, therefore, by infusing fifteen grains of the unmanufactured leaf in a pint of boiling water. If this produce no unpleasant effect, increase the strength to thirty—even sixty, grains to the pint. In many cases the addition of a drachm of borax to each pint of the infusion greatly increases the efficacy of the treatment. Many patients, however, are unable to use the tobacco at all, as even a very weak infusion causes nausea and faintness. I therefore recommend, when this is the case, or where you fear to run the risk of causing any discomfort, to substitute for tobacco an infusion of hops, directing an ounce of hops to be infused in a pint of boiling water with or without the addition of borax, as you may deem advisable.

Cases are, however, frequently met with where no vaginal inflammation or even irritation exists, but where profuse and weakening leucorrhœal discharge is constantly being poured out, which it is necessary to check; here astringents, such as alum or zinc, in the proportion of sixty grains to the pint of tepid water, prove often most useful. Should they irritate you will frequently find the decoction of oak bark serviceable. Warn your patient, however, that the infusion of oak bark stains linen, for ladies will not be pleased to find their under-clothing or towels covered with ugly stains. This reminds me to give you a caution respecting the use of the solution of nitrate of silver. A few years ago this was almost the only remedy employed in the treatment of uterine disease. I can with confidence say that as an application in cases of disease of the body of the uterus or of the cervix, it is perfectly useless. In cases of vaginitis it may be employed with advantage. It must be applied through a speculum, the surface of the vagina being brushed over with a solution containing twenty or thirty grains of the salt to an ounce of water. The application may be repeated at intervals of two or three days. I now seldom employ the solution of nitrate of silver, as I look on its use in the majority of cases as a mistake, and, moreover, I believe I can obtain better results by other means.

Of all the agents which are applied to the vagina for the relief of inflammation or congestion of the canal, glycerine, without doubt, is the most valuable. A small roll of cotton-wool will absorb five or six drachms of glycerine; you fasten to this a strong thread or piece of twine, introduce it through a speculum, and leave it in the vagina for twelve or even twenty-four hours, directing your patient to withdraw it at the expiration of that time by means of the string which is left hanging outside the vulva. Glycerine thus applied produces a copious watery discharge, which has a marked effect on the mucous surfaces in immediate contact with it. Thus, after its application the vagina and vaginal aspect of the cervix uteri appear pale, and the copious discharge seldom fails to relieve, for the time at least, that distressing sense of heat which is complained of in severe cases of vaginitis. In less acute cases the addition of ten grains of tannic acid to the ounce of glycerine often proves useful, but if used before the acute symptoms subside, it may cause increased irritation. Be sure whenever you use glycerine to warn your patient that she is to expect a copious discharge, otherwise the great flow which often comes on almost immediately will cause much alarm.

Such applications are, however, quite insufficient when an extensive granular condition of the lips of the os uteri exists; such a condition is always coexistent with a similar state of the canal of the cervix, and frequently of the interior of the uterus also. In these cases it is necessary to carry your treatment into the cavity of the body of the uterus, even up to the very fundus, for it is seldom possible to cure disease of the interior of the uterus unless applications be made directly to the diseased surface. This is the department of uterine surgery in which the greatest advance has been made of recent years. Cases are daily being treated successfully now which were looked upon as incurable a short time ago. Much has been done in the treatment of these obscure affections, but much still remains to be done; however, I am very hopeful of our future, and believe that the incurable ones will ere long form but a small percentage of the cases of uterine disease.

Numerous medical agents are now employed in the treatment of disease of the body of the uterus. These may be used in the form of fluids, of solids, or of ointments. I mention them in what I consider to be the order of their value.

With respect to fluids I give you one caution: do not inject them into the uterus; such a method is fraught with great danger, and except that it is generally easy of execution, possesses no advantage.

The fluids most commonly employed in the treatment of intra-uterine diseases are a saturated solution of carbolic acid, the tincture of the perchloride of iron, tincture of iodine, the pernitrate of mercury, chromic acid, and the fuming nitric acid. A solution of nitrate of silver is also sometimes used, but I believe it to be altogether inefficacious.

Carbolic acid is a mild, but by no means a painless application; applied to the vaginal surface of the cervix it produces a very superficial slough, its effects passing off in twenty-four hours. Applied to the interior of the uterus its effects are equally superficial and transitory. It is therefore useful in cases where you desire to apply a mild stimulating caustic, but it is not suitable when it is necessary to destroy the so-called granulations which in severe cases cover the vaginal surface of the cervix and extend into its canal; nor where an unhealthy condition of the mucous membrane lining the body of the uterus, the result of chronic endometritis, exists, a condition which often gives rise to profuse menorrhagia. It is best applied by means of a flexible silver or copper probe round the end of which is wrapped a layer of cotton; this can be passed into the uterus to the desired depth. When carried beyond the os internum the carbolic acid sometimes causes pain, which, however, soon subsides.

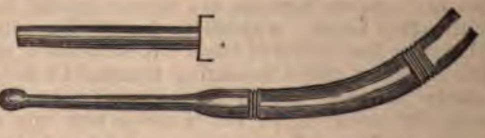
The perchloride of iron is an admirable styptic, and, as such, should be used when it is desirable to check uterine hemorrhage. You can apply it in the same manner as the carbolic acid; but it is generally better to saturate a small roll of cotton with the tincture (or, as being less irritating, with a saturated solution of the perchloride in glycerine). Pass this up through a speculum, and place it in contact with the os uteri, and then, outside this, another and larger pledget of cotton well soaked with glycerine. Both these should be removed within twelve hours of their application. I have seen a very deep slough produced in the vagina in a case where the cotton, saturated with the perchloride, was accidentally left in the vagina for two days. When it is desirable to check hemorrhage depending on a granular condition of the cervix, the perchloride of iron is a very valuable agent. Iodine has been used for the same purpose; it will sometimes answer, but it is less certain in its effects. The pernitrate of mercury is a powerful and active caustic. It has been recommended by some practitioners as an application in cases of malignant disease. I never employ it, because I believe I have at my command a caustic equally if not more efficacious, and one, at the same time, much safer, for very severe salivation has followed the use of the pernitrate in persons susceptible of the peculiar action of mercury.

Chromic and nitric acid are nearly identical in their action. The former is, however, in my opinion, more uncertain in its effects; it is also more irritating. I therefore prefer the nitric acid. Its application causes very little, indeed, in general, no pain; it produces but a superficial slough, and has often a wonderful effect in bringing about a healthy condition of the mucous membrane lining the body and cervix uteri. It also, in many instances, exerts a directly sedative influence, allaying the severe pain and vesical irritation so constantly present in cases of endometritis.

No matter which of these fluid caustics you may select, certain rules applicable to all should be borne in mind. In the first instance, local inflammation, indicated by tenderness of the uterus when touched, should, if present, be removed, or at least mitigated, by appropriate treatment before any of them be used. To effect this the cervix, if soft and engorged, should be punctured, or if enlarged and indurated, leeched.

When it is desirable, as is frequently the case,-to carry the application up to the fundus, this should, if possible, be done through a canula, or tube, with the double object of preventing the agent selected from being weakened by admixture with the secretions during its passage through the cervical canal and by contact with its walls, and also of protecting the healthy structures from the action of the caustic which may be employed, for it must be borne in mind that the mucous membrane lining the cavity of the uterus may be, and often is, diseased, that lining the cervical canal being in a perfectly healthy condition. It is therefore all-important that the healthy structures should be protected from the action of the caustic.

With the view of effecting this object, I have devised an instrument of very simple construction. It consists of a short tube or canula, made of platinum, and of a curved stilette, which fits the canula accurately, and which is fixed to a boxwood handle. The instrument being introduced into the uterus in the same manner as the uterine sound, the stilette is to be withdrawn, and the canula, being held steady, by means of a pair of long forceps, a copper, or better still, a platinum rod, dipped in the agent selected, is to be passed through the canula up to the fundus. You have all repeatedly seen me treat cases of endometritis in this manner, and with the most satisfactory results. I may refer you to the case of a patient at present in the house as an example. She is a widow, aged 45, was admitted on the 15th of last month, suffering from profuse menorrhagia. A careful examination of her case convinced me that the hemorrhage depended on a granular condition of the mucous membrane lining the cavity of the uterus. The vagina was plugged to arrest the flow, which had continued incessantly for fifteen days, the interval between the commencement of this and the termination of the preceding period having been but ten days. On the subsidence of the flow the canula was introduced with the greatest ease. This was done in the manner I usually adopt, the os being ex-

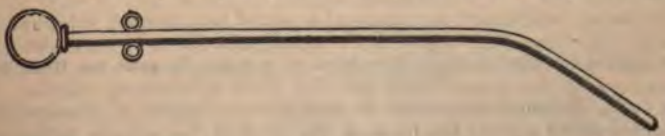


posed by means of a full-sized Ferguson's speculum; the stilette, armed with the canula, was with ease passed through the cervical canal into the uterus. There is, in general, little difficulty in effecting this, for in suitable cases the cervical canal is patulous, or at least relaxed. In the case under consideration this was so. Having introduced the instrument and withdrawn the stilette, I passed through it this platinum rod, round the end of which was firmly wrapped a roll of cotton saturated in the fuming nitric acid; no pain followed this application, which was repeated after the lapse of a week. The result was most satisfactory. The menstrual flow did not reappear for five weeks, and then lasted for but five days, the quantity lost also being moderate. Bear in mind that for months past this woman had never been free from profuse and debilitating hemorrhage except for a few days at a time. But do not suppose that because I have succeeded, by two intra-uterine applications of the fuming nitric acid, in stopping the excessive loss, that this woman is cured; her condition is improved, but she is not cured. A relapse will certainly occur if the same application be not repeated, and that too, several times.

Now one word as to the details of this operation—if that be not too dignified a name for the proceeding, for you will fail in your attempt to carry out this method of cauterising the interior of the uterus successfully unless you attend to various little points. The first is that you take care to grasp firmly the little projecting ear of the canula with a pair of long forceps before you withdraw the stilette. If you do not do so, one of two things will happen: either the canula will slip out of the cervix, or, if the os be patulous, as is frequently the case, it will disappear *in toto* within the cervical canal. Doubtless it will soon reappear; but it is not then always easy to grasp it, and it will sometimes slip behind the speculum, or, if grasped, may be found full of mucus. By holding the canula firmly with the forceps these troubles will be avoided. Next, and even more important, is the fixing of the cotton firmly on the end of the probe. Draw out the cotton, moisten the tip of the rod, catch but a few fibres of the cotton at first, and roll the rest slowly and evenly on. This is better effected by rotating the rod than by rolling the cotton round and round it. If these directions be not attended to the cotton will wrinkle up as it passes through the canula, and will render the passage of the rod impossible, or, if loosely put in, may be left behind in the uterus when the rod is withdrawn. Neither of these accidents will ever occur if the directions I have given be followed.

These directions apply equally to all liquid caustics used for the purpose of intra-uterine medication, and the success of your treatment will depend very much on the dexterity with which you carry it out. If there be too much cotton rolled round the probe, or if it be loosely rolled the on, rod will stick in the canula, and you will have to withdraw it and reintroduce it again, or if you take up too much of the caustic on the cotton it will trickle down, and may cause a troublesome sore to form on the vagina; so that to carry out this method, simple though it be, skill is needed and must be acquired.

Of the solid caustics, the nitrate of silver and sulphate of zinc are the only ones I use. You can insert these through the canula I have described; but it is better effected through this *porte caustique*. It is known as Sir James Simpson's. By using it you can dispense with the speculum. Ten grains of the nitrate of silver or of the sulphate of zinc, the latter in the form of "zinc points," as suggested by Dr. Braxton Hicks, are to be introduced through it up to the fundus, and left there to dissolve. Either of these caustics so used is liable to cause pain, seldom, however, severe in character; this too can be, in some degree at least, averted by placing a pledget of cotton saturated with glycerine in the vagina. I use both these agents occasionally, but less frequently than formerly, for since I have devised the means of ap-



plying the nitric acid, without previous dilatation, to the interior of the uterus, as is effected by means of the canula, the results have been so satisfactory, that I now seldom resort to the use of the solid caustics. Of the use of ointments I have no personal experience; they are more difficult to apply than either the fluid or solid caustics named. Dr. Barnes, however, considers them to be often of great value in some cases; doubtless they sometimes are so.

It is occasionally advisable to destroy the tissues of the cervix to a greater depth than can be effected by means of the nitric acid. For this purpose two agents are employed—namely, caustic potash, or potassæ c. calce, and the actual cautery; the former is eminently useful in those cases where the lips of the os uteri are in a state of granular erosion. You have seen me use it with the very best results. As I have in a previous lecture explained to you the mode of applying it, I shall not dwell on it now, further than to remind you that it must be used cautiously, and that the vagina must be protected from the action of the caustic by the insertion of a pledget of lint, saturated in vinegar, under the lower edge of the cervix. The actual cautery is not much employed in this country, but in America its use is warmly advocated. Dr. Gaillard Thomas states that, according to his experience, "of all the means of counter-irritation for removing chronic parenchymatous congestion, and causing a diminution in the size of the uterus by stimulating absorption, this is the most efficient and least objectionable as to its consequences." He uses a small steel rod, terminating in a disk not much larger than a split pea. This, heated in a spirit lamp, he applies for ten or twenty seconds to the cervix, so as to create a small slough, reheating and reapplying the cautery so as to cauterise the cervix in two or three places, one at either side of the os uteri.

Dr. Getchell, of Philadelphia, also advocates the use of the actual cautery in cases in which the cervix uteri is hypertrophied and indurated, but instead of a steel rod he employs charcoal sticks made of nitrate of potash, twenty grains; charcoal, seven drachms; powdered acacia, one drachm; and water sufficient to make into a paste. This paste is to be formed into sticks of any required diameter and length. Dr. Getchell uses them of about the diameter of the little finger; the stick is to be held in the flame of a gas or spirit lamp for a few moments till converted into a live coal, and applied through a glass or wooden speculum. His directions are: "Take the caustic in the forceps, and apply it about four or five lines from the os to the lip which is most hypertrophied. Now, if you make slight pressure for a few seconds, you will destroy tissue over a space of about the size of a three-cent piece, and of about two lines in depth; the pain is very slight. On withdrawing the cautery, I sponge the part with cold water. I then introduce a pledget of lint saturated with glycerine, and keep the patient in bed for forty-eight hours." The actual cautery may be applied once a month. I have but recently commenced to try this method; I believe, however, that I shall derive good results from the practice. —*Medical Press and Circular, Dec.* 31, 1873, *p.* 583.

121.—ON PELVIC HÆMATOCELE.

By Dr. ALFRED MEADOWS, Physician-Accoucheur to St. Mary's Hospital, and Physician to the Hospital for Women and Children.

[The term "Pelvic Hæmatocele" is confined to effusions of blood within the peritoneal cavity; when the blood is effused into the cellular tissue beneath the peritoneum it is called "thrombus." The latter must be extremely rare, as Dr. Meadows states that he has never yet met with a case.]

I believe that all cases where the tumour is considerable are cases of true intra-peritoneal hæmatoceles: it is important to bear this in mind, because obviously it has a bearing upon the question of treatment, especially in its surgical aspect; at least I should have less hesitation in tapping a hæmatocele where I knew that the blood was effused and encysted in the peritoneal cavity than I should in puncturing a thrombus with the blood extravasated into the cellular tissue. The risk of mischief resulting is, I believe, far greater in the latter case than in the former. Such, at all events, is my experience in regard to tapping pelvic swellings.

Now, as to the trustworthy symptoms which will in general enable us to diagnose cases of true pelvic hæmatoceles, they are typically seen in the following case, which I have elsewhere recorded :—

Mrs. S., aged 30, married ten years, and has had two children, the youngest being three years of age, consulted me on November 14th, 1870, for a leucorrhœal discharge, which, she had been told, was due to an ulcer of the cervix uteri. On examination, I found that the uterus was large, measuring three inches in length ; the mucous lining of the cervix was everted, and deeply injected, being what is called by some authorities in a state of granular erosion. All the culs-de-sac were entirely free from any deposit whatever. On the day following she went out to dinner, and, although it was very cold and wet, she determined to walk in her light evening dress to the house she was going to visit, which was only a few doors off. Menstruation was about due at the time, but was frequently irregular, so that she could hardly be sure of the date. She had scarcely sat down to dinner when she was seized with severe pain in the hypogastric region, which compelled her to leave the room ; and she was carried home feeling very faint and ill. Some hot brandy-and-water was taken at once, and warm fomentations were applied. The pain, however, continued with but slight abatement, and the next day I was called in. On examining per vaginam, I found a swelling behind the uterus, stretching across from side to side, and pushing that viscus slightly forwards; it also bulged a little into the vagina. There was extreme pain and tenderness in the lump, and also on deep pressure into the pelvis from above. The swelling was somewhat elastic, but more boggy. On the following day the swelling had increased ; there was more pain ; pulse 120, tongue furred, expression anxious. Nothing could be felt externally. Next day there was still greater increase and bulging of the swelling per vaginam, which now invaded the left side of the pelvis, pushing the uterus against the right obturator foramen. The pain was not so severe, but differed in that it was of a forcing, bearing down character. The lump was more distinctly elastic, and I had no doubt that it contained fluid. It had pushed Douglas's pouch so low down as to be quite near the vaginal outlet, and might have been tapped with great facility, either per vaginam or rectum. On the fourth day there was slight aggravation of all the symptoms, with a relative increase in the size of the swelling, which, however, could only be felt externally on pressing into the pelvis. It did not rise above the level of the pelvic brim. There was no obstruction either of the bladder or rectum, though the bowels acted with difficulty and great pain, and only after aperient medicine. During the course of the next ten days the symptoms greatly abated, the pain diminished, and the swelling grew less and less, so that on December 14th, just a month after the commencement of the attack, it was noted that "there is hardly a trace of the lump left ; but a kind of ridge may be felt behind the uterus, forming, as it were, a cast of the peritoneal fold in that direction." Even that ultimately disappeared, and when I examined her early in February nothing abnormal was discoverable.

On the 15th of that month, it being a menstrual period, she again exposed herself to cold, and was seized in almost precisely the same manner. I saw her the day after, and on examination I found that the uterus was depressed, pushed somewhat forwards and to the left side by a boggy, putty-like mass, which occupied the posterior and part of the right lateral cul-de-sac. Defecation was difficult and extremely painful. There was very slight sanguineous discharge from the vagina, and violent bearing down pain, as if everything would force from her. The swelling at no time appeared to be larger than a good-sized orange. A few days subsequent to this and after a good deal of forcing pain, she expelled a small mass, which, on examination, proved to be nothing more than a decolourised fibrinous clot, entangling a good deal of uterine epithelium. After this she felt great relief ; the catamenial discharge came on much more freely ; *the post-uterine lump* began to diminish in size ; and she made a *rapid and complete recovery*, so that when I saw her a few months afterwards the uterus was quite movable; no deposit could be felt in either cul-de-sac, and nothing abnormal was discoverable.

Now it seldom falls to our lot to be able to demonstrate, as was done in this case, the rapid formation of a post-uterine swelling coincidently with the occurrence of symptoms of acute pelvic inflammation. It so happened that a few hours before the attack came on, I had the opportunity of examining this lady, and was satisfied that no pelvic swelling existed. A similar examination made a few hours after the attack set in, showed the existence of a soft, elastic tumour behind the uterus, filling up Douglas's pouch, and pushing the uterus forwards against the pubis. Now there is no swelling or tumour that I know of in connexion with the female generative organs which can be formed so rapidly as this except pelvic hæmatocele. This fact alone, therefore, was sufficient to warrant the diagnosis, and it was corroborated entirely by the accompanying symptoms. We cannot, however, reckon upon such opportunities as were afforded me in this case; and, in the absence of that knowledge, our diagnosis will be founded partly upon the history of the case and partly upon the character of the pelvic swelling.

As regards history, I find that in all the cases which have come under my care there has been a more or less distinct history of uterine or ovarian trouble, which has, as it were, led up to and culminated in the attack which now claims attention. This is more particularly observed in those cases where the hæmatocle is not directly and immediately connected with menstruation. In the majority of cases there is this connexion, and the hæmatocele results either from the regurgitation of the menstrual fluid from the uterus along the Fallopian tube into the peritoneal cavity, or else the afflux of blood which usually takes place to the generative organs at the time of menstruation leads to such an engorgement of the venous plexuses that rupture occurs at some point where either a varicose or other diseased condition existed, which predisposed to the catastrophe. In a certain number of cases—but they are, in my experience, very rare—the hemorrhage occurs in connexion with pregnancy, or rather with delivery, either at term or, more commonly, prematurely, and especially during the earlier months. Probably in these cases the diseased condition, whatever it be, which gives rise to the hæmatocele after the abortion is the principal agent in bringing about the premature expulsion of the ovum. And here also careful inquiry will serve to elucidate the fact insisted upon—viz., the existence of symptoms of uterine or ovarian disease prior to the occurrence of pelvic hæmatocele. Indeed, I do not think it possible for such an accident, if I may so call it, to occur in a woman perfectly healthy in those parts. Happily for the patient, though unfortunately for science, these cases seldom end fatally, and our opportunities, therefore, of examining the parts and of demonstrating the origin of the mischief are few and far between. I believe that a varicose condition of the veins of the pampiniform plexus, resembling varicocele in the male, is of very common occurrence, and may have much to do with the disease we are considering. All this, of course, has reference only to antecedent history and to predisposing causes.

The symptoms which characterise the attack itself are generally very well marked, and although in themselves it would, perhaps, be difficult in some cases to distinguish them absolutely from those which occur in certain acute pelvic inflammations, such as cellulitis or peritonitis, yet in general there are important differences which, taken in connexion with a digital vaginal examination of the resulting pelvic swelling, are sufficiently characteristic to make the diagnosis a matter of tolerable certainty.

Probably the only conditions with which pelvic hæmatocele is likely to be confounded are pelvic cellulitis and pelvic peritonitis; but in regard to each of these there are some few points of dissimilarity in the character of the symptoms, and there are still greater differences in the local physical signs, especially in regard to the formation of the pelvic swelling, its situation, consistence, and general characters.

In all three, when fully developed, there are the usual signs of inflammatory action—namely, local pain and general febrile disturbance. To some extent they resemble one another in the mode of attack, but though each may begin rather suddenly, the symptoms of cellulitis and peritonitis are seldom so severe at first as hæmatocele; the pain of cellulitis is not nearly so sharp as that of the other two, and hæmatocele is generally the most severe of all. Faintness, great prostration, and even collapse to an alarming extent, frequently accompany the accession of hæmatocele; neither of these occurs in connexion with cellulitis or peritonitis. On the whole, it may be said that, taking average cases of these three affections, pelvic hæmatocele is characterised by symptoms which are more sudden, severe, and alarming than either of the other two—it, in fact, shows its traumatic character very distinctly; and pain, prostration or collapse, with symptoms of internal hemorrhage, are its leading features. The pain is of course very local and limited, and it has a peculiar forcing, bearing down character, a feeling which is described sometimes as if everything were being forced away.

With the occurrence of such symptoms, a vaginal examination will in general suffice to make the diagnosis clear. If circumstances have enabled us, as in the case already detailed, to demonstrate the sudden formation of a pelvic swelling, situate chiefly in the post-uterine region, that may be taken as conclusive evidence of hæmatocele; for not only does the swelling in cellulitis form much more slowly, but it differs also in situation and character. It is at first neither in front of nor behind the uterus, but on one side, originating as it does in the cellular tissue of one or other broad ligament. In pelvic peritonitis the swelling forms even still more slowly, is generally behind the uterus—not on one side,—and more limited in extent than either of the other two.

Again, there are differences in the character of the swelling. In hæmatocele it is at first tense, elastic, fluid, and fluctuating; as time goes on, it becomes boggy, doughy, non-fluctuating; then firmer and firmer, till it is almost hard, and with this change it contracts considerably, getting smaller and smaller, till it finally disappears. Cellulitis, on the contrary, is small at first, and gradually increases in size, becoming softer and more elastic as it increases. It sometimes changes its brawny character for one in which fluctuation can be distinctly made out. In all these respects it resembles pelvic peritonitis, and so far both differ from pelvic hæmatocele; but they in their turn differ from one another in this respect, that in cellulitis the swelling is one-sided, while in peritonitis it is situate either anteriorly or posteriorly.

Lastly, in cellulitis the uterus is usually displaced laterally, being pushed to one side by the swelling formed in the broad ligament of the opposite side; while in hæmatocele and in pelvic peritonitis the uterus is displaced forwards or backwards, but most commonly forwards, and much more so in hæmatocele than in peritonitis.

Such are the characteristics of the pelvic swelling in these three conditions. Their differences are in general sufficiently marked to enable us to diagnose them with tolerable certainty, especially when taken in connexion with the history and symptoms past and present. Neither the history nor the physical signs taken separately are in themselves sufficient to establish the diagnosis. Both must be taken together; but a minute and careful examination is of the utmost importance, not only for the purpose of diagnosis, but still more in reference to treatment, and especially in regard to that mode of treatment which I believe to be the best and most successful. I have a strong impression that if the symptoms and physical signs of pelvic hæmatocele were more generally understood and appreciated, we should find that cases of this affection are more common than is generally supposed, and that it is not such a medical curiosity as some imagine. Cases of pelvic cellulitis, pelvic peritonitis, and pelvic hæmatocele have no doubt just sufficient in common to make an error of diagnosis pardonable; but I would venture to hope that mistakes of this kind may be less common in the future as the disease becomes better understood.—*Lancet, Nov.* 15, 1873, *p.* 698.

122.—NOTE ON SPONGE TENTS.

[By Lawson Tait, Esq., Birmingham.

The extremely offensive smell of a sponge tent removed from the uterus after having been there only a few hours must be familiar to all who have used one; and, considering the frequent use of sponge tents, it is singular that this abominable putridity does not more frequently produce serious mischief than it would seem to do. Although I have used a very large number of these tents, I have never seen them do any harm until recently, when a case of fatal septic peritonitis occurred in my hospital practice which I could trace only to the septic influence of a sponge tent.

Sponge tents are far safer than those made of sea-tangle, and it has long been one of my objects to remove the above-mentioned objection to their use. I have tried charging them with various disinfectants, but without any result, until last week I made an experiment with oil of cloves, and I have found that a tent charged with 5 per cent. solution of oil of cloves will remain in the uterus for twenty-four hours without becoming offensive in the slightest degree. There can be no doubt that such a tent is far safer than those ordinarily in use, and it is certainly much more agreeable to the operator. Messrs. Krohne and Sesemann, of Duke-street, W., make these tents according to my formula.—*Medical Times and Gazette, Jan.* 10, 1874, *p.* 35.

ADDENDA.

123.—ON THE ACTION AND USES OF CROTON-CHLORAL HYDRATE.

By Dr. Oscar Liebreich, Professor of Materia Medica in the University of Berlin.

[Croton-chloral is a new remedy, allied to chloral-hydrate in its action, but differing in some most important points from it.]

When chlorine gas acts on aldehyde, croton-chloral is formed, as has been demonstrated by Dr. Krämer and Dr. Tinner. In order to avoid a mistake which is apt to be caused by the name, I must here remark that this body possesses no relation whatever to croton-oil, although its chemical constitution proves it to be the chlorated aldehyde of crotonic acid. Croton-chloral differs in its outward appearance from hydrate of chloral, by its being dissolved with difficulty in water, and by its crystallising in small glittering tablets. Its action, though similar to that of hydrate of chloral, differs widely from the latter with regard to its physiological effects. Four *grammes*, or a drachm, of this substance, dissolved in water, and introduced into the stomach, produce in the course of from fifteen to twenty minutes a deep sleep, accompanied by anæsthesis of the head. Whilst the eyeball has lost its irritability, and the nervus trigeminus shows no reaction whatever on being irritated, the tone of the muscles remains unaltered.

I have experimented with this remedy on maniacs during an attack of mania. They remained quietly sitting on their chairs in a deep sleep, their pulse and respiration being unchanged, for two whole hours together. If anæsthesis had reached so high a degree in consequence of the application of hydrate of chloral, the patients would have dropped from their chairs, and both their pulse and respiration would have been considerably retarded. I have seen croton-chloral acting in the same way on healthy individuals. In some cases of tic douloureux, the remarkable phenomenon is exhibited that pain ceases before sleep sets in. I am sorry to say, however, that this remedy acts only as a palliative in this dreadful disease. I nevertheless prefer its action to that of morphia, because it has effects as good as the latter remedy, without being so detrimental to the constitution in general. I have never observed any unfavourable effects of croton-chloral on the stomach or any other organ, although I have made frequent experiments with it.

The indications for the use of this remedy are to be found—1. In cases where hydrate of chloral is inapplicable on account of heart-disease; 2. In cases of neuralgia in the district of the nervus trigeminus; 3. In cases where very large doses of chloral are necessary to produce sleep. I there recommend the addition of croton-chloral to hydrate of chloral.

Whilst examining the difference between the action of hydrate of chloral and that of croton-chloral, I have discovered the remarkable fact that it is not the first, but the second, product of decomposition of the latter substance which is brought into action, on account of the first being too rapidly destroyed. Croton-chloral, when subjected to the influence of an alkali, first forms allyl-chloroform, a trichlorated body, which is rapidly decomposed into a bichlorated substance called bichlorallylene. Now, both chloroform and trichlorated substances act, as I have shown, in their first stage on the brain, in the second on the spinal cord, and in the third on the heart. The retardation of respiration is to be explained by the agency of these substances on the last mentioned organ.

Bichlorated substances act differently, as is proved by bichloride of ethylene. Even if the circulation of the blood in an animal have been stopped by this latter agent for one minute, life may be restored by artificial respiration, which is impossible whenever trichlorated substances have produced this effect, in which case the muscles of the heart remain paralysed. Well, in animals poisoned by croton-chloral to such a degree that both circulation and respiration are stopped entirely, artificial respiration is able to restore the action of the heart immediately, and the life of the animal may thus be saved. Bichlorallylene, inhaled by the lungs, produces the same effect on animals as croton-chloral. We thus see these bichlorated substances acting on the brain, spinal cord, and medulla oblongata, but not on the heart, which explains the fact that both respiration and circulation remain unaltered in man by a medicinal dose. It is a highly interesting fact, however, that under favourable conditions, we still are able to produce in animals the effects of the first product of decomposition of croton-chloral—*i.e.*, of the trichlorated substance or of allyl-chloroform. In order to observe these effects, it is necessary to introduce immense doses of croton-chloral into the body, when paralysis of the heart actually does ensue.-*Brit. Med. Journal, Dec.* 20, 1873, *p.* 713.

124.—COCA: ITS THERAPEUTIC USE.

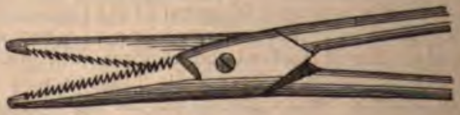
By Dr. E. H. Sieveking.

Many years ago my attention was drawn to the reputed restorative powers of "coca," by a pamphlet written by M. Mantegazza of Milan, of which I gave a short *résumé* in the British and Foreign Medico-Chirurgical Review for July, 1859. Although I have found it alluded to in works that dealt with the Indians of South America, I have never had the good fortune to meet with any medical man who had a personal acquaintance with it, nor till now have I succeeded in obtaining the substance itself. Through the kindness of Mr. Fernandos, a chest of coca leaves, as they are used by the Indians of Bolivia, has reached me, and I have placed it with Mr. Squire, her Majesty's chemist, from whom samples may be obtained. The leaves are derived from the *Erythroxylon coca*, a shrub that is indigenous in South America. They are chewed by the Indians, who are stated by Mantegazza, Moreno, Guibourt, and others to go through laborious work for several days and nights in succession, without exhaustion, although they have little or no food, provided that they have a supply of about eight ounces of coca-leaves to chew during the day. An infusion of half a drachm to a cup of hot water is said to produce beneficial effects.—*British Medical Journal, Feb.* 21, 1874, *p.* 234.

125.—TOOTH-EDGED CUTTING SCISSORS.

By Dr. Benjamin W. Richardson, F.R.S.

I have recently had constructed for my use a pair of tooth-edged cutting scissors on the plan shown in the diagram. The scissors are of the ordinary construction in all respects except in the cutting edge. The cutting edge of each blade, instead of being even and sharp, is divided into finely-pointed



teeth, each tooth being directed with a slight inclination towards the handle of the scissors. When the blades meet the teeth cross each other, and as they pierce any structure that may lie between them, they crush also, between their surfaces.

If a piece of moderately firm substance be placed between the blades—a piece of paper or of thin card, for example—the scissors perforate it in a series of perforations resembling what is seen in the postage stamp—that is to say, they do not cut clean through the substance so as to leave it in two distinct parts at once. A little lateral or half rotating movement of the closed blades is, however, sufficient to tear through the still connected lines of substance and to complete the separation. The same occurs if the substance placed between the blades be a portion of soft animal structure, only that more force is required in the lateral or rotating movement to cause complete separation. The parts punctured are crushed between the teeth, and are separated by the twist or torsion.

I find these scissors useful in dividing directly and quickly structures in which there are many minute bloodvessels, and which, when divided by the knife, bleed freely. These toothed scissors, as they can be made at one and the same time to pierce, crush, and twist, control bleeding remarkably.

It occurred to me at first to modify a pair of torsion forceps so as to make them cut with toothed edges at the part where they compress, and I used a forceps thus modified with success. Mr. Perkins, jun., of Baker-street, brought me one day a patient with an epulis. The gum beneath the base of the tumour was much thickened and vascular, but with my toothed forceps I cut through easily and removed the whole mass quickly and thoroughly, effecting a good cure. But I found a want of purchase in handling the forceps, so I determined to carry out the method with scissors as above described. Mr. Perkins, who, as a dentist, is as mechanically skilful as he is energetic, earnest, and intelligent in his professional work, was good enough to make a model of my design out of a pair of ordinary surgical scissors, and from this model Messrs. Krohne and Sesemann, with their usual facility, have constructed the special instrument figured in the diagram.

I have put the scissors to a good test in a case of epulis in a patient under the care of my friend Dr. Davson. In this instance the tumour was increasing rapidly, and three teeth were involved in it. It was very vascular, had a broad base, and might at first sight have been taken for a malignant rather than for a fibrous tumour. The three teeth being extracted, I found I could get a deep grasp of the tumour between the blades of the scissors. I carried the teeth of the scissors well through the base of the tumour, crushing some portion of bone in the way, and gently and easily twisted the mass off, and lifted it away upon the blades without the loss of any blood whatever. The healing in this case was rapid and good.

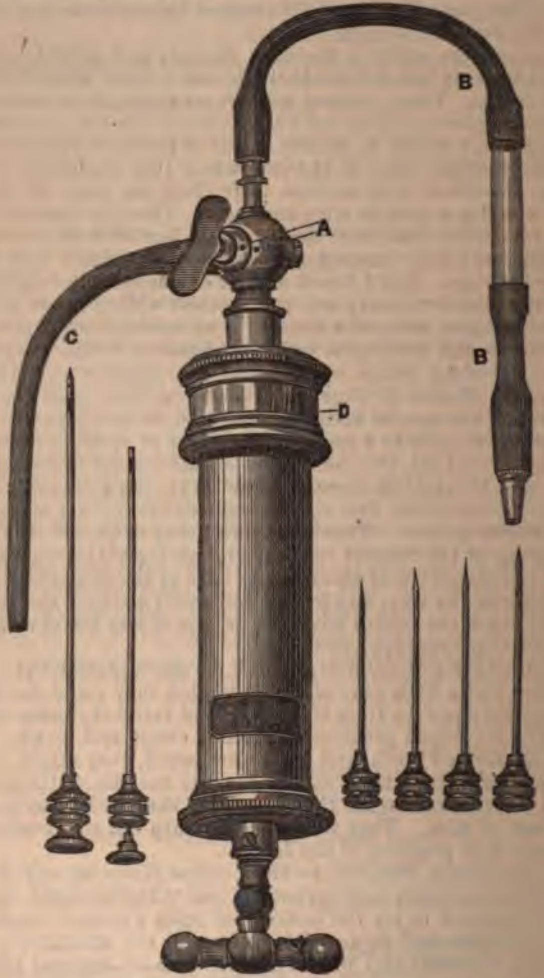
These tooth-edged scissors may, I think, be usefully modified. Made with longer handles and a little curved in the blades, they could be used for removing abnormal growths from the surfaces of internal organs—such as the uterus—by the combined processes of pierce, crush, and twist. Made with very delicately formed teeth, and altogether small, they might be used for removal of abnormal growths from the ear, the nostrils, or the eyelids. They could easily be constructed for the removal of the tonsils, and they are ready for use in cases of piles. They will probably turn out serviceable in operations for removal of portions of the tongue.

[Mr. C. F. MAUNDER, Surgeon to the London Hospital, one day accidentally saw Dr. Richardson's new invention, the "Tooth-edged Cutting Scissors," and determined to try the instrument upon a case of internal hemorrhoids upon which he had arranged to operate in the afternoon.]

The case was a typical and uncomplicated one of internal piles, which I proposed to ligature. Having seized one hemorrhoid, I proceeded to sever with the scissors the mucous membrane at its junction with the skin at the base of the pile, in order to form a groove for the ligature to lie in. This instrument effected my object most readily, and *without* bleeding, the structure *divided, requiring perhaps* two applications of the toothed scissors *where one of the* common scissors suffices. My colleague Mr. McCarthy, and Mr. R. Kershaw, late House Surgeon of the London Hospital, who assisted me, noted the absence of bleeding. Five hemorrhoidal tumours were thus treated.—*Med. Times and Gazette, Dec.* 20 *and* 27, 1873, *pp.* 689, 714.

126.—COXETER AND SON'S ASPIRATOR.

Various modifications of the aspirator have been brought out by different instrument-makers; and Messrs.Coxeter's is one of the latest, if not the best. The objection to the glass cylinder adopted in many instruments is the difficulty, if not impossibility, of making it so true that the piston shall fit accurately throughout and maintain a perfect vacuum. In this new instrument Messrs. Coxeter have reverted to the brass cylinder, but have fitted to its lower end a narrow glass ring, through which the character of the fluid withdrawn can be readily seen. The aspirator is fitted with an ingenious



"three-way" stop-cock, by which the use of a second stop-cock, which is always troublesome in use, is dispensed with. As it is found impossible to make the ordinary flute-key stop-cock of the stomach-pump sufficiently air-tight for purposes of aspiration, this arrangement seems to be the most satisfactory which can be obtained. In addition to the glass in the cylinder, the tube to which the aspirating needle is attached is fitted with a piece of glass tubing, so that the nature of even a very small quantity of fluid may be at once ascertained. The needles themselves are made of seamless steel, and are, therefore, more durable, and less liable to become impermeable from rust.—*Lancet, Feb.* 14, 1874, *p.* 234.

127.—TREATMENT OF BRONCHOCELE BY PHOSPHORUS.

By S. MESSENGER BRADLEY, Esq., Manchester.

I should like to mention a disease which appears to be influenced in the most beneficial and rapid manner by the administration of phosphorus—I mean bronchocele. I have only notes of one case, but this is an instructive one. The patient, a girl aged 20, had an increasing goître for some months, when she was placed on the usual iodine treatment, with no benefit whatever. After a month's interval of no treatment, phosphorus was given in doses ranging from the one-fiftieth to the one-twentieth of a grain, with the effect of directly causing a diminution in the size of the gland; the decrease went on steadily until the neck, in a month, measured two inches less than it did before the commencement of the treatment.—*British Medical Journal, Nov.* 29, 1873, *p.* 630.

128.—ON ONYCHIA MALIGNA.

By WILLIAM MACCORMAC, Esq., Surgeon to and Lecturer on Surgery at St. Thomas's Hospital.

[This disease, as is well known, is of the most inveterate nature, the pain being most intolerable—all local treatment, and even the evulsion of the nail, generally producing but temporary good effects. The only treatment which is effectual is the complete excision of the secreting structures at the root of the nail. This is a severe operation.]

The appearance of the ulcer is something quite peculiar. It has an ashy-coloured surface, with shreds of tissue attached to it; often red points dot it here and there, and the discharge has an intolerable smell. The edges, owing to the inflammatory swelling, are much elevated, and are often as sharply defined as possible. The pain is usually continuous, and very severe, of a gnawing character, aggravated by warmth, and preventing sleep in many cases.

The malady, if not interfered with, continues for years, there being little or no tendency to spontaneous cure. There is a great tendency to relapse after simple evulsion of the nail, while complete excision of the matrix ensures a permanent cure.

It is the opinion of some eminent surgeons, that the disease is often the extension of hereditary syphilis; but I could never satisfy myself that there was any such connexion. I believe it, at all events, to be rare. In the many cases I observed, the affection seemed always clearly traceable to local conditions. Often it would arise in persons of otherwise unimpaired health; but very frequently it was observed amongst the indifferently fed overworked factory hands.

From what I have been able to learn, it is in most places a rare disease. Mr. T. Smith, in vol. v. of Holmes's Surgery, states that it is almost confined to children under ten years of age, and is usually observed on the fingers.

Amongst seven thousand surgical out patients under twelve years, he noticed only nine cases, and these occurred between the ages of one year and seven.

My attention was recently recalled to the subject of onychia by reading an elaborate monograph on the disease by Professor Vanzetti, of the University of Padua. After giving many most interesting historical and other details, he recites the particulars of twelve cases of onychia maligna treated by the application of powdered nitrate of lead to the surface of the ulcer. The result seems to have been simply marvellous. In each case, a speedy and permanent cure rapidly followed, with the reproduction of a perfectly healthy nail. Dr. Vanzetti's antecedent experience had been somewhat similar to my own; he had exhausted, without satisfaction, all sorts of remedies, and relied on excision for a cure—a means which, on account of its severity, he very reluctantly employed.

Very shortly after the first application, all pain ceases; the swelling soon begins to diminish; also the suppuration, which loses its fœtor; and it is not rare, at the end of five or six days to see a perfectly healthy healing sore.

Professor Vanzetti was much impressed with these statements, and determined to give the remedy a trial at the earliest opportunity. After a time, a boy of seven presented himself in his *clinique* with onychia of the middle finger of one year's duration. The pain was so excessive as to cause a species of convulsion every night. No form of treatment had produced the slightest benefit. The last phalanx was double its natural size, livid red in colour, ulcerated along the sides and upper edge of the nail. The loose portion of nail was removed by scissors, so that the powder might be freely applied to every part of the diseased surface. Considerable pain was complained of until evening, but then the child slept soundly all night. On removing the hard crust which formed over it, the ulcer was seen, two days afterwards, to be looking well. Three applications in all were made, and in eleven days the sore completely healed up. A year afterwards the little patient was seen by Dr. Vanzetti, who found that a new and perfectly well formed healthy nail had grown, only differing from that on the opposite hand in being a trifle larger.—*British Medical Journal, Dec.* 6, 1873, *p.* 653.

SYNOPSIS,

(Arranged Alphabetically), containing

A Short Abstract of the most Practical Articles in this Volume, showing at a glance the most important Indications of Treatment published by different Writers within the Half-Year.

AFFECTIONS OF THE SYSTEM GENERALLY.

Bronchocele.—*Phosphorus.*—If the usual iodine treatment proves of no service, try the internal administration of phosphorus in doses of from one-fiftieth to one-twentieth of a grain. (Mr. S. M. Bradley, Brit. Med. Jour., Nov. 29, p. 630.)

Fever.—*Cold Baths.*—The use of cold baths in fever is of great value when the temperature is very high. The cold, however, does not restrain the excessive heat, it only relieves from its consequences. Its action is thus diametrically opposed to that of alcohol, quinine, and other agents, which directly moderate the febrile combustion; its *rôle* is that of spoliation, not of economy of waste. In slow, chronic fever, there is no danger from the elevation of temperature, but in active pyrexias the danger is great, and our duty before all things is to cool the blood by the rapid extraction of heat from it; an indication which hydrotherapy alone is capable of fulfilling. By means of a few baths administered in each twenty-four hours, we succeed in beating down and mastering a febrile movement which everything seemed to show would be violent and lasting. In satisfying this essential indication, the lowering of temperature, hydrotherapy weakens at the same stroke almost the whole group of the symptoms of typhoid pyrexia. (Prof. Behier, p. 26.)

Alcohol as a Reducer of Temperature in Fevers.—Abundant evidence goes to prove that alcohol possesses the power in acute pyrexial diseases of fulfilling those two objects so desirable in the treatment, viz., reducing the temperature and checking the tissue-waste of the body. Hence, the "empirical facts" of Todd are borne out by the results of scientific investigation. In the early days of typhus fever the urea excretion is *double*, implying great waste of the muscular tissues, including the heart. How important in this disease, then, must be the early use of a remedy which possesses the power of checking this morbific process! (Dr. Anstie, Practitioner, Dec., p. 22.)

Quinine as an Antipyretic.—Quinine has a certain and marked antipyretic power, but it is necessary to use it in suitable stages of proper cases. It is no use giving heavy doses of quinine to depress the elevation of the first four days of typhus or pneumonia, or in the hyperpyrexia of rheumatic fever, in all which it would probably be positively harmful. When we have to meet immediate danger from a rapid rise, I put no trust in quinine, but resort at once to cold baths, bags of ice, and the like. If, however, a high temperature is doing harm at the latter part of a period, quinine will often act like a charm. If, for example, towards the end of the third week of typhoid, a temperature of 104°.5 or 105° is likely to be too much for the frame already too far consumed to resist it as it would once have done, then 20 grains of quinine is often very valuable; and such doses, repeated as may be required, may determine the close of the period and release the sufferer. In the second week we get *no such success* as this. (Dr. T. C. Allbutt, p. 24.)

Lumbago.—*If internal* remedies, such as iodide of potassium and opium, have had no appreciable effect, apply a continuous galvanic current, at the same time making the patient exercise the affected muscles rhythmically, by bending, extending and rotating the spine. From 10 to 18 cells of Mayer and Meltzer's zinc carbon battery should be employed. The positive pole is to be placed at the upper part of the spinal column, in the middle line, and the whole back, including the intercostal muscles, is to be sponged with the negative pole. (Dr. G. V. Pooer, Lancet, Dec. 27, p. 899.)

Rheumatic Fever.—*Quinine.*—Excess of acid is one of the never-failing accompaniments of rheumatism. In the treatment of this disease usually, we pour large and frequent doses of alkali into the blood, in the hope that it will be distributed to every part of the body; but we find that, in spite of all our efforts, with the exception of the kidney secretion through which the excess of alkali is being constantly expelled, the acid continues to show itself wherever we can search for it; the other secretions continuing acid in spite of the excess of alkali in the blood. The endeavour to neutralise the acid is clearly rational, but it may be questioned how far we succeed in attaining our object. In fact, whatever may be said of the alkaline treatment of rheumatic fever, it cannot be called curative in the proper sense of the term. It may help to shorten the duration of the attack, it may serve to lessen the intensity of the inflammation, it may diminish pain, and it may not unfrequently ward off those more serious consequences which give to rheumatic fever its formidable character as a cause of heart-disease. All this it may do, and in my own opinion and that of many others who have made the experiment, it does actually accomplish, but it does not cure. It does not put an end to the paroxysm, as bark puts an end to an attack of ague; it does not eradicate the disease, as iodide of potash will eradicate the traces of syphilis, commencing as soon as the system is sufficiently saturated by full doses of the remedy. Many cases of acute rheumatism treated with alkalies give indications of depression, which can only be explained as the effect of the long-continued use of the remedy. Various means have been tried to meet cases of this kind. Stimulants avail to a certain extent, but have seemed to me to tend of themselves to keep up the disease, to lengthen its duration, and even to re-excite it after it has subsided. Quinine is that which, on the whole, has answered best in my experience. There is frequently in these cases phosphatic deposit, and this deposit rapidly disappears under the use of quinine. (Dr. A. W. Barclay, p. 44.)

Acids.—We must all feel dissatisfaction with the ordinary methods of treating acute rheumatism. There is no proof whatever, that the alkaline method is of any value, and much that mint-water produces quite as good results. In one case 15 drops of dilute nitro-hydrochloric acid was given every four hours, with the effect of relieving the joint affection almost completely in nine days, —curiously enough the urine being alkaline on recovery, although no alkali had been administered. Dr. Wilks maintains that, until we know everything concerning the disease, and especially the relation between the joint affection and the internal complications, we have no scientific basis for the selection of remedies. It is even doubtful whether the subsidence of the arthritic inflammation during the course of the disease is desirable, for in all fatal forms of rheumatism this is usually seen to occur. It may be remarked, however, that sometimes, in cases of internal complication, it does seem as if large doses of potash or of quinine have some influence in arresting the disease. In several cases treated by quinine Dr. Wilks has noticed the urine to become alkaline on recovery, as if this were the natural process of cure; as is seen also in typhoid fever. (Dr. S. Wilks, p. 48.)

AFFECTIONS OF THE NERVOUS SYSTEM.

Feigned Hemiplegia.—*Detection of.*—In these days of railway accidents it is necessary that we should have clear ideas of the means of diagnosis between real and feigned diseases. Where treatment by powerful shocks of electricity is available, it will usually be easy to cause such pain as will lead to the exposure of feigned anæsthesia. The symptoms of motor paralysis must be carefully investigated; and if the paralysis is complete, or nearly so, it will not be difficult to distinguish between the real and the feigned disease. Several tests are available. 1. The well-known appearance of dragging in a paralysed leg. This symptom was at first entirely absent in the case related, the slight dragging which was subsequently observed differed from that of ordinary paralysis. 2. The test suggested by Dr. Hughlings Jackson. In real hemiplegia the paralysed arm falls forward when the patient stoops; in case of feigning, the arm will probably be retained by the side. 3. The condition of the muscles in long-continued paralysis. Where paralysis has continued for several months in a limb, we should expect that the muscles would both diminish in size and lose their electro-contractility. 4. In cases where the patient says that he has been confined to bed for many weeks, the condition of the soles of his feet may be taken as a test of the correctness of this statement. A scaly and cracked condition of the cuticle of the soles will, according to our experience, be invariably found in adults (at least in the lower ranks of life) who have been confined to bed for a considerable time. Where anæsthesia is feigned, it may be detected by the unexpected application of a needle—the patient's attention being at the time taken up by the examination of another part of the body, such as the eyes. (Dr. A. Davidson, p. 57.)

MALINGERING.—Where the physician's suspicions are aroused, faradisation of the skin with a wire brush and a powerful current, may be an excellent means of settling such questions. This proceeding is extremely painful, and yet never does any harm, except in cases of cerebral disease, where it should be avoided. We need, therefore, not be reluctant to employ it in suspicious cases, as it is far more humane than the actual cautery which has often been called into requisition, and yet sufficiently disagreeable to make a repetition of the procedure dreaded by such persons. As in peripheral paralysis the muscles always lose their faradic excitability, the presence of the faradic response in the muscles of the arm enabled us in one case to state decisively that the man was shamming. (Editor of Brit. Med. Journal, p. 60.)

NERVOUS MIMICRY OF DISEASES OF THE SPINE.—It is very necessary to diagnose nervous mimicry of diseases of the spine from true spinal disease. If there be no considerable attendant illness, an intense and horrible pain in or at the spine does not mean serious disease of the spine. Pain of this kind may be due to aneurism or cancer, or to some distant malady in a nervous constitution, but it is not a sign of primary disease of the spine. Excessive tenderness is also characteristic of purely nervous disorder. If the patient flinches or writhes when the finger taps or presses very gently, we may be sure it is not real spinal disease. Another usual character of this pain is its frequently extreme increase in fatigue, not only in bodily fatigue, as in long sitting upright or standing, but in long occupation of any kind, even in mental occupation if it is not very agreeable. With this character, also, it is common for the pain to be accompanied by nausea or even vomiting, and it is a nearly sure sign of merely nervous disorder if pressure on the spine produces shivering or nausea, or a feeling of going to be sick. And, lastly, it is a sign of nervous pain alone if the pain has lasted many weeks or months and nothing has come of it; no immobility of the spine or ribs, no paralysis, no fever, or wasting, or great disturbance of the general health. When we find a patient carefully—not with muscular spasm, but with care—holding his head and neck, or any part of his back very still, turning himself or bending cautiously, we must look for disease of the spine. The opposite state of this in which the patient tumbles this way or that, like a baby, is sure to be without organic disease. (Sir J. Paget, p. 49.)

NEURALGIA.—*Phosphorus.*—After trying several preparations, I now use a formula which Mr. Potts, dispenser to the Manchester Royal Infirmary, hit upon, and which seems to answer every purpose, in being tasteless, transparent, and readily prepared. He dissolves ten grains of phosphorus in two ounces of ether, agitating the solution from time to time; and of this solution, one minim (containing one-hundredth of a grain) is administered in an ounce of water with half a drachm of glycerine. The glycerine suspends the phosphorus so perfectly that a transparent mixture is the result. The addition of a little bitter infusion entirely removes any *soupcon* of lucifer-matches which may hover about the medicine. (Mr. S. M. Bradley, p. 61.)

Supraorbital Neuralgia.—Subcutaneous Injection of Strychnia.—A case of supraorbital neuralgia, which had not been relieved by the administration of quinine, was cured by the subcutaneous injection of strychnia. The solution used contained four grains to the ounce, and of this two minims were injected at a time. The first injection gave relief, but the pain did not completely disappear till a third had been given. The injections were given at intervals of two days. (R. A. J., p. 60.)

PHANTOM TUMOURS OF THE ABDOMEN.—There is often some little difficulty in diagnosing phantom tumours when they occur in the abdominal muscles. They are sometimes large, hard, and nearly fixed in place, size, and form; and they may be deceptively complicated with disorderly states of the intestines, or the aorta, or other abdominal or pelvic organs. But in any case, however difficult the diagnosis, the use of ether or chloroform will decide the matter at once. With complete insensibility all signs of tumour vanish. (Sir J. Paget, p. 53.)

TETANUS.—*Injection of Chloral.*—In a case of traumatic tetanus in which the mouth became so closed that no remedy could be administered, M. Oré of Bordeaux threw an injection containing ten grammes of chloral into the veins, with the effect of producing peaceful sleep; and this was followed by a second and third injection, with the effect of obtaining a sleep of eight hours. (M. Oré, p. 61.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

ANEURISMS.—*Galvano-puncture.*—It is only sacculated aneurisms of the arch of the aorta or of other great arteries of the body which can be treated by galvano-puncture with any prospect of success. We may get all the good and none of the bad effects from Fobeaux's modification of Smee's battery, made by Weiss and Co. It is upon the question as to the most advantageous mode of employing the current that the greatest differences of opinion exist. Some writers recommend the insertion of the negative pole only into the sac, whilst the positive pole is applied to the integuments outside. Others recommend the introduction of two needles into the sac, one of which is to be connected with the positive pole and one with the negative, the current being allowed to pass all the time in the same direction, and in the hands of different operators, being either strong for a short time, weak for a long time, or even strong for a long time. But some of those who recommend the insertion of both poles into the sac, follow Pétrequin in advising that the direction of the current should be changed from time to time, during the same operation. And, lastly, a third set of writers recommend the introduction of a needle or needles in connection with the positive pole into the sac itself, whilst the negative pole is applied by means of a wet sponge to the adjacent skin. The introduction of the positive pole alone, with the application of the negative pole to the neighbouring parts, always brought about a rapid and complete coagulation. It would seem, therefore, that this mode of procedure is the best, even from the point of view of the readiness with which coagulation is brought about, and its satisfactory nature. The needles previously employed by most operators seem to have been larger than it is at all necessary to use, whilst some of them, such as Weiss's modification of Dr. Duncan's needle, are unquestionably very much too large. Recognising the desirability of reducing to a minimum the chance of hemorrhage through the punctures and of inflammation in the track of the needle, and, at the same time, of diminishing the painfulness of the operation, I was led to use a very small hare-lip needle. The experiments of Dr. Fraser and others have unmistakably established the fact that the coagulation induced in the blood by the passage through it of a galvanic current, is wholly attributable to the electrolytic or chemical changes which the current occasions. If, therefore, after the fashion originally introduced by Steinlein, we cover the positive pole with an easily oxidisable metal, such as zinc, we are enabled to summon to our aid what Faraday always distinguished as the 'secondary' effects of electrolysis. The salts as well as the fluid of the blood are decomposed by the action of the current, and a part of the oxygen and acids which are liberated at the positive pole will combine with the zinc and corrode the surface of the needle. Some of the salts thus formed will aid in producing further coagulation around the needle. With regard to the strength of the current, it was found that eleven cells acting for half an hour, although producing no pain or obvious sensation at the time, did induce some amount of irritation of the skin around the punctures. The current from eight cells, however (the battery being in good condition at the time), produced, on the previous occasion, no irritation, so that all traces of this needle-puncture had disappeared in four or five days. This needle also was insulated within half an inch of its point, merely by two layers of spirit varnish; and probably, in employing a comparatively weak current such as this, no insulating material is needed. This, indeed, is the opinion of Dr. Althaus. At all events, it seems desirable that the needle should not be pushed too far into the sac, and that the part of the needle actually in the sac and within its coats should not be insulated, because clot should form in contact with the wall of the aneurism, so that it may be left adhering to it on the withdrawal of the needle. (Dr. H. Charlton Bastian, p. 136.)

Aneurism of the External Iliac Artery.—Lister's Abdominal Tourniquet.—Aneurism of the external iliac artery may be cured by pressure upon the abdominal aorta just above its bifurcation. Chloroform should first be administered, and ether substituted for it as soon as the muscles are relaxed. In a case related by Mr. Wheelhouse the anæsthesia was kept up continuously for five hours, without any untoward symptoms, twenty-five ounces of ether being used. "Lister's large abdominal tourniquet was applied just over the umbilicus, and slowly screwed down until the flow of blood through the aneurism was arrested. Two slips of the instrument occurred during the first half hour, but it was at once replaced. Pressure was commenced at 1.15 p.m. The foot was cold and a little blue at 2.30 p.m.; at 3 p.m. the tourniquet was unscrewed slightly; pulsation in the tumour returned, and it was at once reapplied. At 4 p.m. blueness had extended beyond the knee, the left leg being very cold; at 5 p.m. the right limb was blue to the groin, and the left to the knee. Pressure was then slightly relaxed; the tumour much harder, but pulsation still perceptible. At 6 p.m. both limbs were black, and body blue as far as tourniquet. The instrument was then removed gradually in fifteen minutes, a quarter turn of the handle being taken every minute. The tumour had ceased to pulsate, and was firm and hard." (Mr. C. G. Wheelhouse, p. 135.)

Artery and Torsion Forceps.—*Mr. Spencer Wells's New.*—These forceps have scissors handles. The grasping and holding extremity is roughened by rather deeply cut transverse teeth, so that the bleeding vessel is forcibly compressed, and its coats squeezed or almost crushed together. This is alone often sufficient to stop the bleeding without any torsion, especially if the instrument be left on the vessel for a minute or more. But if the vessel be large, then two or more rotations may be added. Instead of the spring catch, the fastening is effected by a Mathieu's catch in the handles. This is quite as easily fixed and opened as the spring, and is much less likely to get out of order. The instrument is made of steel, but is coated with nickel, which prevents any rusting after use. (Mr. T. S. Wells, p. 144.)

Erectile Tumours.—*Croton Oil.*—It is sometimes difficult to eradicate small erectile tumours. The following plan is very successful and easy of application: Take a piece of cork and pass through it a number of fine needles, so that the points protrude for about two millimètres only. These points should be disposed so as to represent the shape and size of the spot to be operated on. The cork is now to be dipped in croton oil, and then accurately applied over the spot and quickly pressed upon it. A little cotton wool is to be used for dressing. The next day there is a little swelling and vesication. A second application at the interval of a few days will, if as successful as the case from which this is taken, cure the nævus without leaving any scar. (Dr. De Smet, p. 150.)

EPISTAXIS.—*Improved Plug.*—In severe cases of epistaxis the nose may be plugged by means of an elastic india-rubber bag inflated after introduction. For this purpose, I cut a No. 4, gum-elastic catheter into two equal lengths. At the end of one portion, I made small holes by transfixing it with a needle heated to redness in a spirit-lamp. These holes extended, at intervals of a quarter of an inch, for the length of about two and a half inches. Over this punctured portion I tied firmly in two places, first at the tip, and then again about three and a half inches nearer the handle, a soft India-rubber bag, so as to include between the two tied portions all the perforated part of the catheter. To the open end of this catheter I fixed a short piece of India-rubber tubing furnished with a small stopcock. All the air having been let out of the bag by opening the stopcock and gently compressing the India-rubber, somewhat after the manner of fastening an umbrella, the instrument is passed into the bleeding nostril. When it is coaxed so far that the end projects into the posterior nares, or as far as it can be, the nose of a small ball-syringe is applied to the mouth of the stopcock, and the bag is gently inflated while the air is being propelled into it with the right hand. The short piece of India-rubber tubing attached to the stopcock is compressed with the thumb and finger of the left hand at the right moment to prevent the air from passing out again; and then with the right hand the stopcock is turned. By this means, the inflated bag is made to fill all the inequalities of the nasal cavity, and a painless and perfect plug results. (Dr. H. C. Rose, p. 148.)

MIMICRY OF ABDOMINAL AND OTHER ANEURISMS.—In hysterical females there sometimes occurs an abnormal nervous condition of the abdominal aorta, which closely simulates aneurism. The same thing may occur in the subclavian, innominate, and carotid arteries, but more rarely. The chief characteristic of the mimicry is that the affected artery feels much larger than it should be, and pulsates largely at least in one direction, dilating obtrusively, and often so much more visibly than in an ordinary person that it is hard not to believe that it is largely dilated. And yet there certainly is no considerable dilatation; it is rather as if the arterial walls were thin and had too little muscular resistance, as we might suppose them to be in a condition of partial paralysis of their muscular tissue. In the most marked case of imitation of abdominal aneurism that I have seen, examination after death found no disease. (Sir J. Paget, p. 53.)

NÆVUS.—*Sudden Death from Injection of Perchloride of Iron.*—Mr. Kesteven relates a fatal case from injection of five minims of perchloride of iron into a nævus. The child suddenly turned pale and was slightly convulsed, at the same time that it began a series of eight or nine shrill laryngismal cries, attended with distinct struggles to recover its breath, which suddenly ceased in death. In the Lancet for August 17th, 1867, Mr. Thomas Smith of St. Bartholomew's, has collected several fatal instances following shortly upon the injection of nævi with perchloride of iron. In these cases the nævi were situated upon some part of the face or near the veins in the neck. As the result of these fatal consequences, Mr. Smith concludes:—"Sufficient is known of the effect of the possible admixture of perchloride of iron with the general circulation, from injecting nævi on the face, to justify us in rejecting it as a remedy for nævi in these parts, unless, by pressure or by the employment of some instrument, the circulation in the growth is controlled, at least for some time." (Mr. W. B. Kesteven, p. 150.)

Fatal Results of Treatment by Injection of Perchloride of Iron.—However tempting it may appear to treat nævus in a prominent situation on the face by injection of perchloride of iron, because of the absence of scar, this mode of operation must on no account be adopted. Mr. West, of Birmingham, relates a case in which death resulted from embolism; a clot was found stretching from the right internal carotid artery along the middle cerebral artery of the same side. (Mr. J. T. West, Lancet, Mar. 21, p. 492.)

TORSION OF ARTERIES.—The principles of torsion are based on a knowledge of the anatomy and physiology of arteries. The arteries contain in their walls a muscular tissue, which contracts and retracts whenever the vessel is divided, the more so if it is rudely lacerated. An objection to torsion is to be found in the fact that the arteries are liable to be extensively diseased, s that it no longer fulfils its natural functions. Torsion is, moreover, not adapted for cases where the artery cannot be sufficiently withdrawn to allow of free twisting, and is useless when hemorrhage occurs from the side of one vessel, as when a branch has been divided close to the trunk. It appears, therefore, that torsion requires for its complete and safe employment a healthy subject, sound arteries, good instruments, and a persevering operator. The principles of torsion are, that by twisting the vessel the muscular coat is ruptured and becomes contracted, and that its roughened edges act as foreign bodies, which whip out the fibrin, and on which lymph is subsequently deposited; it seems practically to be a matter of small importance whether the end of the vessel is twisted off, or whether only a few turns are made. But the question in dispute is whether the bruised and twisted end sloughs off or retains its vitality. The truth is, that much depends on the size of the vessel and the manner in which torsion is performed, for if the piece have not been severely bruised it may preserve its vitality; but if it have been twisted too frequently, or if the tissue be much broken, there can be no doubt that the end does often slough off and act as a foreign body. (Editor of Lancet, p. 125.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

PLEURISY.—There are some cases of very limited pleurisy, having but little tendency to spread, which may be checked by mechanically fixing the entire side, so as to restrain the movements as much as possible. The best plan is to use strips of plaster, putting on two or three layers in the following manner:—The first strip is laid on obliquely *in the direction of the ribs*, the second *across the course of the ribs*, the third in the direction of the first, about half overlapping it, the fourth as the second, and so on until the entire side is covered. A strip is also passed over the shoulder, which is kept down by another fixed round the side across its ends. (Dr. F. T. Roberts, p. 162.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

DYSPEPSIA.—*Papya Juice a Solvent of Nitrogenous Food.*—The papya tree is indigenous to tropical climates—it is common in Bengal. When the unripe food is scarified, it yields an abundant flow of a milky juice. This juice when added in very small quantities, such as a few drops, to any nitrogenous articles of food, immersed in a little water, has a wonderful solvent action upon them. It is extensively used for this purpose amongst the native cooks in India. A few drops of the fresh juice were added to a pound of minced goat's meat, which was stewed over a slow fire. The whole ran into a diffluent mass in five minutes, owing to a larger quantity of the juice having been used than was necessary to make the meat tender and eatable. What the peculiar nature of the action is, is uncertain; it is not that of a caustic, for the juice has no taste of consequence. The whole action is so identical to healthy digestion that I wonder we have not availed ourselves of this medicinal property, in cases of invalids and dyspeptics, to substitute a process of artificial digestion. A few grains taken immediately after a meal will substitute the power where it is wanting. For medicinal use the fresh juice should be dried in the sun in shallow dishes. (Dr. G. C. Roy, p. 64.)

CLEFT IN THE HARD PALATE.—*New Operation.*—Sir W. Fergusson has successfully adopted a new plan of operating. The first steps of this operation are somewhat similar to the old operation for closing the cleft in the hard palate—namely, paring the edges of the cleft, and making an incision down to the bone parallel to, and about a quarter of an inch from, the edge of the cleft on either side, the point of the knife being carried back just as far as the junction between the hard and soft palate. Into these incisions a chisel half an inch broad is inserted, and its edge directed against the posterior margin of the hard palate and made to cut from behind forwards, thus partly detaching a slice of bone on each side, with the soft tissues and periosteum attached to their upper and lower surfaces. The result of this is that the sides of the cleft fall easily together, leaving a small aperture through the bone on either side. One, two, or, if the fissure be long, three stitches are passed through the lateral clefts by means of an ordinary aneurism-needle, and thus encircle the detached portions of bone and soft tissue, each suture passing through into the nasal cavity. From the liability of the flaps to twist in slightly, and from the thinness of the edge, Sir William Fergusson is careful to pare the sides somewhat obliquely, in order to present wider raw surface for adhesion. The sutures, which are kept in much longer than in the ordinary operation, cause no harmful irritation. The lateral clefts become filled up by new bone, which is rapidly thrown out and tends to keep the parts firmly united in the median line. (Sir W. Fergusson, p. 153.)

Epithelioma of the Lip.—*The Elastic Ligature.*—The author relates a case in which the lip was pierced at a spot below, but quite clear of, the disease, and two india-rubber cords passed, by means of which the whole mass of disease was included. Fourteen days were required for the complete separation of the included part. Powdered charcoal had to be kept upon the part on account of the smell. (Dr. H. S. Gale, p. 113.)

Fistula-in-ano.—*The Elastic Ligature.*—There are some cases in which from the patient's habits and constitution hemorrhage may be a matter of serious consequence. If, therefore, in these persons the fistula is one of considerable length, the elastic ligature is preferable to the knife. The ligature is introduced double by means of a probe, which is then again withdrawn, leaving the loop high up within the rectum. The loop is then drawn out, and the double ligature having been put tightly on the stretch is securely fastened. A morphine suppository will effectually relieve the pain produced. Although I entertain grave doubts as to the alleged immunity from the occurrence of pyæmia, which Professor Dittel claims for this method, I am confident it will be in many instances preferred by operating surgeons to cutting instruments, when, as Professor Dittel observes, "he has to undertake operations in cavities and canals which are out of sight, or which are so narrow that it is extremely difficult, if not impossible, to use cutting instruments within them, or when he feels uncertain whether he may be able to tie the bleeding vessels (as in anal fistulæ situated high up). In other cases he will desire to avoid hemorrhage and the formation of large wounds in children or in old persons." (Prof. W. Stokes, p. 179.)

Hare-Lip.—It is much better to operate for the cure of hare-lip soon after birth than to delay the operation to a later period of life. The best time is three weeks to three months after birth. In the case of double hare-lip, with double cleft in the alveolar ridge, there may be great projection, or little or none. In the latter case, particularly if the columna and lateral portions of the lip be of good size, there may be no need for meddling with the intermaxillary mass. If, however, the projection be considerable, or what may be called great, and if the columna and side portion of lip be scanty, there ought then, in my opinion, to be no hesitation about taking away the projection at its junction with the vomer. The attempt to push this part back by gradual pressure is troublesome, or well-nigh impossible, in most instances, even if, as has been proposed, its narrow neck be broken. I never hesitate to remove the intermaxillary mass when it seems the least in the way of a satisfactory operation. The instances where there is no special projection are common, and require no comment, as there is then, as regards this matter, no obstacle to a satisfactory and successful operation; but, when there is a projection, if considerable, it is a more serious obstacle to these results than those inexperienced may imagine. I believe that this condition is a frequent cause of failure in the ordinary operation, particularly if it be done without the truss-compressor on each cheek to push the lateral portions of the lip towards the mesial line. The projecting portion should be removed as high up towards the nostril as possible, and here *in infants*, as there is only cartilage to divide, a pair of scissors is all that is necessary. Whilst I can offer little objection to this proceeding, I fancy that I have recently fallen on one equally efficacious and void of certain objections which, I think, might be urged against it. Instead of this sweeping wholesale abstraction, I content myself with making an incision, vertical, sloping, or horizontal, with a scalpel through the mucous membrane and periosteum, over the projecting piece of bone; with a few touches of the knife, or a little squeeze with finger and thumb, I so separate these tissues as to permit the entrance of a gouge of a quarter or three-eighths of an inch in breadth, with which I scoop out the body of the milk incisor-tooth in as far as it is formed, taking no heed of the cyst or of that of the permanent one, and even cut out such wall of bone as may be there; usually, at four or eight weeks, only small plates of bone. In this way, the hard projection is removed, and the tissues that remain offer no obstruction to the union of the junction of the lip in front, whilst the operation, as it appears to me, is less destructive, therefore more conservative, in character. There is thus left only the mucous membrane, with possibly some periosteum, which form a soft cushion behind the wound in the lip, and so the remaining intermaxillary bone is not divested of covering so thoroughly as when cutting instruments are passed in the mesial line to take all away on the offending side. (Sir Wm. Fergusson, p. 151.)

HEMORRHOIDS.—*The Elastic Ligature.*—This mode of operating is especially adapted for hemorrhoidal tumours, nævi, and warts, and I should have used it more frequently than I have done had not experience proved the superiority of the curved clamp and the actual cautery where there is no objection to their use. (Mr. Hy. Lee, Med. Times, Dec. 6, p. 648.)

Dr. Richardson's new "tooth-edged cutting scissors" are an admirable instrument for snipping a groove at the lower edge of internal hemorrhoids prior to ligaturing them, as no bleeding occurs and the view of the part is consequently not obscured. (Mr. C. F. Maunder, p. 249.)

STOMACH PUMP.—*Dr. Tosswill's Improved Syphon.*—The ordinary stomach pump is both costly and cumbrous; moreover, its use is not unattended with danger, cases having been known where the tube has been pushed through the coats of the stomach. A much simpler and better instrument is one which has been recently made for me by Messrs. Arnold of West Smithfield. "The advantages I claim for my instrument are—freedom from danger, cheapness, and increased portability. It can be easily carried in the coat-pocket, its cost does not exceed 15s., and it is almost impossible to perforate the coats of the stomach or œsophagus by means of it, be the operator ever so clumsy or ignorant. The instrument consists of an indiarubber tube, about half an inch in diameter and some four feet in length, provided with a ball of the same material, which is three or four inches in diameter, and situated about eighteen inches from one end of it. In fact, it is almost a *fac-simile* of one of Higginson's enema syringes, but without a valve." The tube is nothing more nor less than an enlarged Thompson's indiarubber catheter, with two large oval holes situated near the extremity. (Dr. Louis H. Tosswill, p. 70.)

THREAD-WORM.—Santonine, although almost a genuine specific for round-worm, is of little or no use in the treatment of thread-worm. Injections of quassia, lime water, salt, and castor oil, although affording partial relief, avail nothing towards a permanent cure. It is a mistake to suppose that the thread-worm inhabits only the rectum and sigmoid flexure of the colon. The fact is, that the entire length of the colon is the territory inhabited by the thread-worm. The cæcum itself constituting the parasites' true headquarters. It will be seen at once from this that we must rely on medicines given by the mouth, rather than on injections. Speaking generally, I give a preference to active saline cathartics repeated for several days in succession, followed by the use of cold water enemata. Small doses of chloric ether and sulphate of iron are eminently serviceable additions, and the same may be said of aloes, with or without asafœtida. In cases where these drugs are objected to, I have employed various active mineral waters with good results. Bitters of all kinds are useful, and patients who object to salines will swallow any reasonable amount of the infusion of gentian and other pure vegetable tonics. The rapid passage of the drugs through the bowel will, if several times repeated, carry most of the parasites sufficiently low down to be within reach of the clysters;

and the prevention of the return of the parasites to the upper part of the colon is one of the practical points worth looking to. The employment, therefore, of enemata, after the exhibition of remedies by the mouth, cannot fail to prove beneficial. (Dr. T. Spencer Cobbold, p. 71.)

Umbilical Hernia in Infants.—Push the hernia in with the forefinger, and then pinch up the skin of the abdomen on either side, folding it up with the finger and thumb and bringing over the folds so as to make them meet. The hernia now lies underneath the folds, which are represented by two parallel lines in contact with one another. Over them apply six strips of plaster—four transverse, two oblique. These plasters should be renewed every four days—but the whole of the strips should not be removed at once, but one by one; and as fast as one strip is removed, another fresh one should replace it. The advantage of this plan is that the edges of the hernial aperture are coaxed to approximate, rather than kept asunder, as by the use of the pad. (Mr. J. Gorham, p. 157.)

AFFECTIONS OF THE URINARY ORGANS.

Albuminuria and Bright's Disease.—The existence of albuminuria does not necessarily imply the presence of disease of the kidney. The urine may become nearly solid on the application of heat and addition of nitric acid, and yet may not vary in quantity or specific gravity from the healthy standard; nor yet contain any casts discoverable by the microscope. This condition of urine may occur, and not unfrequently does, in cases of vascular bronchocele and exophthalmos. The existence of blood, pus, or spermatic fluid in the urine renders it coagulable. Again, the excessive use of a diet composed chiefly or entirely of albuminous matter, such as eggs, has been found to produce albumen in the urine. There is a large number of febrile and inflammatory diseases, in the urine of which albumen very often occurs. Among these may be mentioned scarlet fever, measles, erysipelas, smallpox, diphtheria, typhus and typhoid fever, cholera, &c., and of inflammatory diseases, pneumonia. Over and above these relationships there are various forms of visceral disease—of disease affecting the heart, liver, and lungs—in the urine of which albumen appears. (Dr. J. W. Begbie, p. 73.)

Diabetes.—*Carbolic Acid.*—Many cases of diabetes arise from an increase in the ferment which converts amyloid substances into sugar in the liver. Starting from this theory, Drs. Ebstein and Müller, of Breslau, conceived the idea that substances which are known to arrest fermentation would be likely to diminish or arrest the formation of sugar. Creasote was well known to be inert. Carbolic acid has several advantages over creasote as a drug. Its composition is definite, its odour pleasanter, and it can be taken in much larger doses The carbolic acid was given dissolved in peppermint water, strength 1 gramme (=grains 15) to 300 grammes. This quantity was taken in three days—that is, about six to seven tablespoonfuls per diem. Two cases are related—in the first, after taking thirty grains of the acid, the sugar disappeared and did not return during the four months the patient was under observation. Three months after this, however, he reappeared, and his urine was found to be again diabetic; but the sugar disappeared on re-commencing the carbolic acid. The patient never had a purely animal diet. (Drs. Ebstein and Müller, p. 78.)

Extravasation of Urine.—In a case of injury to the urethra from accident, with obstruction of the canal and extravasation of urine, it is the proper treatment to make free incisions in the perineum for the escape of the urine, but not to retain a catheter in the urethra. The reason is, that a good deal of irritation is generally caused by the retention of a catheter, and the urine generally still flows by its side and through the perineal wound. "Could not this contraction of the urethra have been prevented by the retention of an instrument in the first place, and could not much time have been saved thereby? For my part, I say, No." "Though the urethra may be much bruised and very badly hurt by the injury, yet, for the most part, it exhibits a remarkable power of repair. Injuries which have destroyed the urethra in its whole circumference, or damaged it to an extent which may be measured even by inches, are yet repaired in such a way that the continuity of the canal is restored, although the calibre is of course contracted; so that any notion that the constant presence of an instrument is needed upon which the restored portion of the canal may be moulded can hardly be the result of clinical observation. But then again, Could not the contraction—the stricture—have been thereby prevented? No. Because this contraction—assuredly the worst part of it—is a subsequent process. To retain any instrument continuously for several weeks is of course, as a rule, out of the question. Even supposing it to be retained from the first for two or three weeks—and surely this is a liberal assumption, —still there will persist the most inveterate tendency to contraction long after this, and the hardest part of the battle has still to be fought." What is to be done, however, supposing the patient to experience increasing difficulty in passing urine as the perineal wounds heal, and the surgeon fails to introduce any instrument? The operation of perineal section then becomes necessary. (Mr. W. S. Savory, p. 169.)

LITHOTRITY.—*The Future of.*—I have come to the conclusion that stone in the bladder, like many other maladies, is an exterminable one. I believe that this great malady, which has tested surgical skill during two thousand years and produced a ponderous literature, which has been dreaded beyond all things by mankind, and has been the source of untold suffering to thousands in every age, is nevertheless a malady which may be exterminated; so far, that is, as it is a painful and dangerous one. The sources of danger in lithotrity are but two in number: injury to the soft parts by the instruments employed; and injury to those same parts from the sharp edges and angular forms of the fragments which are produced by the process. When we have reduced the mechanical action to the minimum of capability to inflict mischief, and have learned to make fragments in such a manner that they shall produce the least amount of irritation, why then we shall have arrived at perfection in lithotrity. With a calculus of certain weight and dimensions, whether of uric acid, phosphates, or oxalate of lime, say not exceeding the volume of an ordinary nut, a perfect result may be ensured. With the size of the stone, however, the danger increases. With stones of middle size a certain small proportion of deaths is met with, perhaps one in twelve or thirteen cases, and with stones of larger size the mortality is one in eight or ten cases. Then there must always be a number of cases in which the choice of operation hangs on the slightest circumstance. There is a wide border-ground, so to speak, which is common to both operations. No man, whatever be his experience, can lay down a hard and fast line, and say, "On this side lie the circumstances which determine lithotrity, and on the other side are the circumstances which make lithotomy imperative." The border-ground is much broader than I often like, and there are a good proportion of cases in which it is impossible to predict which operation of the two will be most successful: stones which are just large enough to be crushed, which are certainly not outside of the limits of size for lithotrity, and in patients of a certain class for whom a half-dozen sittings will not tell heavily. On the other hand, the crushing of the same stone in the sensitive patient with a worn-out nervous system may make too large a demand on his endurance. Thus you see the only certainty of attaining success lies in getting the stone to operate on when it is small. And success being absolute then, as I think I have been able to prove beyond all manner of doubt, it follows as a matter of necessity—That the Diagnosis of the presence of stone in the bladder and of its size is a matter of the highest importance. Next to the best operation for the removal of large stones from the bladder, the most important question is the best method of discovering the existence of very small stones in the bladder. First, respecting the mode of sounding. It is absolutely essential to employ a light sound, which can be easily turned in the bladder and urethra. Nothing but a quick and delicate movement will elicit an audible note, or produce a sense of contact, with so small a body as a pea lying in the interior of the bladder. Therefore it is better to have an instrument which will roll easily between the finger and the thumb, and not require the wrist or the arm to create the movement. Hence the handle should be cylindrical—like the handle, but smaller, which I originally designed for my lithotrite, and which is now much used here, and is almost universally employed abroad. The beak should be very short, so as to be turned with the utmost facility. To find a small stone the bladder should be empty or nearly so. I prefer a patient to make water a few minutes before sounding, and certainly never to be injected or prepared in any way, which only tends to defeat our object. Let him lie down with his pelvis a little raised, and then let the instrument gently glide down the urethra; it is five to one, however small the stone may be, that it is just grazed as the beak passes through the neck of the cavity into the bladder. This is perceived easily if the sound is only guided lightly by the faintest touch of the finger and thumb. Held and guided by the wrist and arm so slight a graze may be unnoticed. If not felt, let the sound make two or three quick semi-rotations right and left: if still nothing found, depress the handle slightly to turn the beak below, close to the neck of the bladder, and make two or three similar movements there. That is where the stone will be found, if there is one, in the nearly empty bladder; and in the same manner a small fragment will be found at the close of a case. I assert that more than half the stones I operate upon are found in cases in which no suspicion as to the real malady has arisen until the sound has been employed. Now, with the utmost deference to others, and only after the acquirement of a profound conviction on my part, I venture to say that it is my belief that the early signs of calculus are not generally sufficiently recognized." (Sir Henry Thompson, p. 158.)

STONE IN THE BLADDER.—*New Instruments for the Detection and Extraction of.*—Mr. William D. Napier describes some new instruments which we think will prove exceedingly useful. The Calculus Detector is a sound, the terminal two inches of which is coated with pure lead. Before introduction this is to be highly polished by friction with leather, and to be examined with a lens both before and after introduction into the bladder. The lead is so impressible that it is certain to be marked by contact with a rough substance like a calculus. The Calculus Extractor is for the removal of small stones and fragments of stone. "It consists of a soft India-rubber tube, terminating at one extremity in a funnel shaped orifice, greatly resembling the ordinary convolvulus flower, and is capable of containing a calculus or portion of calculus of considerable dimensions. When it is inserted into the bladder, its form and position render it the receptacle of such foreign substances as would from the position of the patient and the flow of the urine determine towards the natural outlet. This receptacle, enclosing such bodies in the folds of its delicate membrane, and permitting itself to be gently withdrawn, retains them in its elastic envelope, and protects the urethra from direct contact with them." The instrument is introduced into the bladder thus:—Having been soaped, it is inserted into a silver catheter open at the end, the whole, including the convolvulus-shaped cup, being withdrawn within the tube. The end is closed by a piece of cocoa butter, which lubricates the passage as the instrument is inserted, and rapidly melts in the bladder, so as to allow of the extrusion of the India-rubber cup. The Extractor may also be made use of as a self-retaining catheter. (Mr. W. D. Napier, p. 163.)

STRICTURE OF THE URETHRA.—The condition of the parts in stricture of the urethra is hardly ever a purely physical one. With the structural change there is associated an active morbid state, either some inflammation or congestion of the parts, and almost always more or less of spasm. How else can we account for the varying size of the stream, even when permanently diminished? It follows from this that the plan of treating stricture by purely physical means is founded on a misconception. The patient should be kept in bed in order to secure complete rest in the horizontal position and uniform warmth. Opium has great power in allaying irritability; it is best administered as laudanum by the rectum. Active aperients are often strikingly serviceable. The use of instruments must be kept in the background until all morbid action has passed by, and they must not even then be used unless we are driven to it, for it is no uncommon thing for effused lymph to be absorbed under favourable circumstances. While I am not suggesting that instruments are never to be employed in the treatment of stricture, I venture to affirm that they have been, and still are, grossly abused; that an untold amount of mischief is perpetrated by their abuse; that the routine employment of instruments in the treatment of stricture is in the highest degree unscientific and improper; that, in a word, in the treatment of stricture, instruments of any kind should never be employed in any way except as a last resource." (Mr. W. S. Savory, p. 166.)

Urethrotome for Incising Narrow Strictures.—It is easy by the instruments hitherto used to incise a stricture which is not very narrow, but it is often these very cases which most require it. Mr. Berkeley Hill describes an instrument which is suitable for the purpose. The distance of the stricture from the meatus is first to be ascertained by accurate measurement. A guide bougie of fine whalebone, about one-fourth the size of a No. 1 catheter, is then to be introduced, and threaded upon this the urethrotome is to be passed. The incisor consists of a slender grooved sound of steel, with the ordinary curve, and provided with a tunneled end. In the groove runs this little rod having a blunt end fixed to it by a joint. When run along the groove of the sound, the jointed end and rod are quite concealed until they reach a point 6½ inches down the shaft. Here the groove becomes shallow and ceases; thus, the jointed end is thrown out and projects about one-tenth of an inch beyond the surface of the sound. It thus forms a little catch on the sound, which will travel along the urethra until it reaches the stricture, against which it will be caught. After the grooved sound has been passed along the guide, through the stricture, the little rod may be run down, and its end made to project into the urethra behind the stricture. By drawing forwards the sound thus armed, the posterior limit of the stricture is ascertained; the anterior limit is already known. The little rod is then withdrawn and replaced by another which carries, instead of a blunt point, a knife attached by a joint in the same way; this knife, concealed in the groove of the sound, travels down until it has reached the place where the groove grows shallow. This termination of the groove being already carried beyond the posterior limit of the stricture, the knife is thus made to project to a distance regulated by a screw at the handle against the floor of the urethra; and, if the sound be drawn forwards, the knife cuts its way until the whole stricture is divided. When the anterior limit of the stricture is reached, the knife is again brought within its groove, and the sound is withdrawn. The knife projects half an inch as a maximum, but, by adjusting the screw, can be set to any less distance. (Mr. Berkeley Hill, p. 175.)

Maisonneuve's Urethrotome in Stricture.—Maisonneuve's urethrotome is probably the best instrument possible for the purpose for which it was designed. It is remarkably efficient and easy to work, and adapted for the narrowest strictures. The sound is of steel, hollow, split along the upper surface, and the size of a No. 2 catheter. The knife, of which I have two sizes attached to a thin flexible arm of steel, which bends as it slides along the concavity of the sound, cuts in advancing and receding, but only along its slopes, not at the free apex, which is knobbed and very blunt, blunt enough to leave uninjured the healthy part of the urethra. The lash-like appendage is a filiform bougie, fitted with a female screw at one end for attachment to a male screw at the tip of the sound. The bougie is first passed, its size being that of an English No. ¼ or less; then the sound is screwed on to it, and pushed *after* it, the bougie coiling up in the bladder. The knife is then run down the sound and withdrawn, the sound and bougie being pulled out next. The operation is easy, rapid, and successful, and produces neither more pain nor more bleeding than does 'divulsion.' (Mr. Rushton Parker, p. 177.)

Maisonneuve's Bougie Conductrice.—Some little care must be taken in the use of this instrument, for the blade may injure the healthy part of the urethra, and a guard has been used to prevent this possibility. It is also not safe to pass the knife until you are certain the instrument is in the bladder—this you can only be by inserting in the sound a stilet which shall fill the slit, so that when it is withdrawn, urine will flow if the bougie has gone into the bladder. As for 'divulsion,' 'dilatation instantanée,' 'immediate treatment,' or any other synonym by which the process of tearing open a man's urethra may be concealed, it has fully justified the almost universal condemnation passed upon it by Parisian surgeons. It has acquired the unenviable distinction of being the most fatal operation known for stricture, and I am aware of thirty deaths following its use by different surgeons. (Mr. W. F. Teevan, p. 178.)

The "Sonde à Vis, portant un Conducteur."—This instrument is the invention of M. Phillips, of Paris, and consists of an elastic catheter, and a flexible bougie. The bougie tapers away at one end to a fine line, whilst its other extremity enlarges to about the size of No. 2, and is fitted with a screw, which works into a socket on the vesical end of the catheter. The ends of the bougie and catheter are screwed together like the bamboo rods used by the chimney-sweeps. The bougie is first passed into the bladder, and the catheter is screwed on to it, and then pushed slowly into the bladder, causing the whole of the bougie to enter that viscus, where its great flexibility allows it to coil up and remain quietly without the patient being aware of its presence till the catheter has effected its object of drawing off the urine. The entire instrument is then withdrawn, or the catheter can be unscrewed to leave the bougie *in situ* to serve as a guide for the next occasion, or to dilate the stricture. The instrument is especially useful in cases of tortuous urethra from enlarged prostate or tumours of the bladder. (Mr. W. F. Teevan, p. 179.)

Retention of Urine relieved by Ice in the Rectum.—M. Cazenave's plan is to plug the rectum with small pieces of ice. Mr. Teevan relates a case in which he adopted this procedure. In exactly twenty minutes after the last fragment had been inserted into the bowel the patient began to pass water guttatim, and in the course of half an hour contrived to empty his bladder. (Mr. W. F. Teevan, p. 173.)

To Relieve the Bladder in bad Cases of Stricture.—Pass an exceedingly fine whalebone bougie through the stricture to the bladder, and upon this thread a fine silver catheter, having its end tunnelled with an eye. (Mr. Berkeley Hill, p. 175.)

VENEREAL AFFECTIONS.

GONORRHŒA.—*Bromide of Potassium.*—Bromide of potassium augments the secretion of urine when administered internally, and renders it less irritating. This salt seems to have the power of diminishing the irritability of mucous membranes, not only when taken internally, but also when applied topically, for, according to Dr. Ringer, some writers aver "that merely brushing the pharynx and soft palate with a solution is sufficient to quell the irritability of these parts, so as to enable the laryngoscopic examination to be made with ease." Assuming such to be the case, it may naturally be inferred that a similar effect would be produced by its topical application to the inflamed urethra. For use as an injection, two drachms of bromide with half an ounce of glycerine should be dissolved in eight ounces of water, and one syringeful used every four hours. When the disease has assumed the form of gleet, the bromide may still, with advantage, be given internally, but it should be combined with tincture of sesquichloride of iron. (Dr. [illegible] Montreal, p. [illegible])

MERCURY IN SYPHILIS.—In the giving of mercury, blue-pill has had too [illegible] As a preparation it is [illegible], and it tends when long given to [illegible] Syphilis is really (as shown by Mr. Hutchinson) a kind of [illegible] however much longer than in ordinary fevers—sometimes even a [illegible] If, therefore, we are to benefit our patients, we must be content to give mercury in such a way as will least interfere with nutrition [illegible] in small doses and over a lengthened period of time. In this way perhaps the liquor hydrargyri perchloridi in drachm doses, is one of the best modes of exhibition, for this contains only a very small quantity of mercury, and, given in a vegetable infusion, is often of the utmost benefit. But in many cases there can be no doubt whatever that inunction [illegible] better than any other mode of giving the remedy; and a paraphrase of the old rule—viz., the stomach for food, and the skin for mercury—might be held to be the best mode of solving the difficulty. (Editor of Medical Times and Gazette, p. 187.)

Although the inunction plan still enjoys undisputed pre-eminence at Aix-la-Chapelle (which may be considered as a sort of better-class Lock Hospital for the North of Europe), there is no reason whatever why it should not be given with as great advantage by the mouth. The inunction plan is dirty and inconvenient. The main rule in giving mercury in syphilis is to use small doses for a lengthened period. In order to secure the antidotal efficacy of mercury against syphilis, it is desirable to introduce a considerable quantity into the system, and to protract its use over a very long time. Ptyalism and other evidences of the physiological action of mercury, so far from being beneficial, are, if possible, to be carefully avoided, since they prevent the sufficiently prolonged use of the remedy. It is impossible to begin the administration of mercury too soon, and it should be resorted to without loss of time in all cases in which a chancre shows a tendency to indurate. It has not yet been proved that there are any special forms of syphilitic disease in which mercury ought to be avoided, although, as a general rule, it is acknowledged that it must be used with more caution in all forms which are attended by ulceration than in others. Iodide of potassium possesses little or no efficacy against either the primary or secondary forms of syphilis. The efficacy of mercury is often most signally proved in cases which have utterly resisted the action of iodide of potassium. It does not much matter whether mercury is given by the mouth, by inunction, or by the vapour-bath, provided that, whichever mode be selected, care be taken to avoid salivation, purging, &c. The doses usually resorted to for internal administration are for the most part too large, and thus often necessitate a premature discontinuance of the remedy. (Mr. J. Hutchinson, p. 186.)

Iodide of Potassium in Syphilis.—Syphilis is a disease more or less approximating to the characters of an eruptive fever, and tending like these to terminate of its own accord, with the evolution of certain symptoms. Mercury directly interferes with the evolution of these symptoms, cuts the various stages short, and acts as a direct antidote to the specific poison which gives rise to the symptoms in question. In the tertiary stage of syphilis, the value of iodide of potassium is just as unquestionable as is the value of mercury in the earlier phases of the malady. It is a very important question for us to settle—if settle it we can—What is the value of mercury in this after-stage of syphilis? Suppose we see a patient with well-marked rupia, with periostitic pains, and other signs of tertiary syphilis, who, nevertheless, has not taken mercury, what should we do? The first thing to note is that such symptoms, though usually sequelæ, may occur in the active stage of syphilis; and as long as syphilis is active, mercury will be of use; but as soon as the active symptoms have passed away, and the so-called tertiary stage begins, we must abandon the attempt to cure by mercury—we must give iodide of potassium. (Editor of Medical Times and Gazette, p. 189.)

Carbonate of ammonia greatly increases the therapeutic action of iodide of potassium. Five grains of iodide of potassium, combined with three grains of carbonate of ammonia, are equal to eight grains of the potassium salt administered in the ordinary way. The iodide may in the same combination be given in internal aneurism, and in the waxy liver of syphilis. (Dr. J. P. M'Sweeny, p. 190.)

AMPUTATIONS, DISLOCATIONS, FRACTURES, AND DISEASES OF THE BONES, JOINTS, ETC.

AMPUTATIONS.—*Safety of Torsion.*—Mr. Bryant reports that he has had only one case of secondary hemorrhage from a stump since he began the practice of torsion of arteries in January, 1868. This bleeding took place from the interosseous artery, to which he applied a ligature of carbolized catgut. There have now been at Guy's Hospital upwards of 200 cases of amputation of the thigh, leg, arm, and forearm, in which all the arteries have been twisted (110 of these having been of the femoral artery), and no case of secondary hemorrhage. (Mr. T. Bryant, p. 144.)

DISLOCATION OF THE HIP.—*Reduction by Manipulation.*—In a case of dislocation on to the dorsum ilii of seventeen days' standing, chloroform having been fully administered, I flexed the leg on the thigh, and the thigh on the abdomen, abducted the limb fully, and rotated outwards. During this last movement, I felt the tearing through of adhesions, and the head of the bone at once returned to the acetabulum. In a case of recent dislocation of the hip, flexion, abduction, and rotation inwards, resulted in transferring the head of the bone to the sacro-sciatic notch, whence it could easily be returned into the thyroid foramen. Having again removed it to the sciatic notch, I put my foot in perineum and made extension aid my movements, when it slipped at once into the acetabulum. It seems as if too great flexion was the cause of the bone slipping from the one position to the other without entering the acetabulum. (Dr. Hector C. Cameron, p. 83.)

BLOODLESS OPERATIONS.—The value of Esmarch's bandages for enabling the surgeon to perform operations with the parts unobscured by blood, is very great. In a case of acute caries of the middle third of the femur, chloroform having been administered and the heel well raised, I bandaged tightly, with india-rubber bandages two inches wide, from the roots of the toes, over the heel, to a point two inches above the intended seat of operation. While an assistant held his finger on the uppermost turn of bandage, the limb immediately above it was very firmly encircled with several turns of india-rubber pipe, about the thickness of the index-finger. The ends of the pipe, which was two yards long, having been firmly tied together, the elastic bandage was uncoiled, leaving the limb shrunken and waxy pale. A three-inch incision in the middle line down to the femur, free use of the gouge and chisel in the removal of a quantity of carious bone, an incision into a boggy spot inside the knee, and the passage of a drainage-tube through the opening thus made from the incision over the middle third of the bone, were the successsive steps of the absolutely bloodless operation. (Mr. S. Gamgee, p. 103.)

The bloodless method of operating by means of the elastic band is a great convenience to the operator, as it makes him independent of sponges during the process, but it is questionable whether the wounds heal so quickly. It is necessary not to be in too great a hurry to close the wound after this method, but to wait a few minutes till the engorged condition of the surface and edges of the wound have subsided, before applying the sutures, a matter which becomes of more importance after the larger operations, such as excision of the knee. (Mr. Cowell, p. 113.)

Mr. Heath has for a long time practised a method by which the limbs may be deprived of blood. The limb being raised, the house-surgeon draws his hand gently along the surface, so as to force back the blood to the trunk. A tourniquet is next applied, and a card placed beneath the screw of the instrument. After the operation, the tourniquet is gradually loosened, when the blood returns slowly to the limb. One great advantage to this plan is, that every surgeon already possesses all that is requisite for its accomplishment. (Mr. C. Heath, p. 106.)

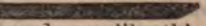
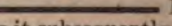
Bandage the limb tightly from the toes to the upper third of the thigh, with elastic bandage about four inches in width, after which, work a stout elastic cord spirally over all, until it reaches the last turn of the bandage, which must then be removed. (Mr. J. K. Smith, p. 97.)

The following is the mode of preparing a limb for a bloodless operation: —A strong elastic bandage is applied in the ordinary manner to the extremity, firmly and slowly: a piece of india-rubber tubing of the diameter of the thumb is wound very tightly round the limb immediately above the margin of the bandage, which is then removed. The preparations are complete, and the limb is perfectly bloodless until the tubing is removed. The facility of operating under these circumstances can hardly be realised. The structures have a semi-transparency resembling that resulting from preservation in turpentine or glycerine, and as may be expected, they are picked out and distinguished with remarkable facility. The operations upon which Esmarch's method confers the greatest advantage are those in which much blood is ordinarily lost, or those performed under circumstances which prevent the operator from providing efficient aid. In cases where collections of septic matter exist, Esmarch warns us against the danger of favouring by pressure its entrance into the circulation. The absence of bleeding points during the operation may cause us to neglect to secure some vessels which will subsequently be troublesome. In military and provincial practice, where, as already remarked, the surgeon must amputate without sufficient skilled assistants, he is free to select between the immediate danger of hemorrhage and the remote disadvantage of sloughing of the flaps. (Mr. J. E. Kelly, p. 104.)

The absence of hemorrhage is of peculiar value in operations upon bone, as the surgeon can see exactly what he is doing. Some arguments might be reasonably brought against the uniform adoption of this method; that though there were many cases in which patients could not afford to lose a drop of blood, yet there were others—for instance, in those of amputation in the thigh—where the sudden addition of so large a quantity of blood as that contained in the limb to the rest of the circulation might be more injurious than the loss of some part of it with the limb when removed. In operations for necrosis especially, this method greatly facilitated the manipulations of the surgeon. Previously the hemorrhage from the vascular structures cut through, so obscured the part that the sense of touch was the only guide to the operator; but now the surgeon could see the sequestrum, ascertain its extent, and easily distinguish it from its new bony casing. (Sir W. Fergusson, p. 82.)

Bloodless Operations by means of the Galvanic Cautery.—The capillary oozing of blood from the general surface of a wound, when the operation by which the wound was made has been performed after the bloodless method, may have some effect in preventing primary union. In aged subjects and in others in whom the vessels are diseased, sloughing or want of action is not unlikely to result, especially if the carbolic-acid spray has been employed at the same time. By means of the galvanic cautery we possess the power of performing many operations without loss of blood; and in comparison with this method all other so-called bloodless methods are delusions. But to secure this result much care and discretion are called for. The battery must be in good working order, and the instruments complete and whole: no make-shift will suffice in any way. If the battery does not work well, the required heat will not be maintained; if the instruments are imperfect, some break in the galvanic current may take place, and thus failure must ensue. As a matter of precaution, before operating the surgeon should, therefore, test the battery and instruments that are to be employed, and in this way save trouble and prevent disappointment or, possibly, failure. There are no operations of importance that the surgeon has to perform which have been more benefited and simplified by the introduction of the galvanic cautery than those upon the tongue; for there are none in which, without its use, hemorrhage is more troublesome or dangerous, and there are none, with its use, which more satisfactorily illustrate its bloodless character. Indeed, before the introduction of the galvanic cautery or écraseur, operations on the tongue were very rarely performed. For the details of the plan of operating for the removal of a cancerous nodule of the tongue, or of a cancerous tongue wholly or in part, we must refer the reader to the article, merely remarking, that what was formerly a very serious operation has, by this plan, become a very simple one. (Mr. T. Bryant, p. 98.)

THE ELASTIC LIGATURE.—The division of parts by an elastic ligature is effected by the continuous pressure of the cord, which compresses the vessels and lymphatics until the vessels are plugged, and the access of nutritive material is completely prevented. Little or no pain and no febrile symptoms are caused. The mode of applying the ligature varies according to the peculiarities of the task to be performed. In nævus a single circular ligature is usually sufficient, and in two cases operated upon it fell off in eight days. In fistula ani or sinuses about the rectum, the elastic ligature has especial advantages where the inner opening of the fistula lies high up or the sinus extends far. In the case of sinus, an inner opening (into the rectum) is first made by means of a trocar. The trocar being withdrawn, the elastic thread is introduced through the cannula and drawn out through the rectum. This proceeding is rendered more easy by first introducing a metallic thread into the rectum through the cannula, seizing it with the finger or forceps, drawing it through the anus, and removing the cannula. The outer end of the wire is now fastened to the small elastic tubing by means of a waxed thread. In this way the elastic cord is very easily drawn through, if the index finger of the left hand can be passed up the rectum as far as the opening. Both ends of the ligature are drawn upon, and tied rather tight. The bridge of intestine with the sphincter is generally cut through in three or four days. The elastic ligature may be employed for the removal of tumours when they are pedunculated, or when they can be isolated, or when it is not possible or necessary to save the skin. The tumour may, if large, be tied in two halves. For this purpose a trocar should be passed beneath the tumour, and the trocar being withdrawn, two small pieces of india-rubber tubing should be introduced through the cannula. The elastic ligature may be applied to arteries. In one case the popliteal artery was tied. The ligature was thrown off on the sixteenth day. (Prof. Dittel, Vienna, p. 108.)

Removal of Tumours by the Elastic Ligature.—Solid Elastic Cord.—After experience with a fine india-rubber tube Sir Henry Thompson has come to the conclusion that solid india-rubber cord is much preferable, as it may be drawn at least twice as tight, and therefore cuts more rapidly, especially as it is also much finer. The principal objection to the plan is the smell which necessarily arises from the sphacelated portion. This may be much controlled by irrigation with carbolic acid and water. No doubt a breast is a rather severe test for the ligature, although I do not hesitate to anticipate a very much better result with a more powerful cord. For the removal of the testicle, and for division of fistula in ano, I think it will be found admirable. The cord itself ought to be more accurately described. Its size before use is the following:  When applied it should be strained until it is a mere thread—say like this:  It would be very easy to devise a simple apparatus to tighten it subsequently, but it is so easy to accomplish this by pulling it out if loose, and tying a bit of fine ligature round the portion so drawn out, that it seems unnecessary to employ any other means. But with the new cord it is very probable that no such readjustment will be required. (Sir H. Thompson, p. 111).

A woman, suffering from a large vascular tumour at the upper part of the vulval opening, presented a good opportunity for the employment of the elastic ligature, whereby the usual dangerous hemorrhage might be avoided. I ligatured the whole mass with a solid India-rubber cord two millimetres in thickness. I used this solid cord because the result of some previous experiments had satisfied me of the inadequacy of drainage-tube or any other variety of hollow caoutchouc tubing. The solid cord which I used is to be obtained at most India-rubber shops, the only difficulty being to procure it of sufficient thinness; when thicker than I have mentioned, it is unmanageable, especially for small growths. On the third day it became evident that the ligature was no longer acting, and on examination it was found to be quite loose. You may remember the simple way in which this difficulty was combated. A slender rod of ivory, one millimetre in thickness, was passed through the ligature circle and twisted round a few times, in the manner in which a ready tourniquet is made from a handkerchief tied round a limb with a stick twisted through it. These ivory rods can be had of various thicknesses in the shanks of ivory crochet-needles. Ivory is, I think, the best material, on account of its strength, lightness, smoothness, and rigidity; and by this simple means ligatural pressure can be kept to the very last to any degree of tightness required. (Dr. F. J. B. Quinlan, p. 149).

Excision of the Ankle-Joint.—This operation is commonly performed by dislocating the bones of the leg inward. The articulating surface of the tibia, 'however, is removed with much less disturbance to the surrounding parts by dislocating the parts outwards. The plan advocated, therefore, is to remove the internal malleolus first, and then the tibia and fibula may be dislocated outward through the external wound with great facility, and without interfering with any important structures. (Mr. Henry Lee, p. 92.)

Dr. P. H. Watson's Plaster-Splint after Excision of the Knee-Joint.—This splint is the best which has been hitherto invented for the after-treatment of excision of the knee-joint. It consists essentially of two parts: first, a suspension rod made of iron; second, a modelled Gooch splint, long enough to extend from the tuberosity of the ischium to beyond the heel. In application, the limb is first laid and carefully adjusted upon the posterior splint, which should preliminarily be padded with lint, and covered with gutta-percha tissue, or hot paraffin, in the situation which corresponds to the site of the operation. The iron rod is then placed in front, and folded lint laid between it and the limb at the groin (where the rod terminates above) at the upper part of the tibia, and at the bend of the ankle. The whole is then rendered immovable by means either of plaster-of-Paris applied by the hand, of a consistence like thick cream, or of paraffin, which, having been rendered temporarily liquid by heat, is applied by a large paint-brush. When the application has solidified, the patient may then be removed to bed, and the limb suspended from the running pulley of a Salter's swinging cradle, or from the roof bar of the common iron wire cradle, employed to support the weight of the bed clothes. I do not regard it as a matter of indifference whether plaster-of-Paris or paraffin is used. Each has its advantages. The plaster-of-Paris is firmer and not liable to be affected by heat like paraffin, but it has the disadvantage of permitting soakage of discharge, it takes longer to consolidate, and when consolidated is less easily chipped through, by means of a bandage-shears, than the paraffin apparatus. (Mr. H. J. Tyrrell, p. 85.)

FRACTURE OF THE CLAVICLE.—M. Broca relates a case of oblique fracture of the clavicle, in which it seemed impossible to prevent the fragments riding over one another. He placed the arm in a semiflexed position behind the back, where it was retained by a bandage for eighteen days, with the effect of completely adjusting the fractured surfaces and producing an excellent cure. For a few days longer a sling was used. The patient, a man of considerable nerve, complained of the inconvenience and pain of the method for only the first twenty-four hours. (M. Broca, p. 95.)

FRACTURES OF THE PATELLA.—*New Apparatus for.*—The apparatus which I use is a hollow wooden splint, 4¾ inches wide, and extending from above the middle of the thigh to the sole of the foot, at which point a footboard is attached by means of a hinge; this splint, having two transverse bars, is fitted into a long box-splint, the sides of which are forty inches long and six inches in depth, in which it travels horizontally. That portion of the splint on which the limb rests can be elevated or depressed as required by means of perpendicular slots cut through the sides of the box-splint. Thus, the splint can be adjusted to suit a long or short leg, the limb can be elevated or lowered at pleasure, and the foot placed at any angle. This hollow splint is fixed in position by means of thumb-screws which fit into the transverse bars before mentioned; two semilunar pieces of metal, softly padded, are fixed one above the other below the fractured patella by means of leather straps which pass round the limb, the leg is secured to the splint by means of two broad web straps, one round the calf the other at the ankle; the foot can be bandaged to the foot board; a roller with each adjustment is fitted in the box-splint below the footboard. From this roller start four cords, which passing through brass sheaves, are attached, two to the upper and two to the lower metal pads by means of chains and light hooks. The roller is turned by means of a key, and acting on the cords, causes the metal pads simultaneously to approach each other, thereby bringing the fragments into apposition; the rack is covered by a brass box, which can be locked, so that the adjustment of the splint cannot be interfered with by the patient. Not only is this splint suitable for the fracture for which I have introduced it, but I believe also for fractures of the thigh or fractures of the leg. (Dr. W. J. Wheeler, p. 94.)

Nervous Mimicry of Joint Diseases.—The absence of swelling makes it very unlikely that a joint is really diseased; so does the presence of only a trivial swelling when the nervous and muscular signs of disease are acute or of long standing; and when swelling exists it must not be counted as adding to the probability of real disease, unless it persists. The sign most to be relied on for diagnosis between real and nervous disease of joints is the temperature. It is so important to estimate it accurately that I cannot too strongly urge you to be always studying it. You should feel with a broad surface of your hand every joint very watchfully, comparing each that is supposed to be diseased with its fellow supposed or known to be healthy, till you learn, as you certainly may, to detect even a small difference of temperature in even a small part of a joint. A joint that feels all over cold, or cool, or not above its natural temperature, is not an inflamed joint: whatever may be the other signs of inflammation in it, it is not inflamed; you may rely on this. Sometimes a joint which feels quite cold, and is free from swelling, is reported as getting very hot every evening, and as being subject to painful heats. In any such case you may decide that the joint is not inflamed. The certainty of the diagnosis based on coldness is, if possible, increased by coincident duskiness of the skin—by its having the dull purplish tint which is commonly called blue or a dull pink. Such colours may be seen at joints long inflamed, but in these cases they are associated with overheat; when they are associated with coldness, they are characteristic of anything rather than inflammation. (Sir J. Paget, p. 80.)

Removal of the Breast for Cancer.—In any case of cancer, the treatment must be very thorough; for any treatment, whether by the knife or caustic, which does not remove the whole of the disease, only does harm by stimulating the part left to increased action. In the case of the breast, it is, I believe, impossible for any surgeon to be sure that he has removed every germ of disease unless he extirpates the entire breast. I recommend you to make a very sparing use of the knife, and to trust very much to your fingers. You will find no difficulty, unless the patient is unusually thin, in stripping up the skin and its subjacent fat from the breast with your finger, aided by an occasional touch of the knife; and this plan has the advantage of securing you against slicing small portions of the breast or making "button-holes" in the skin. You can also better appreciate when you have reached the thin border of the breast, and so avoid leaving some of it behind, as is too often done. Forcibly raising the border of the breast, the surgeon at once ascertains whether the pectoral muscle is involved or not, and if it is, he should, in my opinion, not scruple to cut into it freely, so as to remove, if necessary, a considerable portion of the muscle; and here again hemorrhage will be saved if the fibres are torn as much as possible. In removing the axillary glands, it is especially desirable to be chary of the knife, both on account of the hemorrhage and because the finger can hook them out so much more satisfactorily than any instrument. After completing the removal of a breast, I am in the habit of mopping out the wound very freely with the solution of chloride of zinc recommended by Mr. De Morgan, both on account of its antiseptic qualities and its possible effect upon any cancer-germs left in the tissues. I used to employ it of the full strength (gr. xl. ad ℥ j.), but I fancied that it now and then caused the skin to slough, and I have therefore reduced it to one-half the strength, and I think, with advantage. In closing the wound, it is most important to leave a sufficient and dependent opening for drainage, and this is most conveniently placed near the axilla; and for a dressing I find nothing so satisfactory as oakum or carbolised tow, for the supply of real oakum has failed. This soaks up the discharge, and allows it to permeate, and hence keeps the wound dry, which I take to be the great point in favour of rapid healing and limited suppuration. It is convenient to place a piece of gauze between the skin and the oakum, so as to prevent the latter adhering, and to cover the oakum with a folded towel to absorb the moisture, which is abundant at first. (Mr. C. Heath, p. 40.)

Resin-Cloth in the Treatment of Wounds.—The carbolic gauze used by Professor Lister is most efficient for the purpose of treating wounds antiseptically. A much cheaper and equally efficient substitute may be made by saturating very thin calico in the following solution, after which it is to be pressed and dried. The exact composition of the solution is as follows:—Carbolic acid crystals, melted, two fluid-ounces; castor-oil, two fluid-ounces; purified resin, sixteen ounces by weight; methylated spirit, forty fluid-ounces; mix. To dissolve these ingredients easily, we must add them together in a certain order. To the resin, liquefied by heat and removed from the fire, add one-third part of the spirit; when these are well mixed, put in another third of the spirit, in which the oil has been previously dissolved; and, lastly, the acid in the remaining portion of the spirit must be slowly added to complete the mixture. The whole must be agitated until all the constituents are thoroughly incorporated, and afterwards passed through a muslin filter to get rid of any extraneous matters. If this plan be not adopted, the resin will concrete into a mass at the bottom of the vessel, and it will be extremely difficult afterwards to get it perfectly mixed. When thus prepared, the solution is of a dark colour, clear and free from any deposit; and it can be kept unchanged in a well corked bottle for a long time. It is, in fact, a kind of thick varnish. To make the resin-cloth, as I term it, for the sake of distinguishing it from the cere-cloth dressing for wounds, which I described in a paper read at the Leeds meeting of the British Medical Association in 1869, it is needful to select a very thin, cheap, porous calico, or calico-muslin, known in the trade as "mulls," which costs at wholesale price about four shillings per piece of twenty yards. This, divided into strips, each about nine inches wide and six yards long, is reduced to a convenient form for general use. The castor-oil does away with the brittleness and adhesiveness of the resin. (Mr. E. Lund, p. 126.)

Self-Adjusting Sling Arm-Splint.—Mr. Callender's sling splint for patients confined to bed with diseases or injuries of the arm is very useful. The apparatus consists of a wrought-iron support, a sling, and a counterweight. The support is made of an iron bar bent at a right angle, the vertical part of which is round and works in a socket fixed by hooks or screws to the bed-head, so that it can be moved to the right or left readily by the patient when necessary. At the angle of the support is a small iron pulley over which the cord of the sling runs, and on the horizontal arm is another pulley, which can be moved backwards or forwards at pleasure. The sling is made of strong canvas, with four holes on each side, and supported by four cords with hooks, which can be so placed in the holes of the sling as to give the arm or hand any angle or position. These sling cords are attached to the single cord which passes over the pulleys, and has at its other extremity a bag containing shot, so that the weight may be increased or diminished as occasion requires. (Mr. H. H. Smith, p. 93.)

Silica Bandage.—Silica bandages are now in use in the Paris hospitals in place of gypsum and starch, and an exceedingly firm bandage results. The silicates of potash and soda, dissolved in an excess of caustic alkali, are now prepared in large quantity in the manufacture of soap, and can be easily obtained at a very moderate cost. Messrs. Hopkins and Williams, of 16, Cross Street, Hatton Garden, have supplied it in large quantities at 4d. a pound, and in small quantities at 6d. a pound, and it can be obtained, though inferior in character, at 2d. and 3d.,—so that it is not an expensive material. In fact, it is about half or a quarter the price of the thick gum solution used for similar bandages, and being firmer, does not require to be used in much more than half the quantity. The plan which I have adopted has been to encase the joint or fractured limb in cotton wool, lint, or a thin flannel roller; then over this to apply a common bandage, dry; over this to paint the silica by means of a brush or sponge; and to repeat the bandage and silica so that two or three layers of each exist. As soon as the last layer is dry, another coating of silicate is to be put on, so as to give an even surface. The limb is to be left exposed to the air for about half an hour, but there is no fear of any of the silicate coming off after the first few minutes, and after half an hour or less (varying with the temperature) the bandage is firm enough to prevent movement. However, the bandage continues to harden for about two or three days, at the end of which time it should be quite firm; but it is usually firm enough in a few hours to insure immobility of a limb. We have therefore in this silicate bandage a means for providing immobility of joints and fractures which far surpasses in convenience, lightness, cleanliness, firmness, and cheapness any of the means hitherto made use of. If a limb be covered with cotton-wool, lint, wool, a worsted stocking, or any other soft protecting material, a surgeon may make use of strips of linen, bandage, or paper saturated with silicate of soda mixed or not with a salt of lime, such as chalk, whitening, or plaster of Paris, and procure a rapidly setting and extremely firm splint, the expense of which is reduced to a minimum. (Mr. W. W. Wagstaffe, p. 128.)

AFFECTIONS OF THE SKIN, ETC.

CARBUNCLE.—*Local Treatment.*—The use of carbolic acid so as, as it were, to saturate a carbuncle with it, whilst yet in its early stage, enables us in many cases to completely arrest its progress, and in all to modify it favourably. For this purpose, pass into the holes some threads of lint soaked in a strong solution of carbolic acid in oil (one part to four), and cover the hole with a piece of lint wet with the same, and let the application be renewed after a few hours. The carbolised lint must be carefully and scrupulously thrust to the very bottom of the small holes, a probe being used for this purpose. (Dr. P. Eade, p. 184.)

LUPUS, EPITHELIAL CANCER, AND NÆVUS.—*The Galvano-Caustic.*—One application of the galvano-caustic is in many cases sufficient to set up a new action in the part, and heal a lupus, destroy a cancer, or cure a nævus. It possesses advantages such as are not equalled by any other means the surgeon has at his command. It is applied with facility, certainty, and success: and it has rendered the treatment of these hitherto troublesome affections comparatively a simple matter. In some cases of nævi it is not necessary to destroy the whole. I have had several in which one of the eyelids was involved down to its ciliary border. In these I have been content with destroying half the growth, leaving the ciliary half alone; and I have been pleased to find that the ciliary portion of the disease subsequently disappeared, the redness gradually going, and leaving the skin white and pliable. In extensive nævi, with these facts before us, it is consequently wise to deal with them piecemeal—to destroy a spot the size of a shilling at different parts of the growth; to cauterise the surface of the nævus when the skin is involved, and the deeper part by perforating it when the cellular tissue is affected; and in some cases it is well to make a clean sweep of the heated wire through the base of the growth;—the object under all these circumstances being the same—to destroy the vascular tissue, and excite sufficient inflammatory action in its deeper parts to cause what remains to wither. The larger and more extensive nævi may be thus treated with every probability of acquiring a successful result. (Mr. T. Bryant, p. 114.)

ONYCHIA MALIGNA.—*Nitrate of Lead.*—In onychia maligna the last phalanx is usually double its natural size, livid red in colour, and ulcerated along the sides and upper edge of the nail. The pain attending the disease is most intolerable, and the treatment of it hitherto, most unsatisfactory; even removal of the nail does not cure it. The loose portion of the nail should be removed by scissors, and powdered nitrate of lead freely applied to every part of the diseased surface every two days; the hard crust which forms is to be removed, and the powder re-applied. The effects are most satisfactory, all pain ceasing from one to three days after the first application, and a complete cure resulting (Mr. Wm. MacCormac, p. 251.)

Dust the powdered nitrate of lead upon the onychia every night and morning, and at the same time administer cod liver oil and *steel wine.* (Mr. Howard Marsh, British Medical Journal, Jan. 17, p. 79.)

SUSPECTED RINGWORM.—The accurate diagnosis of suspected ringworm is very necessary, especially in the case of schools. There may be merely a "scurfy spot" upon which the hair appears to be growing fairly. The only real diagnostic test is furnished by means of the microscope, for if a few of the scales are scraped off and examined, they will always have little bits of diseased hair entangled in them where ring-worm is present, and which diseased hairs are not perhaps visible to the naked eye. (Dr. Tilbury Fox, p. 180.)

SYCOSIS.—Sycosis is the common inflammation of the hair follicles of the beard and whiskers. Epilation is a fit procedure only at a certain stage of the disease, when the skin is much inflamed, the follicles freely suppurating, and the hairs being thereby loosened in them. In the early stage of the case, a saline aperient, or aperient tonic, such as sulphate of magnesia with sulphate of iron, is advisable; at the same time, hot fomentations and simple soothing applications, which exclude the air. When free suppuration has occurred, mild astringents such as zinc lotion and ointment should be applied. In the chronic stage, a course of Donovan's solution is to be given together with locally hot fomentations, and the application of a weak nitrate of mercury ointment morning and night. (Dr. Tilbury Fox, p. 182.)

AFFECTIONS OF THE EYE.

A NEW PHOTO-PERIMETER.—Mr. Jeaffreson describes an instrument by means of which the limits of the field of vision of a patient can with rapidity and accuracy be transferred to a chart and kept for future reference. It will also test the field of vision for colours, and can be made to indicate the presence of astigmatism and show the meridian in which this exists if present. We must refer to the article itself for a description and diagrams of the instrument. (Mr. C. S. Jeaffreson, p. 194.)

AMAUROSIS.—*Hypodermic Injection of Strychnia.*—Dr. Taylor, of Nottingham, relates a case in which the patient had been nearly blind for six months, without any constitutional cause or cerebral defect to account for the loss of vision. On examination the disease was found to be simple white atrophy of the optic discs in both eyes, and the patient was at once injected under the skin of the brow, with the twelfth of a grain of nitrate of strychnia. His sight improved so rapidly, that he was enabled to resume his occupation as a moulder, at the end of about three weeks. The injection was repeated occasionally, but at what intervals of time is not stated. In another case, however, the hypodermic injections were used twice daily for five weeks. This mode of treatment does not always give such good results, but, as we cannot say beforehand whether it will succeed or not, and as it cannot be productive of harm, it should always be tried in cases of this nature. (Dr. C. Bell Taylor, p. 197.)

PURULENT OPHTHALMIA.—The treatment which has met with most success at the Royal London Ophthalmic Hospital is the frequent use of alum-water, generally six grains to the ounce of distilled water. Before doing this each time it is well to syringe out the conjunctival pouch under the upper and lower eyelid with tepid water; for this a common pewter squirt will do. At first, every second hour will not be too often, and then, as the ophthalmia yields, the intervals may be increased, until in a couple of days, six to four applications in the twenty-four hours will suffice. If the surgeon prefers the nitrate of silver, the eyelid should be everted and cleansed in the same way, then brushed with a solution of the salt—ten grains to the ounce. One such application a day is enough, and in the intervals a little alum water, pure or mixed with ext. belladonnæ, may be dropped three or four times into the eyes. (Mr. J. W. Hulke, p. 198.)

MIDWIFERY, ETC.

Amenorrhœa.—*Catelectrotonus of the Ovaries.*—Where the general health is as a rule excellent, the usual medicinal and surgical treatment of amenorrhœa often fails to rouse the dormant function of the sexual organs; and the various forms of electricity then step in as the right thing in the right place. The most effective mode of applying electricity I believe to be the induction of catelectrotonus of the ovaries. This is effected by placing the negative electrode of the constant battery alternately to the right and left ovarian region, putting the anode alternately to the lumbar spine and to the os uteri by means of an insulated sound. A current of from fifty to sixty cells of Daniell's battery was used, and the action of it kept up for fifteen minutes at a time. This treatment should be repeated periodically, just before the time when the flow ought to appear. The induction of catelectrotonus of the ovaries is a powerful remedy, by means of which, if perseveringly employed, the function of the ovaries may be restored even after having been in abeyance for years. (Dr. J. Althaus, p. 235.)

Chloral as an Anæsthetic during Labour.—The more experience I have had of the use of chloroform in labour, the less do I like it, and this, not because it does too little, but on account of its tendency to do more than we wish. In certain cases, when given with judgment, only during the pains, and not until these have become strong and forcing, it answers admirably, soothing the patient's suffering without retarding her labour or producing complete anæsthesia; in others, it has an unquestionable tendency to diminish the force and frequency of the uterine contractions. I know not what may have been the experience of others, but my own certainly is that in a large number of cases it has a very marked effect in diminishing the strength of the pains, and thereby very materially lengthening the continuance of the labour. Over and over again, when the administration of chloroform has been commenced, I have observed the character of the pains completely to alter, and again recover their former efficiency as soon as the inhalation was suspended. Besides this, I have no doubt that a very continuous use of chloroform during labour has a marked effect in predisposing to post-partum hemorrhage, inasmuch as the tendency to undue relaxation of the uterine fibres continues for a time after the birth of the child. While, in my judgment, chloroform is apt to be too freely and incautiously used, the administration of chloral as a means of lessening the pains of labour is, I think, by no means as yet appreciated at its proper value. It has this immense advantage over chloroform, that it does not seem to diminish the strength and intensity of the pains, while it very markedly diminishes their painfulness. It has also another great recommendation, that it is chiefly applicable at a period when we would not think of administering chloroform—towards the termination of the first stage of labour, before the complete dilatation of the os, and when the sharp grinding pains perhaps produce more suffering and are less easily borne than the more forcing pains of a later stage. Hours and hours of really intense agony often elapse, until the patient is wearied and exhausted by her fruitless sufferings. In cases such as these, a common and very useful practice has been to administer a considerable opiate, so as to produce some hours of refreshing sleep, after which we expect the labour to recommence with fresh vigour and effect. The disadvantage of this plan, however, is that during the action of the remedy the labour is suspended, and much time is thus lost. If, however, chloral is administered instead of the opiate ordinarily employed, the probabilities are that the same refreshing rest will be obtained without any suspension of the pains or protraction of the labour. The character of the uterine contractions will be observed to alter; they will become steady and useful, but they are not suspended. Another condition frequently associated with the former is rigidity and spasm of the cervix. Very generally in this class of cases the cervix is thin and rigid, with a sharp edge. Soon after the chloral has taken effect the tissues seem to relax, and I have not unfrequently observed a thin os, which had remained unaltered in character for many hours, dilate rapidly under the influence of the remedy, far more so than when chloroform is inhaled for this indication. The way I give the drug is as follows:—I order a six-ounce mixture, containing a drachm and a half of the hydrate of chloral. When the pains are becoming severe, and I deem it advisable to employ the anæsthetic, which is generally not until the first stage of labour is approaching completion, I give one-sixth part of the mixture,—*i.e.*, fifteen grains of chloral. This I repeat in about twenty minutes; and usually after the second dose enough has been taken to bring the patient sufficiently under the influence of the remedy. Its farther administration must now be regulated by its effects. (Dr. W. S. Playfair, p. 201.)

Chloral given during the first stage of labour has the effect of promoting the dilatation of the os, and at the same time it much deadens the acuteness of the suffering. A case is related in which three doses of thirty grains each were given at intervals of half an hour with the effect of producing a rapid and almost painless labour, the patient sleeping between each pain. (Mr. W. Berry, p. 203.)

HEMORRHAGE FROM ABORTION.—*Hypodermic Injection of Ergotin.*—Ergotin injected subcutaneously in two cases of severe hemorrhage occurring with abortion in the third month of pregnancy had an excellent hæmostatic effect. In each case that I adopted this procedure, hemorrhage had occurred of such magnitude as to completely prostrate the patient and imminently imperil life. Two injections in both instances were employed. In the first case, a grain of Bonjeau's ergotin in ten minims of distilled water twice, at an interval of seven hours prior to the expulsion of the ovum, each exhibition appearing completely to control the hemorrhage; and during the intermediary period there was no hemorrhage whatever. In the second, almost total collapse had accrued; hemorrhage had existed eighteen hours; the vagina had been carefully and most completely plugged; one ovum had come away and yet the hemorrhage continued. The pulse was imperceptible in both wrists; the stomach failed to retain even iced-water. Everything imaginable had been tried, when I injected subcutaneously, a grain and a half of ergotin in eleven minims of distilled water. Not fifteen minutes had elapsed before plug and a second ovum were forcibly extruded and hemorrhage immediately subsided. About eight hours subsequently flooding again commenced, owing most likely to relaxation of the uterus, and perhaps retention of some portions of membrane. Again I injected the ergotin, this time two grains, with equal success toward effecting control of the hemorrhage. (Dr. F. E. Clarke, p. 203.)

INFLAMMATION AND CONGESTION OF THE VAGINA AND OS UTERI.—The use of nitrate of silver is a mistake, and moreover, better results can be obtained by other means. Of all the agents which are applied to the vagina for the relief of inflammation or congestion of the canal, glycerine, without doubt, is the most valuable. A small roll of cotton-wool will absorb five or six drachms of glycerine; you fasten to this a strong thread or piece of twine, introduce it through a speculum, and leave it in the vagina for twelve or even twenty-four hours, directing your patient to withdraw it at the expiration of that time by means of the string which is left hanging outside the vulva. Glycerine thus applied produces a copious watery discharge, which has a marked effect on the mucous surfaces in immediate contact with it. Thus, after its application the vagina and vaginal aspect of the cervix uteri appear pale, and the copious discharge seldom fails to relieve, for the time at least, that distressing sense of heat which is complained of in severe cases of vaginitis. In less acute cases the addition of ten grains of tannic acid to the ounce of glycerine often proves useful, but if used before the acute symptoms subside, it may cause increased irritation. Be sure whenever you use glycerine to warn your patient that she is to expect a copious discharge, otherwise the great flow which often comes on almost immediately will cause much alarm. (Dr. Lombe Atthill, p. 237.)

OVARIOTOMY.—No more favourable arrangement of circumstances can present itself than a solid ovarian tumour, freely movable, and surrounded by ascitic fluid; whilst on the other hand, a solid ovarian tumour, fixed and immovable, I invariably approach with the most serious consciousness of impending difficulty. It is easy, I think, to understand why this should be so. If a solid ovarian tumour be tied in position by adhesions, the probability is that such adhesions will be pelvic and posterior; they will be situated at the very points at which it will be most difficult to separate them, and, even supposing this successfully accomplished, the raw surfaces produced will occupy the very positions which are fraught with the greatest after-dangers to the patient. I always hail the presence of ascitic fluid in the peritoneum as of highly favourable augury; and even adhesions of lymph, so only that they be soft and easily broken down, generally impress me favourably rather than otherwise. In the case of an unilocular cyst, I have long held with Mr. Southam of Manchester, that it is advisable, in all cases, to give the patient the benefit of tapping once, at all events, and that for more reasons than one. To tap a manifestly multilocular cyst is, in my judgment, to run a very serious risk, and one which is not generally counter-balanced by the amount of relief likely to be obtained by the operation; and hence it becomes a matter of considerable moment to be able to form a probable diagnosis as between the two forms. I desire explicitly to record my feeling in favour of a full and free primary incision. As to the best method of dealing with adhesions: if they be recent and soft, they are easily broken down; and, following them carefully with the finger and the eye, to detect any vessel that may seem inclined to bleed, I generally so deal with such; but if the tumour be firmly adherent to the abdominal walls, or to any of the viscera, this cannot be done. If they be old and well organised, and pretty certain to live, I much prefer to cut out the portion of the cyst, and leave them attached, than to attempt to dissect them away. As to the best method of dealing with the pedicle, I am careful, above all things, if it be possible, to avoid having any drag on the stump; and when, having severed a tumour, and arrested all hemorrhage in the stump, I feel that I can safely drop it into the pelvis, and see it no more, I am the most easy about it. The two methods from which I have decidedly seen the happiest results have been "searing" and "torsion." The former is effected by seizing the pedicle near the uterus with a clamp, and then cutting off the tumour with the searing iron, and carefully burning away every atom of the eschar; the latter by simple twisting. The uterine portion of the pedicle, having been grasped by a clamp made specially for the purpose, is firmly held, and the end from which the tumour has been detached is enclosed in a second clamp, and the portion is then slowly and deliberately twisted off. In using either of these methods, I am still careful, when the clamps have been removed, to search carefully over the end of the stump, and if I see any bleeding point to tie it; and having done this, and assured myself that all is safe, I delight to feel that, henceforward, I have done with the pedicle. (Mr. C. G. Wheelhouse, p. 231.)

Immediately after making the incision through the walls of the abdomen the index finger should be passed up to the region of the umbilicus, and if it can be swept freely across from side to side it must be within the abdomen. This, of course, is an easy matter when no adhesions exist. It is always possible in parietal adhesions, when the finger is inside of the peritoneum. It is not possible, without the most unwarrantable violence, when the finger is between the layers of the abdominal parietes. The non-observance of this rule has led to the separation of large portions of the peritoneal layer of the walls of the abdomen, even when no adhesions existed, the operator having mistaken the peritoneum itself for an adherent cyst-wall. (Dr. Atlee, p. 235.)

PELVIC HÆMATOCELE.—In hæmatocele the blood is effused into the cavity of the peritoneum. The hæmatocele results either from the regurgitation of the menstrual fluid from the uterus along the Fallopian tube into the peritoneal cavity, or else the afflux of blood which usually takes place to the generative organs at the time of menstruation leads to such an engorgement of the venous plexuses that rupture occurs at some point where either a varicose or other diseased condition existed, which predisposed to the catastrophe. In a certain number of cases—but they are, in my experience, very rare—the hemorrhage occurs in connexion with pregnancy, or rather with delivery, either at term or, more commonly, prematurely, and especially during the earlier months. It will almost invariably be found that symptoms of ovarian or uterine disease have existed in these cases prior to the occurrence of the hæmatocele. The diagnosis has to be made from pelvic cellulitis and pelvic peritonitis. Pelvic hæmatocele is characterised by symptoms which are more sudden, severe, and alarming than either of the other two—it, in fact, shows its traumatic character very distinctly; and pain, prostration or collapse, with symptoms of internal hemorrhage, are its leading features. The pain is of course very local and limited, and it has a peculiar forcing, bearing-down character, a feeling which is described sometimes as if everything were being forced away. There are differences in the character of the swelling. In hæmatocele it is at first tense, elastic, fluid, and fluctuating; as time goes on, it becomes boggy, doughy, non-fluctuating then firmer and firmer, till it is almost hard, and with this change it contracts considerably, getting smaller and smaller, till it finally disappears. (Dr. Alfred Meadows, p. 242.)

POST-PARTUM HEMORRHAGE.—No one should go to a case without a catheter, secale, opium, ammonia, a good syringe, and (as far as my present experience has shown me its value) the perchloride (or a kindred preparation) of iron. If we have reason before the birth of the child to suspect that post-partum hemorrhage will occur, a good dose of secale should be given just before the head comes on the perineum. Should flooding occur, the uterus and vagina must be cleared of all clots, and firm pressure externally maintained. By attending to the actual condition of the uterus after the conclusion of the labour by means of the hand pressed upon it gently through the abdominal walls, we ought to be forewarned of hemorrhage and act accordingly. The next thing to try, supposing the hemorrhage continues, is cold externally, and if this fail, a jet of cold water carried up to the fundus by means of the syringe, or passing a piece of ice into the uterine cavity. If these means fail the case is a very desperate one, and only two measures of treatment remain to us. The first is, pressure upon the aorta and vena cava, by means of a firm pad pressed well down upon the vessels, the patient being of course upon her back. The second, and only remaining resource is injection of a styptic into the uterine cavity, such as perchloride of iron. In my own practice, I employ a rather weak solution at first—one of the weaker liquor ferri perchloridi to six or eight of water. If that fail, then I make it stronger; but, if the flooding be rapid, I use the full strength of the weaker solution of the tincture, because much of it is lost in the blood, or is prevented from coming easily into contact with the walls. The tube of the syringe should be carried to the fundus, and the fluid gently injected; or it may be conveyed with a sponge, which should be passed over all the internal surface. Perhaps this is the safest mode; but it must be removed promptly, or the uterus may retain it, as occurred in one case I know. I have tried it in all kinds of *post partum* hemorrhage, after removal of clots, which is important, or they become coagulated and firm, and cannot be removed afterwards without trouble. I have found it particularly satisfactory in cases of irregular and intermittent relaxation, causing the uterus to assume its proper form at once. (Dr. Braxton Hicks, p. 214.)

Cold Water Injections.—Having introduced the hand to ascertain that there is no retained fragment of placenta, grasp the fundus as firmly as possible, and at the same time inject cold water freely into the uterus. For this purpose the long tube of an ordinary stomach pump, or enema apparatus, is to be employed and passed well up into the uterine cavity. (Dr. W. Boyd Mushet, p. 211.)

Perchloride of Iron.—We have three stages of hemorrhage to deal with. 1. There is hemorrhage with active contractility of the uterus. Here the diastaltic function may be relied upon; excitants of contraction find their application. 2. There is the stage beyond the first, when contractility is seriously impaired, or even lost. Here excitants of contraction are useless; our reliance must be upon the direct application of styptics to the bleeding surface. 3. There is the stage beyond the first two, where no remedy holds out a hope unless it be transfusion; and even this will probably be too late. The practical directions are as follows:—Take a Higginson's syringe, to which is connected a uterine tube nine or ten inches long. Mix in a deep basin four ounces of the strong liquor ferri perchloridi of the British Pharmacopœia with twelve ounces of water; pump through the delivery-tube two or three times to expel the air; then pass the delivery-tube into the uterus, so that its end touch the fundus of the uterus; then pump gently and slowly; the styptic fluid will thus bathe the whole inner surface of the uterus. (Dr. R. Barnes, p. 211.)

I always carry the perchloride of iron with me, and, if any excessive loss occurs, use it as directed by Dr. Barnes in his work on "Obstetric Operations." I believe the satisfactory results which I have met with are due to my using the solution promptly, and in not waiting till the patients were in a state of collapse. For the purpose of injecting it, I use a common syphon syringe, to which an ordinary vaginal flexible nozzle is attached. This should be carried fairly up to the fundus of the uterus. I generally find that four or five ounces of the solution are sufficient. As to the strength, Dr. Barnes recommends that four ounces of the strong liquor ferri perchloridi be added to twelve of cold water. I generally use it stronger—about two parts of water to one of the liquor. One precaution only is needed, namely, to take care that the tube is passed well into the uterus before any of the fluid is injected; otherwise the vagina will be corrugated and the os uteri contracted from the effects of the iron, the styptic will not reach the interior of the uterus, and great difficulty will be experienced in any subsequent efforts to introduce the tube. The same reason, namely, the effect produced by the action of the styptic on the vagina and os uteri, renders the application of the iron by means of a sponge difficult and unsatisfactory. The great injury to which steel instruments are liable, when kept in a bag which also contains a solution of the perchloride of iron, has induced me to carry the salt in the solid form, the bottle containing it being enclosed in a boxwood case. (Dr. Lombe Atthill, p. 213.)

Danger from Injections of Perchloride of Iron.—No reasonable objection can be urged to the employment of these strong styptics, provided the one essential condition be attained—the complete and permanent contraction of the gravid uterus. For it appears a matter of little consequence by what means the contraction is produced; so long as it be complete and permanent, as to close the arteries, and obliterate the canals of the veins or sinuses, and prevent any injurious absorption, the woman is safe, and will make a good recovery. But the danger in the use of these agents, as shown by the fatal cases which have followed the employment of them, appears to consist in only partial contraction being produced, by which although the hemorrhage was arrested, yet the veins or sinuses remained open, and permitted the perchloride of iron to be taken up and conveyed into the general system. The consequence of this, as shown by the fatal cases, has been that symptoms of blood-poisoning supervened in the course of a few days, ending in the almost inevitable death of the individual. It seems desirable to have some agent always at hand, and incapable of producing such results. Ordinary vinegar in the proportion of one part to two or three parts of water has been tried in some six or eight cases with very encouraging results. In some of the cases, however, in which styptics, such as perchloride of iron, have been injected, there was not the least occasion for it, seeing that firm pressure externally, with or without the introduction of the hand into the uterus, had not even been tried. (Dr. T. Snow Beck, p. 218.)

After the completion of a natural labour, the uterus becomes sufficiently relaxed to allow the blood to escape from the open ends of the torn arteries. The means which are adopted to induce the contractile tissue to again contract and close the canals of these vessels, either fail to provoke contraction or only induce partial contraction; and in either case the hemorrhage continues, when a solution of the perchloride of iron, of varying strength, is injected into the cavity of the uterus. After the injection of the perchloride, the hemorrhage ceases at once or very soon afterwards; the patient revives; and all things appear to be going on favourably till about the third day, when the usual series of fatal symptoms commence by the woman complaining of feeling very ill. The symptoms, once begun, continue gradually to increase in gravity, and at the end of two, three, or four weeks, the patient appears to fall asleep as death closes the scene. The whole mischief in these cases of *post partum* hemorrhage appears to turn upon the single point of the want of proper contraction in the gravid uterus; and the sole question in the treatment upon the best means of inducing and maintaining complete contraction. Want of contraction, or subsequent relaxation, which practically is the same thing, allows the blood to escape from the arteries; want of proper contraction permits the canals of the sinuses to remain pervious, the noxious fluids to enter them, and death to result. When complete and permanent contraction is induced, the woman appears to be safe from all ill consequences, and will make a good recovery. But it is also known that even the use of the perchloride is sometimes not followed by any perceptible contraction; or by only partial contraction, by which the flow of blood is arrested, and the way to injurious impregnation of the general system left open. (Dr. T. Snow Beck, p. 223.)

SPINA BIFIDA.—*Injection of Iodine.*—Dr. Watt relates a case of spina bifida cured by injection of Dr. James Morton's solution of iodine and iodide of potassium in glycerine. Two tentative tappings with a medium cannula, of respectively twelve and ten ounces of clear fluid, were made, with an interval of eight days, both being followed by an irritable and feverish condition of the child, and gradual refilling of the tumour to its original size. Ten days afterwards, other ten fluid-ounces were removed, and half a drachm of the solution injected. The opening was instantly closed on each occasion by a layer of lint soaked with flexile collodion. Ten days afterwards eight fluid-ounces were removed, and a similar quantity of the solution injected. This was followed by severe irritative symptoms, but a fortnight later the tumour was only one-third of its original size, and a repetition of the treatment at that time resulted in a cessation of any further formation of fluid—a roughened, darkened, thickened condition of skin replacing the tumour, and closing the spinal aperture with an apparently gelatinous mass. (Dr. J. Ross Watt, Brit. Med. Journal, Jan. 31, p. 137.)

SPONGE TENTS.—Sponge tents are far safer than those made of sea-tangle, and it has long been one of my objects to remove the very offensive smell after their use. I have tried charging them with various disinfectants, but without any result, until last week I made an experiment with oil of cloves, and I have found that a tent charged with 5 per cent. solution of oil of cloves, will remain in the uterus for twenty-four hours without becoming offensive in the slightest degree. There can be no doubt that such a tent is far safer than those ordinarily in use, and it is certainly much more agreeable to the operator. Messrs. Krohne and Sesemann, of Duke-street, W., make these tents according to my formula. (Mr. Lawson Tait, p. 246.)

TRANSFUSION OF BLOOD.—If a sufficient amount of blood is not procurable, a solution of phosphate of soda, carbonate of soda, and chloride of sodium, at a temperature of 100° F., should be injected first, as one factor in the danger attending these cases of prostration is the nearly dynamic one arising from the absence from the heart of fluid to act upon. The blood which is procurable may be injected afterwards. Blood may be used either whole or defibrinated. The choice will be governed by circumstances. If we have Aveling's or other proper apparatus to practise transfusion from arm to arm, so that the blood passes direct from giver to receiver, without ever being exposed to the air or allowed to rest, this will often be the preferable plan; but where the proper apparatus is wanting, as it is likely to be, defibrination is better. So long as it is considered necessary to have a special apparatus, the operation must be performed rarely. But let it be understood that blood either whole or defibrinated will do, and that almost any syringe can be made to answer, and the great obstacle to the extension of the operation will have vanished. Nothing can be more easy than to furnish every stomach-pump case with a flexible tube to which a transfusion-canula is attached. As no practitioner of medicine ought to be without a stomach-pump, the *necessary* apparatus will never be wanting. I should not hesitate to use an ordinary Higginson's syringe. (Dr. R. Barnes, p. 227.)

VAGINAL INJECTIONS.—The solutions of alum and of sulphate of zinc so frequently used often aggravate the case when vaginitis is present. They coagulate the albumen of the leucorrhœal discharges, producing a number of hard masses, which often cause much discomfort. Borax is a better agent, but it sometimes causes irritation, though in a less degree. A drachm of borax to the ounce of water is the proper strength. Where the object is to soothe and to allay irritation this is best effected by the use of an infusion of tobacco. Tobacco must, however, be used with caution. Some patients are peculiarly susceptible of its action, especially those in whom the orifice of the vagina being narrow some of the fluid is retained in the canal. Begin, therefore, by infusing fifteen grains of the unmanufactured leaf in a pint of boiling water. If this produces no unpleasant effect increase the strength to thirty—even sixty, grains to the pint. In many cases the addition of a drachm of borax to each pint of the infusion greatly increases the efficacy of the treatment. Many patients, however, are unable to use the tobacco at all, as even a very weak infusion causes nausea and faintness. I therefore recommend, when this is the case, or where you fear to run the risk of causing any discomfort, to substitute for tobacco an infusion of hops, directing an ounce of hops to be infused in a pint of boiling water with or without the addition of borax, as you may deem advisable. (Dr. Lombe Atthill, p. 237.)

VASCULAR GROWTHS OF THE FEMALE URETHRA.—*Application of Chromic Acid.*—Concentrated chromic acid has the two great merits of very efficaciously destroying the growth, and at the same time being much less painful than potassa fusa or nitric acid. The use of the actual cautery is out of the question unless chloroform is given. My usual plan is to roll a little cotton wool round the end of a small piece of wood (an ordinary match will do); to soak it for a quarter of an inch in the acid solution; and then to carefully yet firmly press it upon the vascular growth until the surface becomes distinctly shrivelled; the surrounding parts being guarded by cotton wool soaked in solution of carbonate of soda, which is also applied to the growth subsequently, to neutralise any excess of the acid. Slight burning pain is produced, but seldom sufficient to need any employment of morphia, either hypodermically or as a local application. (Dr. A. W. Edis, p. 229.)

MISCELLANEA.

BROMIDE OF POTASSIUM.—Dr. Reynolds speaks of KBr as if it were the only remedy worth consideration in the direct treatment of simple *epilepsy.* He says: "It is to be demonstrated, in my opinion, that there is something 'specific' in the action of KBr. Potassium—given as iodide—is without such effect; and bromine—given as bromide of ammonium—has no obvious influence on epilepsy; but in combination these two elements—bromine and potassium—are of undoubted value." And he sums up the general question as to epilepsy thus:—"(1) The cure of epilepsy is effected by doses varying, for the adult, from 5 to 40 gr., given three times daily. (2) It is not the mere administration of the drug, but its presence in certain quantity, that is necessary for a cure. I would earnestly recommend that bromide of potassium should not be discontinued in the treatment of a case of epilepsy because of its apparent failure; but that the dose should be gradually increased, and the exhibition of the drug most patiently carried on for a period of many months or even years." Elsewhere Dr. Reynolds remarks that, while thus remarkably efficacious in *epilepsia gravior*, the bromide scarcely at all affects the attacks of *petit mal*, and is also less efficacious than usual where the convulsive attack only occurs at night. Cases of "cure" in the ordinary acceptation of the term, *i.e.*, in which well developed fits had been stopped completely, and had remained absent long after the discontinuance of the medicine, are, however, perhaps never seen. On the other hand there are *numerous* instances in which very long periods of freedom have been obtained. In spasmodic diseases like *false croup* and *asthma*, we have the very high authority of Dr. W. Begbie for the statement that bromide of potassium is a powerful remedy; and this is backed by the testimony of Ringer, who also adds that it is most efficacious in the colic of children, and also in the night terrors of young children. The latter writer expresses the following opinion as to its effect in convulsive diseases:—"Although convulsions may be excited by many causes, it is probable that the conditions of the nervous centres producing the attack are in every instance identical; and it appears to be these conditions which the bromide controls." (Dr. Anstie, Practitioner, Jan. 7, p. 19.)

COCA.—*Its Therapeutic Use.*—It is possible that this drug may turn out to be of considerable value in cases of exhaustion. The leaves are derived from the *Erythroxylon coca*, a shrub that is indigenous in South America. They are chewed by the Indians, who are stated by Mantegazza, Moreno, Guibourt, and others, to go through laborious work for several days and nights in succession, without exhaustion, although they have little or no food, provided that they have a supply of about eight ounces of coca-leaves to chew during the day. An infusion of half a drachm to a cup of hot water is said to produce beneficial effects. (Dr. E. H. Sieveking, p. 248.)

CROTON-CHLORAL.—Croton chloral, unlike the hydrate, dissolves with difficulty in water, and crystallizes in small glittering tablets. Its action differs widely from that of chloral hydrate in some important particulars. Four *grammes*, or a drachm, of this substance, dissolved in water, and introduced into the stomach, produce in the course of from fifteen to twenty minutes a deep sleep, accompanied by anæsthesis of the head. Whilst the eyeball has lost its irritability, and the nervus trigeminus shows no reaction whatever on being irritated, the tone of the muscles remains unaltered. In some cases of tic douloureux, the remarkable phenomenon is exhibited that pain ceases before sleep sets in. I am sorry to say, however, that this remedy acts only as a palliative in this dreadful disease. I nevertheless prefer its action to that of morphia, because it has effects as good as the latter remedy, without being so detrimental to the constitution in general. I have never observed any unfavourable effects of croton-chloral on the stomach or any other organ, although I have made frequent experiments with it. The indications for the use of this remedy are to be found—1. In cases where hydrate of chloral is inapplicable on account of heart disease; 2. In cases of neuralgia in the district of the nervus trigeminus; 3. In cases where very large doses of chloral are necessary to produce sleep. I there recommend the addition of croton-chloral to hydrate of chloral. (Dr. Oscar Liebreich, p. 247.)

Croton chloral is a remedy of remarkable efficacy in some cases of neuralgia of the branches of the nervus trigeminus. As an example, a patient had had no rest at night for three weeks from severe neuralgic pains, arising from decayed teeth. Two doses of five grains each given within three quarters of an hour of one another completely relieved the pain, without producing any drowsiness. The patient had two doses given him to take, one on each of the following nights, and five days afterwards he reported he had had no return of the pain. The remedy has also the power of affording relief in other obstinate forms of neuralgia and in certain cases of diffused muscular pains; but it has little appreciable effect in purely rheumatic cases, and none in hysterical. Of its value in relieving some forms of irritative and spasmodic cough I have had abundant evidence. If I may trust to the short experience I have hitherto had of it, I should say there is scarcely any remedy that is likely to prove more valuable for the relief of the distressing night cough of chronic phthisis. It should be given in moderate and quickly repeated doses until the amount of tolerance of the drug in each particular case has been discovered. In severe neuralgias from two to five grains may be given every hour, or the smaller dose every half-hour, until fifteen grains have been taken. At present Dr. Burney Yeo does not consider it safe to go beyond this dose. The following formula yields the strongest solution that is permanent; stronger solutions throw down a crystalline deposit on cooling:—Croton chloral hydrate, sixty-four grains; glycerine, half an ounce; warm distilled water, an ounce and a half. This gives a solution containing four grains of the drug in each fluid drachm, and having a slightly bitter and camphoraceous taste. I have also given it in pills mixed with an equal part of the confection of roses, and if these are kept in a bottle they form a very good vehicle for its administration. (Dr. Burney Yeo, Lancet, Jan. 31, p. 159.)

Modern Anæsthetics.—Ether will never take the place of chloroform, the latter being so much more convenient and pleasant, both for the patient and operator, and there are many cases in which it is quite unobjectionable. Whatever anæsthetic we may employ, ether is a *most valuable auxiliary*, and one we ought at once to resort to if it is necessary to keep up the anæsthesia and there is an appearance of collapse or failure of the heart's action. In my own cases ether is always at hand, and should the patient get faint under chloroform or other anæsthetic, I always substitute the ether-mask with the invariable result of restoring the pulse; in fact, the administration of ether at the commencement of an operation, though we may go on with a more convenient anæsthetic afterwards, has a similar effect to a preliminary dose of brandy, and it may be alternated in this way just as we would give a stimulant several times during a prolonged operation, with the best effects; this appears to me to be the true place of ether as an anæsthetic. Bichloride of methylene is not quite so convenient as chloroform on account of the inhaler employed, but it is far more rapid in its effect, and when there are a great number of patients to be operated upon, and time is of importance, this is the anæsthetic which will always be preferred. The bichloride is best administered with an inhaler that almost excludes the air, though a little may be admitted at the commencement; two or three drachms should be poured on, and the agent be pushed when the patient shows signs of going off; when fully off, remove the inhaler, and do not give another inspiration unless the patient shows signs of returning sensibility. Patients succumb very quickly to the bichloride, and recover as quickly; hence, it is a most convenient anæsthetic, and perhaps safer than chloroform; it is, however, like chloroform, a lethal agent, and requires careful watching. Attempts have been made to combine it with ether, and the result has been the discovery of a definite compound called ether methylene, which I frequently employ, and which is a very convenient and very safe anæsthetic administered in the same way as the bichloride. I have given a fair trial to nitrous oxide gas, and like all other surgeons that I know, have abandoned it. With regard to relative safety, I should place ether very decidedly first; next to that ether methylene with ether; next, ether methylene alone; and lastly, I would bracket chloroform and bichloride of methylene together, as at once the most convenient but most risky of anæsthetics. (Dr. C. Bell Taylor, Medical Press and Circular, Jan. 28, p. 67.)

A very extensive experience has shown that etherisation pushed to the very last stage of insensibility is never dangerous to life so long as one maintains the act of respiration. Chloroform acts more quickly, and is more agreeable to the patient, but it has a paralysing action much greater than that of ether, and has a special influence on the nerves of the heart. Death sometimes occurs during anæsthesia from chloroform, very suddenly. Professor Schiff considers that chloroform should be banished from practice as an anæsthetic agent, except in cases in which extraordinary resistance to the effect of ether shows itself, in which instances it might be allowedto mix a little chloroform with it in order to produce the commencement of anæsthesia, which should afterwards be continued with pure ether. (Dr. T. G. Hake, Practitioner, April, p. 241.)

Raising the chin, and pulling it as far as possible away from the sternum, generally prevents the movements of deglutition, which are excited by the administration of chloroform, and which prevent its free inhalation. It will be found on trial that swallowing cannot be accomplished with the chin considerably elevated. Spasmodic closure of the glottis may occur, when chloroform or ether vapour of too great pungency is given. Should this occur, the chin should be well raised, and, if necessary, the tongue drawn forwards by forceps, which are to be kept ready for that purpose. Ether is undoubtedly safer than chloroform, but it is far from being really safe. Most English surgeons are in favour of giving the ether slowly for about a minute or so at first; and then, when some degree of anæsthesia has come on, to give the vapour strongly. I am decidedly in favour of this plan, and have sometimes succeeded in getting a patient to sleep without any struggling. The most pleasant way is to give nitrous oxide till consciousness is lost, and then administer the ether of moderate strength. Next to nitrous oxide, I think chloroform is the least unpleasant anæsthetic to breathe. Patients who have previously taken chloroform are often unwilling to try nitrous oxide, because they have no objection at all to the former; but I have rarely found a patient who had once been put fully under the influence of ether, who was willing to inhale it again, unless it had been preceded by nitrous oxide. I admit that chloroform has more deaths to account for than ether has; but we must not forget that chloroform is much more frequently administered than ether, and that coroners' inquests are not held in countries where the practice of giving ether instead of chloroform is prevalent. Still there are greater dangers from chloroform than from ether, although I do not think them so insidious that every one who inhales it runs an infinitesimal chance of losing his life. In whatever way the chloroform is given, the condition of the patient's pulse, as well as his breathing, should be carefully watched all the time he is inhaling; and, when the breathing is deep or the pulse weak, the chloroform should be diminished or removed. In the administration of anæsthetics generally, but especially in giving chloroform, it is advantageous to have the stomach empty or nearly so. I like to give a teaspoonful of brandy, without water, a few minutes beforehand, but not so much as a tablespoonful. If wine be given, or if the patient must have some water with the brandy, then they should be given half-an-hour before inhaling, to allow time for their absorption. A patient should have his pupils insensible to light, or breathe stertorously, before the commencement of a severe operation; but one or two inspirations of pure air should be taken before the anticipated shock is given. I now very rarely give bichloride of methylene from choice, having less confidence in it than chloroform. In one case the respiration and pulse ceased, and artificial respiration had to be resorted to. I have used a mixture of alcohol, one part; chloroform, two parts; ether, three parts; and like it much, for cases where moderate narcotism has to be kept up a long time—*e.g.*, when caustic chloride of zinc has been applied to a cancerous growth. I have tried, in a few cases, tetra-chloride of carbon, ethyliden chloride, and most of the other anæsthetics that have been recommended; and the conclusion at which I have arrived is, that they are neither safer nor more pleasant than ether and chloroform. Nitrous oxide causes sleep without any delirium or excitement. It is by far the best anæsthetic for many short operations, such as the extraction of teeth, opening abscesses or boils. It answers very well in operating for strabismus. Removal of the eyeball was performed lately by Mr. Bowman for a lady, who said she had no consciousnesss of the operation. It is well suited for examining hysterical cases, wrenching stiff joints, and reducing luxations of recent date. I do not think it suitable for cases where it is necessary to keep a patient quiet more than three or four minutes; but, if the patient be allowed to recover consciousness after one inhalation before another is commenced, the anæsthesia may be kept up tolerably well for half an hour. In administering nitrous oxide, a plentiful supply of gas is essential to success. There is no fear of patients inhaling too much at first; indeed, they should be told to breathe deeply and slowly. Nitrous oxide may be used with advantage to prepare the way for giving ether; but I would not advise it before giving chloroform, because, firstly, chloroform is not very unpleasant if given gradually, and, secondly, it would be more dangerous to give cholorofom in the state of excitement from gas than without it. (Mr. Joseph T. Clover, Brit. Med. Jour., Feb. 14, 1874, p. 201.)

A New Expedient in Administering Chloroform.—Herr Heiberg, of Christiania, has hit upon an expedient in administering chloroform, which in his opinion is available for the avoidance of danger from imperfect respiration with rattling and lividity, which is liable to occur during the administration of chloroform. This consists in drawing forward the under jaw *in toto.* When the rattling, incomplete respiration begins—that is to say, in all those cases in which the teeth are otherwise forced apart, and the tongue drawn out—I draw the under jaw forwards by the following means:—Standing preferably behind the reclining patient, the operator places both thumbs on the symphysis of the lower jaw, presses the second joint of the bent forefingers behind the posterior margin of the rami ascendentes of the under-jaw, and thus holding the whole bone fast between the two hands, draws it forcibly forward (anatomically speaking). The most successful impulse is that which would be given if the intention were to lift the whole head and body by this grasp. By this proceeding and as long as it is continued the jaw is kept luxated forward. The obstacle to the respiration is removed, and, in short, exactly the same result is obtained as if the tongue had been drawn forwards and the mouth kept open by a gag. (Herr Heiberg, Medical Times and Gazette, Jan. 10, 1874, p. 36.)

RICHARDSON'S TOOTH-EDGE CUTTING SCISSORS.—These scissors are intended for the removal of tumours, and for general use in any operation where it is desirable to avoid much hemorrhage. They are particularly suitable for cases of epulis. They are of the ordinary construction in all respects except in the cutting edge. The cutting edge of each blade, instead of being even and sharp, is divided into finely-pointed teeth, each tooth being directed with a slight inclination towards the handle of the scissors. When the blades meet the teeth cross each other, and as they pierce any structure that may lie between them, they crush also, between their surfaces. If a piece of moderately firm substance be placed between the blades—a piece of paper or of thin card, for example—the scissors perforate it in a series of perforations resembling what is seen in the postage stamp—that is to say, they do not cut clean through the substance so as to leave it in two distinct parts at once. A little lateral or half-rotating movement of the closed blades is, however, sufficient to tear through the still connected lines of substance and to complete the separation. The same occurs if the substance placed between the blades be a portion of soft animal structure, only that more force is required in the lateral or rotating movement to cause complete separation. The parts punctured are crushed between the teeth, and are separated by the twist or torsion. I find these scissors useful in dividing directly and quickly structures in which there are many minute blood-vessels, and which, when divided by the knife, bleed freely. These toothed scissors, as they can be made at one and the same time to pierce, crush, and twist, control bleeding remarkably. (Dr. B. W. Richardson, p. 248.)

SULPHIDES.—*The Therapeutic Use of.*—The sulphides of potassium, sodium, and calcium are therapeutic agents of value in cases of suppuration. When they are administered, a thin, watery, unhealthy discharge becomes at first more abundant, afterward diminishing, and becomes throughout thicker and healthier. The condition of the sore improves correspondingly, and its healing is promoted. The sulphides appear often to arrest suppuration. Thus in inflammation threatening to end in suppuration they reduce the inflammation, and avert the formation of pus. This effect is manifested when sulphur compounds are employed locally in acne indurata. The advantage to be derived from these agents is especially seen in cases of slow suppuration in the glands of the neck in children, such as follows scarlet fever. In some of these cases the system is worn out by the pain and constitutional disturbance. If a tenth of a grain of sulphide of calcium, mixed with a grain of sugar of milk, is given in such a case every hour, or two hours, the results are most striking. The swelling becomes smaller, the pus reaches the surface in four or five days, and when it is evacuated leaves a benign wound which quickly heals. The effects of these remedies are equally conspicuous in mammary abscesses, although in rare instances they appear temporarily to increase the pain— a remark which seems sometimes to hold good with respect to boils. In boils and carbuncles these remedies yield excellent results. A tenth of a grain of sulphide of calcium, given every two or three hours, generally prevents the formation of fresh boils, while it lessens the inflammation and reduces the area of the existing boils, and quickly liquefies the core, so that its separation is much more speedy, thus considerably curtailing the course of the boil. Where the skin is not yet broken, and the slow-separating core therefore not yet exposed, the sulphides often convert the boil into an abscess, so that on bursting pus is freely discharged, and the wound at once heals. The topical effect of sulphur ointment, or of an ointment of the hypochlorite of sulphur, or, still better, of the iodide of sulphur of the Pharmacopœia, is most marked in acne indurata and acne rosacea. Here, again, the effects are twofold, and even opposite, according to the stage of the eruption. If applied at the very commencement of the eruption, as soon as the little hard knot is felt under the skin, further development is arrested, and the hardness speedily disappears. If rubbed over the skin it appears to check the formation of the acne spots. If rubbed over the nose and neighbouring parts of the face, in acne rosacea its effects are often striking. Not only does it act as in acne indurata, but the hardened, swollen tissues become softened and reduced to a more natural state. (Dr. S. Ringer, Lancet, Feb. 21, 1874, p. 264.)

THERMOMETER.—*Patent Self-Registering.*—Mr. Casella, of Holborn-bars, has succeeded in constructing a most useful modification of the clinical thermometer, the idea being to prevent the oft-recurring difficulty in clinical thermometers which renders them practically useless—viz., the escape of the index into the bulb, where, mixing with the bulk of the mercury, it is irrecoverably lost. The instrument is constructed in precisely the same way as the ordinary clinical thermometer, with this important exception—viz., that the capillary tube issuing from the bulb is gradually contracted for a distance of half an inch, and then suddenly expands into an oval chamber or trap, beyond which the tube is continued of the usual diameter to the extremity of the instrument. The thermometer supplied to us, which we have had in use and found most efficient, has, whether by accident or intent, a contraction in the tube both above and below the chamber or trap. This, without being a disadvantage, provides a still further security against the destruction of the index, for it is almost impossible to shake it even into the trap or what may be called a secondary bulb, much less into the all-engulfing bulb. Mr. Casella guarantees this instrument from becoming disarranged by any amount of violence, whether in the hands of the physician or the most inexperienced person. We can strongly recommend this thermometer as superior to any we have seen or used. (Medical Times and Gazette, Feb. 28, p. 252.)

TOBACCO SMOKE.—*Effects of.*—M. Gustave Lebon, of Paris, has recently issued a treatise upon the Chemical and Pathological Effects of Tobacco Smoke upon the Human System, the author having first constructed a variety of apparatus by which he has been enabled, he states, to collect and show with exactness the various elements of smoke which condense and deposit themselves on the organs of the smoker. One hundred grammes—1,300 grs., English troy—of French tobacco furnish 0.550 grs. of nicotine and 0.490 grs. of ammonia. Tobaccos of the Levant are the least dangerous, some of them possessing but a small trace of nicotine. In small doses, he says, tobacco smoke instantaneously excites cerebral activity and the intellectual forces, and facilitates digestion. In excessive and frequently repeated doses, it produces difficulty of digestion, benumbed intelligence, and clouded memory. (M. Lebon, Medical Press and Circular, Dec. 24, p. 575.)

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From the Journal of Applied Chemistry, January, 1874.

SOAP FOR TOILET AND MEDICINAL USES.

By Prof. W. A. Wetherbee, M.D.

It has been said by an eminent chemist that "the *quantity* of soap consumed by a nation would be no inaccurate measure whereby to estimate its wealth and civilization." With equal accuracy it might be added that the chemical and medical intelligence of that nation has much to do in determining the *quality* of those soaps, especially of those employed for toilet purposes.

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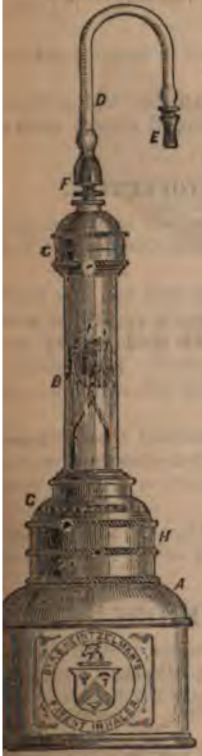

FIGURE 1.

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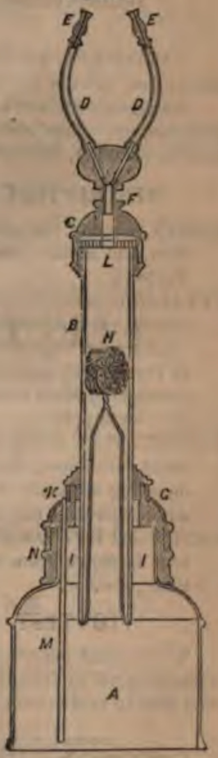
D. Flexible Tube.
E. Mouth Piece.
F. Attachment of Tube.
G. Perforated Ring Valve.
H. Metallic Coupling.
I. Sponge within the Cylinder.

FIGURE 2.

A. Glass Receiver.
B. Glass Cylinder.
C. Metallic Cap.
D. Flexible Tubes.
E. Nasal Bulbs.
F. Attachment of Tubes.
G. Perforated Ring Valve.
H. Sponge within the Cylinder.
I. Space between Cylinder and Receiver.
K. Perforation for Air.
L. Perforated Diaphragm.
M. Air Tube.
N. Metallic Coupling.

Figure 2.



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